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## Do Democracies Have Different Public Policies than Nondemocracies?

Casey B. Mulligan, Ricard Gil and  
Xavier Sala-i-Martin

**H**ow does the source of political leadership affect public policy? Is the public sector run differently when its leaders are elected “fairly”? How, exactly? These questions are important. Nondemocracies are prevalent around the world. According to data from the POLITY IV (2000) project, which provides a widely used dataset on the authority characteristics of modern polities over the last two centuries, nondemocratic regimes ruled the majority of countries, and the majority of the world’s population, until 1991. Even since 1991, more than 40 percent of countries and people were ruled by nondemocratic regimes. Political economy theory devoted to the modeling of democratic institutions might therefore miss much of the world’s public sector activity.

A comparison of democracies and nondemocracies may also enhance the understanding of democratic institutions by providing an empirical test of some important implications of formal voting models. We begin this paper by contrasting two schools of thought about determinants of policy: one that emphasizes the role of voting mechanisms in determining policy and a second that argues that democracy and other political mechanisms will be (at most) second-order determinants of policy choices once economic and demographic variables are taken into account. This raises the empirical question: are there many public policies that are significantly different between economically similar democracies and nondemocracies and, if so, which types of policies? Looking across countries and over the years 1960–1990, we answer negatively when it comes to the economic or social policies. However, we do find differences in another policy arena, namely those relating to

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the process of winning and maintaining public office including torture, execution, censorship, military spending and regulating religion. One interpretation of our results is that democratic institutions have important effects on the degree of competition for public office, but otherwise have effects on public policies that are insignificant, or incidental to the struggle for political leadership.

## **Democracy and the Public Sector: Competing Economic Perspectives**

At first glance, it may appear that formal voting models have nothing to say about policy differences between democracies and nondemocracies, because they often make no explicit reference to dictators. But even with few direct references to nondemocracy, the existing formal voting literature is already based on three tenets of democratic decision-making that distinguish it from many conceivable models of nondemocracy and, hence, imply policy gaps between democracies and nondemocracies. First, many formal voting models have a process that mutes the expression of intense policy preferences that leads (in the models) to inefficient policy outcomes. For example, in democratic models, the middle-aged and old can benefit themselves by forcing the young to participate in an unfunded Social Security program even when the aggregate costs to the young far exceed the benefits to the old and middle-aged.

A second tenet of many formal voting theories is that the distribution of political power is more equal than, say, the distribution of income or wealth, because public decisions are made (directly or indirectly) by voting. This is an important reason why Meltzer and Richard's (1981) model confirms de Tocqueville's (1835) fear that democracies would excessively redistribute from rich to poor. Acemoglu and Robinson (2003, p. 2) are explicit about this theoretical result: "[D]emocratic regimes generally choose policies that are more favorable to the poor than nondemocratic regimes." By the same logic, the skewness of the distribution of taxable income can be an important determinant of income redistribution in democracies, because it measures the amount that the middle class can gain by forming a voting coalition with the poor (see also Alesina and Rodrik, 1994; Tabellini, 1992). Hence, there should be an interaction between democracy, income inequality and other variables determining public policy (Boix, 2003, p. 176). Olson (1993), Olson and McGuire (1996) and Niskanen (1997) have an alternative theoretical approach, still based on this second tenet. They predict that broad-based taxes will be levied at lower rates in democracies, where each taxpayer has a say in public decisions, than in nondemocracies where (in theory) the political leadership and its sponsors are a small minority of taxpayers. Hence, in their models, nondemocracies tax more, even while they are spending less on the poor.

A third tenet of formal voting theory is that the "form of the game is important" and not just a reaction to efficiency considerations. As explained by Myerson (1995), an important goal of game theoretic models of public choice is to predict how different political institutions would result in *different* policies. For example, it

should matter for policy whether a nation is divided into electoral districts, with each district electing one policy-making representative, or whether the whole nation votes on the same list of policymakers with the top vote getters making policy. From the perspective of game theory, the public decision-making mechanism could hardly be more different in democracies versus nondemocracies. If policy instrument  $p$  were found (empirically) to be the same function of economic and demographic variables in democracies and nondemocracies, then it seems that the best positive theory of why policy  $p$  is adopted would not rely on the three tenets, but instead on underlying economic and demographic variables. In other words, much of the voting literature implies that particular measures of democracy, perhaps interacted with other variables, should help predict public policy, holding constant economic and demographic variables.

At the other extreme from formal voting theory are positive theories of public policy such as Barro (1979) or Wittman (1989) where efficiency is the main determinant of public policy, while political factors—such as the mechanism by which public decisions are made—are presumed either to be unimportant or just reactions to efficiency considerations as various agents use the political sector to facilitate bargaining with each other (for a model along these lines, see Weingast and Marshall, 1988). Related are the “Chicago political-economic school” approaches of Stigler (1971), Peltzman (1976, 1980), Becker (1983) and others who admit that public policies may create inefficiencies such as monopoly rights or excessive taxation, but nonetheless deemphasize specific political institutions as determinants of public policy.<sup>1</sup> Instead, they emphasize economic and demographic variables such as interest group size, group cohesion, urban location and the technology of tax collection as determinants of public policies via their effects on both the public interest and on the political success of special interests. Conflicts over policy must be mediated by any political leader (whether democratic or not), and in this view, economic and demographic variables may determine the outcome.

Some studies modify formal voting theory by including logrolling (for example, Tullock, 1959; Coleman, 1966), endogenous voter turnout (Ledyard, 1984), campaign spending (Grossman and Helpman, 1996) and other factors in order to relax or to deviate from some of the three tenets (and may go so far as to conclude that only efficient policies have electoral success!). Following Machiavelli (1515, Chapter XIX), the concept of dictatorship can also be taken a step closer to democracy by assuming that dictators should be and probably are sensitive to popular support. An emphasis on popular support gives the impression that the Chicago approach is essentially similar to the formal voting literature, but this relation is only metaphorical,<sup>2</sup> because the formal voting literature assumes that

<sup>1</sup> Perhaps Stigler’s (1970) analysis of the political success of the middle class (“Director’s Law”) is an exception, but even this article gives as much attention to the tax collection technology as to the voting process.

<sup>2</sup> Peltzman (1980, p. 221) explains “majority voting considerations . . . should not be interpreted literally . . . what is essential here is simply that popular support contributes to the viability of public policies. . . .”

democratic leaders are sensitive to popular support in different ways than are nondemocratic leaders. The real question is not whether voting or popular support is part of the public decision-making process, but whether that process adheres to the three tenets (as assumed by even the most modern formal voting models) or, as claimed by the Chicago school, voting is a small and relatively insignificant part of a complex network of bribes, political advertising, popular perceptions of the public interest, logrolling, threats of strikes and other mass interruptions of private affairs, political participation, and compliance with the law that connects public policies with “popular political support.”

The next section of our paper therefore compares economic and social policies observed across countries and over the years 1960–1990 to obtain an estimate of the effects of democracy on the public sector. A broad range of policies is measured in a way that includes a broad cross-section of countries, and at the same time includes items from the tax, subsidy and regulatory arenas. We also include several policy measures, such as programs and tax provisions that might redistribute from rich to poor, education spending and conscription, where previous authors had suggested that differences should arise between democracies and nondemocracies.

By distinguishing economic and social policies from policies like torture, execution and censorship that limit political competition, the next empirical part of our paper reflects an influence of a “barriers to entry” approach originating with Becker (1958), Tullock (1987), Wintrobe (1990) and, pursued more recently, by Volckart (2000) and Tsui (2002). The barriers to entry approach emphasizes that suppression of political competition is an especially important activity for dictators, because it helps them retain their position.<sup>3</sup> In other words, this approach embeds the Chicago school in a model of the industrial organization of the public sector, where under all regimes various groups can express their intensity of preference for economic and social policy but, especially under the nondemocratic regimes, are blocked from expressing their preference for who holds public office.

## Cross-Country Regression Evidence

In recent years, the country-panel data used in the economics and political science literatures has expanded significantly and improved in quality. There are also multiple political science data sets measuring the extent and form of democracy in nearly all the countries of the world and, for any one of those countries, providing distinct democracy measures for each of dozens (if not hundreds) of years. In this section, we begin by discussing how we measure democracy and what other economic and demographic variables might also shape what policies are

<sup>3</sup> Mulligan and Tsui (2003) take this argument a step further, arguing that democracy affects public policies with the potential for limiting political competition and affects the “markup” received by the leader who successfully blocks his competition, but otherwise does not affect economic and social policy choices. Mulligan and Tsui argue that, in theory, the markup comes in the form of a little extra government revenue, some of which may be spent on activities limiting political competition.

chosen. We then display cross-country regression estimates of the effect of democracy on economic and social policies, conditional on some economic and demographic characteristics of countries. Four of the policy variables are amounts of government spending on various programs, while four others are measures of the amount and design of taxation. Six of the eight are arguably important instruments for income redistribution.

### **Measuring Democracy**

To examine how democracy influences policy choices, the starting point is measuring democracy. The POLITY IV (2000) project from the University of Maryland calculates various measures of how a country is governed for 181 countries going back as far as 1800. Of the 181, 142 countries existed for more than a few of the years from 1960–1990, which is the main timeframe of our analysis. Practically every piece of the land on the earth is covered by one of these 142 countries, except for some island nations. The POLITY data are fairly complete, but they are missing during years of occupation, political interruption or political transition, which might include occupation by foreign powers, a collapse of central political authority or an executive-branch-guided process of institutional planning. Since our study begins with the three tenets of voting theory, it is important that the POLITY index measures the existence and fairness of the voting process, rather than civil liberties, economic freedom or policies that may affect these liberties.

The POLITY IV data calculates a variety of different measures and indices. We will emphasize their “democracy” index, which takes integer values 0–10. In our empirical work, we divide the index by 10 to put the numbers on a scale from 0 to 1. The democracy index includes as one component an indicator of the extent to which government executives are chosen through competitive elections, plus the presence of rules for political participation and the transfer of executive power. We construct a country’s averages (using the available years) for the period 1960–1990.<sup>4</sup> Hence, in a regression of policy index  $p$  on democracy and other variables, the democracy coefficient might be interpreted as the partial effect on  $p$  of going from 30 years of complete autocracy (as in Iraq, North Korea and 35 other countries in our sample) to 30 years of complete democracy (as in the United States, the United Kingdom and 20 other countries).

### **Other Control Variables**

Variables other than democracy are at least part, and maybe all, of the determinants of the various public policies. Therefore, we control for such variables, with an emphasis on those used in prior studies of policy determination. Five control variables are included in all of our specifications: a dummy variable for whether a country is communist; a dummy variable for whether the country has a

<sup>4</sup> We also experimented specifications that use one of two other alternative POLITY indexes. The first alternative is an “autocracy” index that also takes integer values 0–10, although it weights the “democracy” index components differently (and negatively). A second alternative is just the competitive executive elections component (measured 0, 1, 2, 3 by POLITY). Replacing the democracy index with one of these others yields very similar results.

British legal origin; the percentage of the population over age 65; a measure of the total population; and a measure of per capita GDP (or the share of value added from agriculture). We also experimented with a number of other control variables, with some of the results presented here, including ethnolinguistic fractionalization, the income Gini coefficient and various measures related to military activities.

We include a dummy variable for whether a country is communist or not for more than a few years during the period from 1960–1990, based on the 26 communist countries listed by Kornai (1992). Centrally planned economies never had fully democratic political institutions, but unless we are sure that central planning was the consequence of nondemocracy rather than the reverse, it is important to try to separate nondemocracy from central planning. In addition, centrally planned economies are subject to especially severe measurement problems; for example, how large were government purchases or government transfers in the USSR? Using the communist dummy variable helps us prevent confounding answers to the relatively easy question “Is the public sector different in a centrally planned economy?”—of course it is (Pryor, 1968; Kornai, 1992)—with the harder question “How does democracy affect the public sector?” Interestingly, as compared with other nondemocracies, communist countries were significantly different in only seven of our 15 policy measures.<sup>5</sup>

Countries with “British” legal systems—namely those relying more on judicial discretion, broad legal principles and oral arguments rather than explicit legal codes—are distinguished from countries with civil law systems (of various types) as measured by World Bank’s Global Development Network Growth Database. La-Porta, Lopez-de-Silanes, Shleifer and Vishny (1999) and Glaeser and Shleifer (2002) offer more interpretation of this variable, although for our purposes, it is not important whether this variable be interpreted as an effect of the legal system or as a proxy for a variety of things British.

Public pensions are a big component of government spending, so it may be important to know how many people are old. For this purpose, we use the share of population that is 65 years old or older. The public sector may exhibit economies of scale; for example, Mulligan and Shleifer (2003) suggest that economies of scale are important for regulatory policy, so we measure total population. Our population measures come from the ILO (1996) and are available only at ten-year intervals. We construct each country’s averages for the period 1960–1990.

The standard of living in a country may influence the level of government services provided and the amount of taxation. 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 1960–1989. Eleven of the 141 countries are not in the Penn World Tables. Eight of these eleven countries are communist; the other three are Equatorial Guinea, Lebanon and Libya.

We use World Bank variables measuring the fraction of national value added from agriculture, averaged for the years 1960–1990, in part because these variables

<sup>5</sup> We also experimented with a “Soviet bloc” dummy, rather than a “communist” dummy, with no substantial effect on the estimated coefficients.

proxy for the level of development, but also because they indicate the fraction of the population that may have been covered by, or have ready access to, various public programs (note that many countries, including the United States, do not or have not covered agricultural workers in some of their programs). Although not reported here, we have also used World Bank measures of the fraction of population in urban areas and the fraction of labor in agriculture.

Finally, we include in some cases a measure of military situations as a control variable. For example, we include years in which the country was at war (from Jones, Bremer and Singer, 1996) in regressions about military spending and conscription. Young men are important inputs into military activities, so we also measure the number of males aged 15–24 in each country. Like the information on the share of the population who are elderly, this data comes from the ILO (1996) and is available only at ten-year intervals. We construct each country's averages for the period 1960–1990.

### **Spending Policy: Government Consumption, Education Spending and Social Spending**

Government consumption, government revenue, education spending and social spending do not significantly impact political entry barriers (more below on how we classify policy instruments on their potential to limit competition for public office), so we consider them first as a group in our comparison of democratic and nondemocratic public sectors. Table 1's columns 1–2 are regressions that seek to explain the level of government consumption, measured as a percentage of GDP. Most countries in our sample are measured to have government consumption between 10 and 30 percent of GDP. The democracy coefficient in column 1 is statistically insignificant, although the magnitude is fairly large. Since we find democracies to spend about 3 percentage points of GDP less on the military (more on the military later), and military is a component of government consumption, our point estimate implies that democracies are spending somewhat more on nonmilitary government consumption. However, if we include military spending as an independent variable as in column 2, we still fail to find a statistically significant difference between democratic and nondemocratic government consumption.<sup>6</sup> Perhaps another difference between our government consumption and military spending regressions is that the latter is monitored by the Stockholm International Peace Research Institute, which provides an outside source of information on military expenditures, whereas the government consumption figures from the Penn World Tables are based in part on national accounts information provided by each government. Large countries, poor countries and British legal origin countries spend a greater fraction of GDP on government consumption.

<sup>6</sup> We also experimented with using (government consumption – military spending) as the dependent variable. The coefficient on democracy remains statistically insignificant. Also, our results do not change substantially if we exclude the four countries that according to Summers and Heston (1991, p. 342) have particularly questionable national accounts. The number of countries sometimes varies across columns in our Tables 1–3. For brevity, we exclude specifications that show how little the democracy coefficient changes just by changing the sample of countries, keeping the same dependent and independent variables.



*Table 1*  
**Democracy and Spending Policy across Countries**

<i>Independent variables</i>	<i>Dependent variable: (each is a percentage of GDP)</i>		<i>Government consumption, 1960–1990</i>	<i>Education spending, 1980–1990</i>	<i>Social spending</i>	
			<i>1960–1990</i>	<i>1980–1990</i>	<i>Pension</i>	<i>Nonpension</i>
	(1)	(2)	(3)	(4)	(5)	
Democracy index, 1960–1990	<b>–1.27</b>	<b>1.57</b>	<b>0.42</b>	<b>–0.08</b>	<b>0.72</b>	
	(2.14)	(1.99)	(0.52)	(0.44)	(0.63)	
Communist dummy	–0.87	–0.45	1.09	0.82	1.62	
	(1.75)	(1.66)	(0.45)	(0.40)	(0.60)	
British legal origin	2.80	2.91	0.53	–0.35	–0.86	
	(1.28)	(1.17)	(0.31)	(0.28)	(0.40)	
Percentage of population aged 65+, 1960–1990	0.01	0.25	0.07	0.58	0.49	
	(0.25)	(0.23)	(0.06)	(0.05)	(0.07)	
Log(population)/10, 1960–1990	–9.77	–8.16	–2.28	–0.24	0.02	
	(2.54)	(3.36)	(0.86)	(0.76)	(1.08)	
Real GDP per capita, 1960–1989 average, log	–2.96	–4.58	–0.06			
	(0.75)	(0.76)	(0.29)			
Share of value added from agriculture, 1960–1990			–3.38	–1.95	–2.01	
			(1.60)	(0.93)	(1.35)	
Military spending, percentage of GDP		0.60				
		(0.14)				
Adj-R-squared	.27	.38	.25	.78	.65	
Countries	131	125	110	110	102	

*Notes:* Ordinary least squares standard errors in parenthesis. All regressions include a constant term (not shown).

According to the Chicago political-economic school, and the barriers-to-entry qualification of it, education and social spending—for that matter any spending that does not directly benefit a dictator or help a dictator to block competition for office—should not be significantly correlated with democracy.<sup>7</sup> Education spending (also expressed as a percentage of GDP; from UNESCO) is the dependent variable in column 3. The coefficient on democracy is statistically insignificant. The communist coefficient is more than 1 percent of GDP, perhaps consistent with Lott's (1999) finding that totalitarian countries spend more of their GDP on education.<sup>8</sup> Richer countries, smaller countries and perhaps also British legal origin countries also spend more on education.

Policy instruments that have the potential to redistribute from rich to poor

<sup>7</sup> The barriers-to-entry approach has one qualification of this, because political barriers to entry permit the dictator to collect a "markup," which raises the marginal deadweight cost of taxes that can be directed to programs not benefiting the dictator. See Mulligan and Tsui (2003) for a formal model and a discussion of the possible magnitude of this effect.

<sup>8</sup> Lott's (1999) totalitarian variable is from Freedom House and may be more similar to POLITY's democracy variable than to our communism variable. He includes a measure of total government size as a regressor; when we do so (not shown in the Table), the democracy coefficient is significantly *positive* (1.21) and the communist coefficient is –0.19. Lott does not include British legal origin, population, the elderly share or agriculture as regressors.

may provide the toughest test of the hypothesis that “voting is not an important determinant of public policy.” Voting models like Meltzer and Richard (1981), Boix (2003) and Acemoglu and Robinson (2003) emphasize the equal distribution of votes as compared to income and wealth, so that democracies should better represent the poor. Among spending programs, those classified as “social security” may help the poor. We measure the amount of social security spending as a fraction of GDP and separately for pension and nonpension programs.<sup>9</sup> The democracy coefficient is negative for pensions and positive for other social spending in specifications 4 and 5 of Table 1, although neither is statistically significant. The democracy coefficient of 0.72 shown in column 5 is the larger of the two in magnitude, but is still small since the standard deviation of nonpension social spending across countries is 2.8 GDP percentage points, and the point estimates imply that changing from complete nondemocracy to complete democracy for every one of the years 1960–1990 would have a smaller effect on nonpension social spending than would a mere 1.5 percentage point increase in the fraction elderly. The communist coefficients are significant. Older and less agricultural countries spend more on pensions and other social programs.

In principle, democracies and nondemocracies may spend the same total amount on education, social security or some other program, but be very different in how they spend it. Investigating this possibility is beyond the scope of this paper, but in Mulligan, Gil and Sala-i-Martin (2002), we have looked at public pension programs in more detail. Conditional on the age of the population, it is hard to see any correlation between democracy and how pension programs obtain their revenue (for example, by using a flat or capped payroll tax or taxing mainly employers rather than employees) and whether benefits are paid subject to a retirement test or an earnings test. Lott (1999) has a similar finding for public health spending and its composition, but more research is needed to determine whether democracies are special in terms of the detailed composition of their spending on education and other items.

### **Tax Policy: Corporate Rates, Personal Tax Flatness, Payroll Tax Caps and Total Revenue**

We have four measures of tax policy, three of which indicate the degree to which a government taxes the rich. The first is the top rate of corporate income taxation, although measured only from 1987–1990. Table 2’s specifications 1 and 2 show that democracy does not predict the corporate tax rate, and the sign of the democracy coefficient depends on whether democracy is measured 1960–1990 or 1975–1990 (arguably it needs to be measured more recently in the corporate rate equations because that is our only policy variable not dating back to the 1960s or 1970s). None of the demographic and economic variables predict the corporate tax

<sup>9</sup> As reported by the ILO, nonpension Social Security spending includes “family allowances,” “unemployment benefits,” “employment injury benefits” and “sickness-maternity benefits.”

Table 2  
**Democracy and Tax Policy across Countries**

<i>Dependent variable:</i>	<i>Corporate tax rate 1987– 1990, percentage</i>		<i>Personal income tax flatness, 1973–1990</i>		<i>Payroll tax capped, 1958–1995</i>		<i>Total revenue as percentage of GDP, 1973–1990</i>	
<i>Independent variables</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy index, 1960–1990	<b>2.65</b> (4.20)		<b>0.08</b> (0.03)	<b>0.08</b> (0.03)	<b>0.10</b> (0.21)	<b>0.14</b> (0.22)	<b>−3.85</b> (3.30)	<b>−6.94</b> (3.23)
Democracy index, 1975–1990		<b>−0.39</b> (4.03)						
Communist dummy	3.91 (5.71)	2.74 (5.76)	0.03 (0.03)	0.03 (0.03)	−0.33 (0.19)	−0.32 (0.19)	9.03 (3.09)	
British legal origin	−1.12 (2.66)	1.79 (2.58)	0.02 (0.02)	0.01 (0.02)	−0.02 (0.12)	−0.10 (0.13)	1.69 (1.94)	1.81 (2.00)
Percentage of population aged 65+, 1960–1990	0.32 (0.47)	0.48 (0.48)	0.01 (0.00)	0.01 (0.00)	−0.85 (2.41)	−0.01 (0.02)	1.76 (0.38)	2.03 (0.39)
Log(population)/10, 1960–1990	4.92 (7.40)	5.22 (7.42)	−0.02 (0.05)	−0.02 (0.05)	0.10 (0.33)	0.26 (0.33)	−12.42 (5.56)	−12.10 (5.70)
Real GDP per capita, 1960–1989 average, log	−0.51 (1.80)	−0.22 (1.79)	0.02 (0.02)	0.03 (0.02)	0.09 (0.09)	0.15 (0.11)	5.21 (1.24)	5.17 (1.28)
Ethnolinguistic fractionalization				0.06 (0.03)		0.36 (0.23)		
Adj-R-squared	.00	.00	.63	.65	.04	.07	.55	.52
Countries	80	80	60	59	71	69	115	115

*Notes:* Ordinary least squares standard errors in parenthesis. All regressions include a constant term (not shown).

rate, either. We also tried, unsuccessfully, to predict the corporate tax rate with the Gini coefficient and ethnolinguistic fractionalization.<sup>10</sup>

Personal income tax flatness is measured as the ratio of the economy-wide average income tax rate to the top marginal income tax rate (Becker and Mulligan, 2003). Flatter personal income taxes (namely, those that collect a larger fraction of GDP for a given top marginal tax rate) may be associated with less income redistribution. Specifications 3 and 4 show that democratic, old and fractionalized countries tend to have flatter income taxes.<sup>11</sup> In other words, specifications 3 and 4 by themselves suggest that democracy and inequality are associated with *less* redistribution!

<sup>10</sup> All of the cross-country regressions in our paper are ordinary least squares. Although not shown in the tables, we have also estimated every regression as a median regression, with each estimated democracy coefficient essentially the same as its ordinary least squares analog. The only exceptions are the median regression analog to the corporate tax rate specifications 1 and 2 in Table 2 and our trade policy openness specification 9 in Table 3. For example, when estimated by median regression, specification (2) has a democracy coefficient of  $-7.55$  (s.e. = 3.27), a communist coefficient of 10.00 (s.e. = 4.49) and an elderly coefficient of 1.00 (0.39); democracies seem to redistribute *less*, as measured by the corporate tax rate. See below for more details on trade policy openness.

<sup>11</sup> The 60-country sample average and standard deviation of the flatness variables is 0.08. The United States value is 0.18.

The U.S. payroll tax for Social Security is collected in 2003 as 12.4 percent of earnings below \$84,000 annually and *none* of the earnings above that. Obviously such a “capped” tax is a less powerful tool for rich-poor redistribution than an uncapped tax such as, say, a 10 percent tax on *all* earnings. A number of countries have also capped their social security tax, and we have coded the fraction of the years 1958, 1975 and 1995 (or subset of these years if data were unavailable for one or two of them) that they did so. Columns 5 and 6 show small and statistically insignificant (and *positive*) democracy and fractionalization coefficients. Mulligan, Gil and Sala-i-Martin (2002) find democracies to be more likely to cap the payroll tax, and their democracy coefficients are statistically significant, using a different index (the Gastil index) to measure democracy. If Table 2 suggests any difference between democracies and nondemocracies, it is that democracies redistribute slightly *less* than economically and demographically similar nondemocracies.

What about the impact of democracy on total revenue collected by the government? Some models based on the three tenets of formal voting models predict that democracies collect more revenue because they redistribute more to the poor and middle class. Boix (2003, p. 171) explains: “Under an authoritarian system, where all or a substantial part of the electorate is excluded from the decision-making process, the size of the public sector should remain small.” Olson (1993) also begins his analysis with the highly unequal distribution of political power under authoritarian systems, but stresses that authoritarian governments should act almost as a leviathan—which taxes up to the point where tax base shrinkage is so severe that no additional revenue can be raised—because the leadership and its sponsors bear very little of the tax burden. Hence, Olson’s approach suggests that a nondemocracy would have a much larger budget than a democracy, although both approaches agree that nondemocracies would spend little on the poor. For very different reasons, the barriers to entry approach suggests that nondemocracies would raise somewhat more revenue; for example, Mulligan and Tsui (2003) guess about 3 percent of GDP, because that would be a markup of roughly the size earned by private sector monopolies.

In fact, column 7 suggests that democracies collect almost 4 GDP percentage points less in revenue than do noncommunist nondemocracies.<sup>12</sup> The barriers-to-entry approach interprets this 4 percentage points as a markup, part of which would be spent on limiting political competition. Interestingly, nondemocracies are spending almost exactly this amount extra on the military (see below). Our communist coefficient of 9 percentage points of GDP in the revenue regression is quite large. But as Kornai (1992, p. 138) explains, communist governments have an important revenue source—profits from government enterprises—that is much

<sup>12</sup> The magnitude of the democracy point estimate is somewhat sensitive to the treatment of United Arab Emirates (UAE), a country with very little “revenue” but a lot more “grants.” If, in the least squares specification, we measure the dependent variable for UAE as revenue + grants or drop UAE, the point estimate is –5 or –7, respectively. If we use a median regression, the point estimate is –4 regardless of how we treat UAE.

smaller in other countries. If we remove the communist dummy from the regression as in Table 2's last specification—in effect combining communist countries with other nondemocracies—we see democracies taxing 7 percentage points of GDP less.<sup>13</sup>

### **Policies that Affect Barriers to Entry: Death Penalty, Military Spending, Civil Liberties, State Religion, Conscription and Open Trade**

To this point, the main theme is that democracy has little effect on many economic and social policy variables. The cautious reader may wonder whether the lack of correlation between democracy and political policies may be an artifact of poor measures of democracy or policy. While no data in this area are perfect, there are several reasons to believe that measurement error does not drive our results. First, as noted earlier, the quality and quantity of cross-national data has improved and expanded dramatically in recent years. Second, in many cases we average each country's economic and political variables over time, with the hope that some measurement errors wash out in the time averages. Third, we identify case study countries (see below) where we are quite confident that a political regime change has occurred, but nonetheless observe little change in economic and social policies. Fourth, we *do* find significant correlations between democracy and our public policy measures, when the public policies measured are among those that arguably limit entry into the competition for political office. The purpose of this section is to display the results for the competition-limiting policy variables.

Our first step is to classify policies according to their potential to limit competition for public office. Tullock's (1987) book is quite useful in this regard, because it gives little attention to the democracy-autocracy policy gap and much attention to autocratic activities related to erecting political entry barriers. Tullock mentions five policy steps that are important for blocking entry: torture (pp. 61, 62, 64, 65), the death penalty (pp. 6, 20, 65, 80), press censorship (p. 154), regulation of religion (p. 108) and maintaining an army. He also mentions conscription and trade barriers, but suggests (pp. 72, 123, 186) that their impact on political competition is ambiguous. Our other policy measures—education spending, government consumption, pension spending, nonpension “social” spending, corporate rate, income tax flatness and payroll tax caps—are not mentioned by Tullock, and we thereby classify them as “economic and social” policies rather than barriers to entry.

Clearly, violent government tactics like kidnapping, torture and execution are expected to cause its potential competitors to pause. Ideally, we would like measures of the degree to which citizens are deterred from *political* activity by the *potential* of the government to use violent tactics. Instead, we must settle for three

<sup>13</sup> None of the regressions shown in this paper include interaction terms. But we have interacted democracy with the Gini coefficient, percentage agriculture and percentage old. The democracy-agriculture and democracy-Gini interactions never matter (for that matter, the Gini never matters in levels), even when one of them is included by itself to the exclusion of all other interactions terms. When an old-GDP interaction is included—and it should be for the same reason that the democracy variable should be in a levels regression together with GDP—the old-democracy interaction matters only for personal income tax flatness (democracy flattens income taxes, especially when the country is older) and for two of the “barrier-to-entry” policies studied below.

proxies. The first proxy comes from a panel data set for 142 countries on use of the death penalty. This is not to imply that the death penalty serves only the purpose of blocking political entry (it may mainly deter criminal activity), only that non-democratic governments have *more* reasons, because of creating barriers to political entry, to put citizens to death. Perhaps having the death penalty is also correlated with use of other violent tactics to limit competition. The other two proxies—one from the U.S. Department of State and another from the human rights organization REDRESS—are dummy variables for whether a country has a widespread practice of torture by the police.

The dependent variable in columns 1 and 2 of Table 3 is a dummy variable for whether widespread torture was practiced circa 2002, according to the U.S. State Department and REDRESS, respectively. Our most recent POLITY data are for 1998 and 1999, so our democracy variable here is the average for those two years. Even though “credible” reports of torture have been received from both democracies and nondemocracies around the world, the democracy coefficients are economically significant (and barely statistically significant). Both specifications 1 and 2 include a measure of the number of young men as a proxy for the potential amount of police activity (reports of torture are those committed by the police). Because torture and some of the other barriers-to-entry behaviors are recorded only when there is a minimum absolute number of internationally recognized incidents, we include log population in all specifications to capture the possibility that small countries may have better success at suppressing political competition without international recognition.

We measure the presence of the death penalty by the fraction of years 1976–1990 that Amnesty International codes each country as having a “retentionist” policy regarding the death penalty. The sample size is 131 countries, because the Penn World Tables lack information on per capita GDP over this time in 11 nondemocratic countries. The democracy coefficient of  $-0.56$  shown in specification 3 is economically and statistically significant, and the communist coefficient is much smaller, which means that nondemocracies are much more likely to use the death penalty regardless of whether they are of the communist variety. Interestingly, large and British legal origin countries are more likely to have the death penalty. The coefficient on the GDP variable is small and marginally significant—richer countries use the death penalty more.

Naturally, we experimented with a number of other specifications not shown in the table. For example, we dropped the per capita GDP variable and ran the regression with a full sample of 142 countries, but the coefficients on the other variables changed little. In specifications that added an explanatory variable for the fraction of population that is Muslim, some observations were lost and the estimated Muslim coefficient is positive, but statistically insignificant, and the other coefficients are very similar to those shown in the table.<sup>14</sup>

<sup>14</sup> We also reran every regression in this paper, adding a variable that measures connectedness with the rest of the world economy,  $\log[(\text{exports} + \text{imports})/\text{GDP}]$ , as an independent variable. The democracy coefficient was never affected by the addition, and the coefficient on the added variable was statistically

Table 3

**Democracy across Countries—Policies that Might Affect Public Office Competition**

<i>Dependent variable:</i>	<i>Widespread torture c. 2000</i>		<i>Fraction of years 1976–1990 with death penalty</i>	<i>Military spending 1973–1990</i>	<i>Civil liberties, 1973–1990</i>	<i>Regulated religion, 1970–1990</i>	<i>Fraction of years 1985, 1990, 1995 drafting</i>	<i>Open trade policy</i>
<i>Independent variables</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy index, 1960–1990 (but see notes)	<b>−0.28</b> (0.14)	<b>−0.29</b> (0.13)	<b>−0.56</b> (0.13)	<b>−3.44</b> (1.20)	<b>0.55</b> (0.04)	<b>−0.30</b> (0.15)	<b>−0.01</b> (0.14)	<b>0.17</b> (0.11)
Communist dummy	−0.17 (0.12)	0.04 (0.11)	0.11 (0.10)	0.87 (1.03)	−0.18 (0.03)	0.41 (0.13)	0.12 (0.11)	−0.31 (0.08)
British legal origin	0.06 (0.09)	0.08 (0.08)	0.31 (0.08)	0.17 (0.73)	−0.01 (0.02)	−0.08 (0.09)	−0.44 (0.08)	0.03 (0.07)
Fraction of population aged 65+, 1960–1990	3.15 (2.07)	0.73 (1.87)	0.22 (1.49)	−21. (14.)	0.78 (0.47)	−0.57 (1.79)	1.57 (1.56)	4.39 (1.27)
Log(population)/10, 1960–1990	1.04 (0.25)	0.86 (0.23)	0.46 (0.21)	−1.82 (2.26)	−0.01 (0.07)	−0.07 (0.25)	0.71 (0.25)	0.01 (0.19)
Real GDP per capita, 1960–1989 average, log	0.08 (0.06)	0.03 (0.05)	0.09 (0.04)	1.40 (0.46)	0.04 (0.01)	0.16 (0.05)	−0.06 (0.06)	0.08 (0.05)
Years at war since 1950				0.58 (0.13)			−0.09 (0.16)	
Armed forces per male aged 15–24, 1985–1995							1.67 (0.58)	
Fraction of males aged 15–24, 1985–1995	2.92 (2.71)	3.93 (2.45)						
Adj-R-squared	.21	.15	.22	.23	.85	.17	.35	.53
Countries	121	121	131	125	130	131	127	113

*Notes:* Ordinary least squares standard errors in parenthesis. All regressions include a constant term (not shown). Democracy index is 1998–1999 average in specifications (1) and (2). Torture from State Department and REDRESS in specifications (1) and (2), respectively. Military spending (specification 5) measured as percentage of GDP.

Military spending is another policy measure with implications for barriers to entry, and therefore should be lower in democracies. The military affects a leader's position in at least three ways. First, a dictator's best competitor may have to take over by force, and the military may have some of the personnel and equipment needed for a coup. Military policy therefore affects this kind of entry, perhaps by

significant in all models except for education spending, nonpension social spending and total revenue (coefficient was positive coefficient in these cases). Including this variable reduces sample sizes significantly (eliminating mainly nondemocracies), so it was excluded from the regressions shown in the paper.

keeping the soldiers happy with the regime or by keeping them occupied with foreign conflicts. Indeed, Elman (1997) uses some of these reasons to explain why nondemocracies might be more prone to international conflict. Second, democratic leaders may have less reason to be concerned with foreign military threats. For example, foreign invaders may be more eager to attack a nondemocratic country because its citizens are less supportive of the regime (or domestic political competition may leave the country to avoid the political repression, and stage an attack from abroad). Third, military personnel may be used for domestic policing purposes.

Specification 4 uses military spending as a percentage of GDP as the dependent variable. Its coefficient of  $-3.44$  is large and statistically significant. The communist coefficient is much smaller, which means that nondemocracies spend a couple of percentage points more of their GDP on the military, regardless of whether they are communist. Richer and warring countries spend more of their GDP on the military. Schmitter (1971) and Habibi (1994) also find military spending to be correlated with political regime, and there is a large “democratic peace” literature on democracy and fighting wars. Elman (1997) surveys some of the literature, where findings lean toward some connection between democracy and peaceful foreign policy, at least vis-a-vis other democratic countries.

Press censorship, prohibition of various public gatherings and other suppression of civil liberties can be used to block entry and/or allow the political leader to gain a public support advantage over the competition. A civil liberties index is the dependent variable in column 5. Freedomhouse.org has an index taking on integer values 1–7 for each country and each year 1973–1990, based on indices of human rights, personal autonomy, freedoms of expression, belief and association (but not on the fairness of the electoral process). To form our index, we standardize their index on a 0–1 scale and average over time for each country. Not surprisingly, specification 5 shows that richer and democratic countries have more civil liberties, while communist countries have less.

An organized religious presence may be able to speak effectively for or against a political leader. Regulating religion can be like regulating the press, so that nondemocracies have added reason to do so.<sup>15</sup> Specification 6, which has as dependent variable the fraction of years 1970, 1975, 1980, 1985 and 1990 that the country had either a state religion or prohibited religious practice, shows that richer and nondemocratic countries are more likely to regulate religion, especially the communist countries.<sup>16</sup>

<sup>15</sup> Adam Smith (1776, Book V, Part I, “Of the Expence of the Institutions for the Instruction of People of all Ages”) explains how politicians join with the clergy, with both parties attempting to stifle their respective competition (we owe this reference to Larry Iannaccone). See Gill (2002, p. 15) for some qualifications of the proposition that political competition promotes religious liberty.

<sup>16</sup> If we change specification 7 to exclude the nine countries missing information about religious composition of the population, coefficient estimates are hardly changed. If we add fractions of the population Muslim and Catholic, the democracy and communist coefficients (standard errors) are  $-0.14$  (s.e. = 0.16) and  $0.50$  (s.e. = 0.14), respectively. The coefficient on the Muslim and Catholic fractions are  $0.57$  (s.e. = 0.13) and  $0.32$  (s.e. = 0.12), respectively.



The decision to have a conscripted or volunteer army may have important consequences for political barriers to entry, although the direction of the effect may not be clear. A volunteer army may be happier and more effective at stopping foreign invaders. On the other hand, the regular turnover of soldiers in a conscripted army may hinder the organization and training of military personnel needed to stage a successful coup. Ross (1994) predicts conscription in a cross-section of countries, using a democracy index, the size of armed forces, a measure of participation in wars and public spending independent variables. Mulligan and Shleifer (2003) follow Ross (1994) and add, among other things, British legal origin and log population. Neither finds effects of democracy or public spending on conscription. In our findings, the democracy coefficients are almost exactly zero. The other coefficient estimates suggest that smaller countries, common law countries and countries with smaller armed forces per capita are more likely to have a volunteer rather than a drafted army.

Limiting international trade—if it has the collateral effect of limiting the international flow of political ideas and of military hardware to the political opposition—may help erect barriers to entry, for the same reason press censorship and military spending might. International trade serves many other purposes and in these other ways may promote popular support. Openness of trade policy is coded 0–1 by Sachs and Warner (1995) for 113 of our 142 countries, for the years 1950–1990, based on tariff rates, nontariff trade barriers, the black market exchange rate premium and whether there was a state monopoly on major exports. Our “trade openness” policy variable for each country is the fraction of years 1960–1990 that Sachs and Warner coded open. The final specification in Table 3 has this as the dependent variable. The communist coefficient is economically and statistically significant, which perhaps accords with common knowledge that communist regimes limit contact with the outside world. But the reader should also note that communist is part of the definition of Sachs and Warner’s variable, and like us they code communist countries using Kornai (1992). The democracy coefficient estimate is positive but statistically insignificant, which means that our data do not permit us to reject the hypothesis that noncommunist dictators have the same (in terms of open versus closed) trade policies as democratic countries.<sup>17</sup> Interestingly, older and perhaps richer countries have more open trade policy.

<sup>17</sup> Results from the median regression analogue to specification 9 are somewhat different: the democracy, communist and elderly coefficients are 0.40 (s.e. = 0.08), –0.17 (s.e. = 0.06), and 4.80 (s.e. = 0.93), respectively. If POLITY’s autocracy index replaces the democracy index, the three ordinary least squares coefficients are –0.24 (0.12), –0.31 (0.08) and 4.61 (s.e. = 1.22), respectively. Also note that we have coded trade policy openness by country year from the verbal description in the Sachs-Warner appendix. Alternative specifications relate separate numerical measures of tariff policy, quota policy and state export monopolies (Rodrik and Rodriguez 2000)—which are measured for only one year—to democracy and the other variables; when we do so, we find no systematic democracy effect (but see Dougan and Snyder, 1993).

## Case Studies

Case studies obviously lack the power of systematic statistical tests, like the regressions presented in the previous section, but they offer some advantages, too. They may sharpen the data by relying more on country-specific data sources. They can help to clarify causal mechanisms; for example, if we think that the death of Francisco Franco in Spain would be the end of dictatorship regardless of when it occurred, and that the year of his death was not affected by Spain's public policies, GDP or the average age of its population, then the transition from dictatorship to democracy in Spain may offer a sharp test of how such a change affects public policy.

Our overall strategy for selecting countries for closer study is to look first for examples of a dramatic change in political regime (either from democracy to dictatorship or vice versa) and then to observe whether this change is followed by dramatic alterations in the size or the design of various public programs relative to changes occurring elsewhere in the world. Our case study countries are therefore Portugal, Spain, Greece (with Italy as a continuously democratic benchmark) and Chile (with various South American countries as benchmarks). Mulligan, Gil and Sala-i-Martin (2002) present more details on how countries were selected.<sup>18</sup>

## Spending and Tax Policies

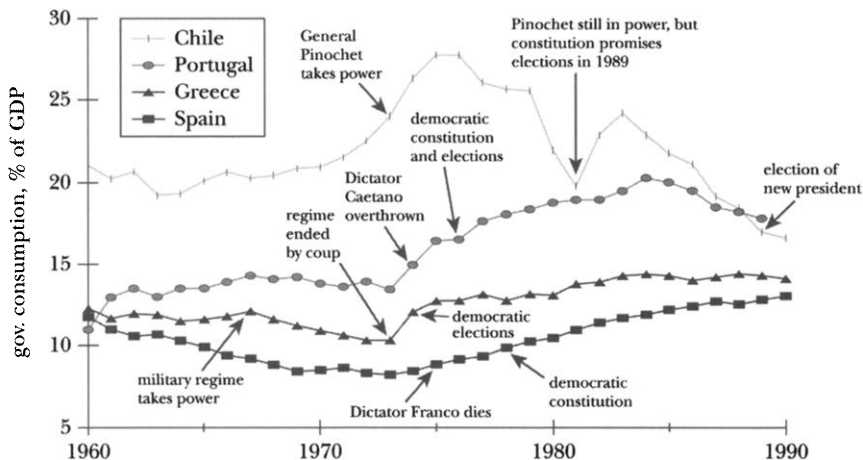
Figure 1 displays government consumption of goods and services for the case study countries. Remember that the most recent Portuguese and Greek democratic regimes began in 1976 and 1974, respectively (Greece also had a democratic regime ending in 1966). In Spain, dictator Francisco Franco-Bahamonde died in 1975, and a democratic constitution was adopted in 1978, although arguably, the threat of nondemocracy was still very serious until the early 1980s. Spain and Portugal show slightly more government consumption after 1980. Since continuously democratic Italy (not shown in the figure) had a very constant ratio of government consumption to GDP during the period 1970–1990, this might be interpreted as a small effect of democracy, although the uptrends in Spain and Portugal may begin some years before their democratic regimes.<sup>19</sup>

Government consumption in Greece rises only slightly after the arrival of democracy. The data for Chile shows a more dramatic correlation with democracy,

<sup>18</sup> The case studies can be put in a regression context by taking our data set of 142 countries over 31 years, splitting it in two time periods (1960–1974 and 1975–1990) and obtaining panel estimates of democracy's effect on various policy outcomes (only government consumption, social spending, revenue, military spending, trade policy openness, civil liberties and religions regulation can be measured in the earlier time period; we comment below on the time pattern of democracy and death penalty). Such panel estimates are probably dominated by the case study countries, so it may not be surprising that, regardless of whether we include country fixed effects, the results are the same as shown in our Tables 1–3, except that democracy effects on military spending and trade policy openness are insignificant when country fixed effects are included. Although beyond the scope of our data set, following the case studies beyond 1990 would also be interesting.

<sup>19</sup> Figure 1 derives from Penn World Tables, which interpolate individual country data whenever years are missing.

Figure 1

**Government Consumption of Goods and Services**

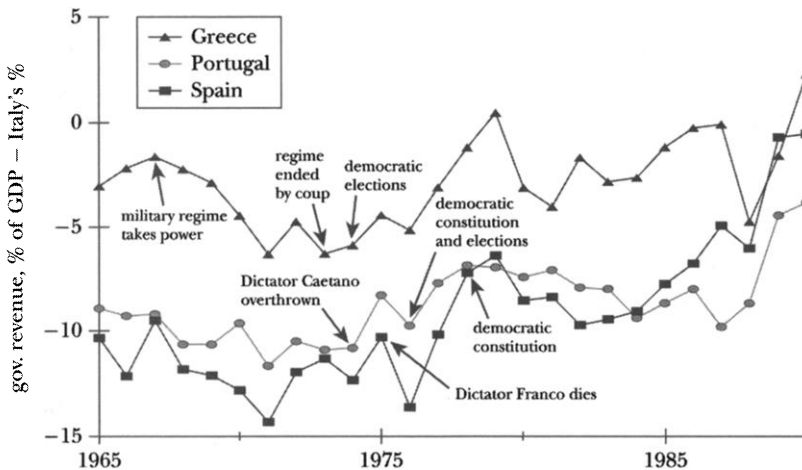
although in the opposite direction. In Chile, government consumption was at its peak during its least democratic years; that is, dictator Pinochet took over in 1973, and Chile made a transition to democracy sometime during the 1980s. Taken together, these four cases do not indicate a systematic effect of democracy on government consumption.

Government revenue over GDP is growing during this period even under democratic regimes, so Figure 2 shows government revenue's percentage of GDP minus the benchmark percentage for Italy for each year. The Italian government collected about 30 percent of GDP in the 1970s and 35 percent of GDP in the 1980s. The cross-country estimates suggest that a move to democracy should decrease revenues. However, the Portuguese and Greek series each may suggest about a 3 percentage point *positive* effect of democracy on revenue. The Greek series is at its lowest during the early 1970s, during the military regime. Spain's series can be interpreted either way because revenue jumps shortly after Franco dies, although before the democratic constitution was adopted in 1978. Is this the effect of eliminating dictatorship? Revenue/GDP grew more slowly in Spain than in Italy in the five or six years after Spain's constitution was enacted in 1978. Is this the effect of introducing democracy? Or something special about Italy during the period 1979–1984?

Chile's data, which is not shown in this figure, are consistent with the cross-country estimate. According to the IMF, the Chilean revenue share was highest—32–33 percent of GDP—from 1975–1981 during Pinochet's dictatorship, as compared to 24–29 percent of GDP in the late 1980s. Mulligan, Gil and Sala-i-Martin (2002) point out that Pinochet added almost 40 percentage points to the employer portion of payroll tax rates between 1973 and 1975.

Mulligan, Gil and Sala-i-Martin (2002) look at time series for public pension spending in Spain, Portugal, Greece and Chile, as well as the use of earnings tests,

Figure 2

**Government Revenue's Percentage of GDP Minus the Benchmark Percentage for Italy**

retirement tests, delayed retirement credits, payroll tax caps and splitting of the payroll tax between employer and employee. Transitions to and from democracy were not associated with pension policy changes, except for slower spending growth in nondemocratic Greece and higher payroll taxation (and pension spending?) in nondemocratic Chile.

The European case studies seem to confirm our cross-country finding that democracies have flatter personal income taxes, although the magnitude of the democracy effect seems smaller in the case studies. During the period 1974–1979, many countries in the world were hiking the top personal income tax bracket (for example, Italy's hike of the top tax bracket from 48 to 72 percent and France's hike from 48 to 60 percent), the new democracies of Greece, Portugal and Spain were increasing the rate for the top bracket more slowly or not at all (Greek, Portuguese and Spanish hikes were 8, 2 and 11 percentage points, respectively) even while they were able to increase personal income tax revenues at rates similar to other countries.

**Policies Potentially Limiting Political Competition**

Our international information on the death penalty dates back to about 1970. Since then, a number of democracies have eliminated the death penalty. A number of other countries, like Nicaragua in 1979 and Haiti in 1987, eliminated the death penalty between dictatorships. Perhaps there has been a general trend in both democracies and nondemocracies toward abolishing the death penalty, although we are aware of few nondemocratic countries where the death penalty was eliminated mid-regime (Brazil's abolition in 1979 may be an exception). Spain abolished the death penalty as part of the 1978 constitution establishing its democracy. Portugal did the same two years earlier with the change in regime. As a comparison

to these two cases, Greece and Chile still keep the death penalty, but they restricted its use since 1993 and 2001, respectively. When compared with the rest of the world, these four cases suggest that democracies are less inclined to have the death penalty or perhaps that democratization happened to be coincident with a worldwide trend toward abolition.

So far as religion regulation is concerned, our data do not indicate many changes from democratic to nondemocratic regimes. Portugal, Spain and Italy had Catholicism as their official religions throughout the period we study (Barrett, 1982; Barrett, Kurian and Johnson, 2001).<sup>20</sup> Greece had the Orthodox religion as its official religion. There has been no official religion in Chile, at least since 1970. Therefore our case studies do not suggest that democracy has any influence on the religion regulation, but the reader should recognize that we have not studied communist countries and use a coarse measure of religious regulation.

With one exception noted below, the case studies suggest that democracies spend less on the military. To see this, notice first that, according to the Stockholm International Peace Research Institute (SIPRI), Italian military spending is a fairly stable percentage of GDP, declining slowly and steadily from about 3 percent in 1960 to about 2 percent in 1990. Portugal quickly decreased its military spending about 3 GDP percentage points after the end of the military regime. Chile's military spending (at least through 1978) was 1–2 GDP percentage points higher under Pinochet than it had been prior to his rule. Military spending in Greece was a larger fraction of GDP during most of the military regime than during the relatively democratic early 1960s. So we have three observations suggesting that democracies spend less on the military (SIPRI does not have much information on Spain), but we point out that recently democratic Greece seems to be spending about 2 GDP percentage points more than did the junta.

Data on the number of men in the armed forces (from the International Institute for Strategic Studies) also suggest that Portugal cut the size of its military as it began democracy in 1976, which coincides with the military spending data from the Stockholm International Peace Research Institute. Spain's armed forces show a temporary increase in the 10–15 years after Franco's death, but this may be a consequence of the baby boom's reaching military service age rather than a reaction to democracy. In Chile and Greece, the size of the armed forces fluctuates much less than does military spending as reported by SIPRI.

All of our case study countries had a military draft 1960–1990, so transitions into and out of democracy are not (among these countries) associated with eliminating or introducing conscription. However, we can look at the intensity of use of the draft, such as the number of months of service required. Spain and Italy required about the same length of service throughout this period, even though months of service were declining (from 24 months to 9–10 months), and Spain was democratic only half of the time. Greece required more months of service than Italy

<sup>20</sup> Barro and McCleary (2003, p. 19) suggest that, despite Barrett's coding, religion regulation relaxed in Spain and Portugal after 1975. They also look at state religion in a country cross-section and do not find a democracy effect.

or Spain, and that service did not decline much in the 1970s or 1980s as it did in the other two countries. In that sense, the recently democratic Greece used the draft relatively intensively. Chile required 12 months service 1970–1982, but since then service has been 17–24 months. The only South American country in our data set to increase length of service in the 1980s was Venezuela, from 24 to 30 months, while several other South American countries were decreasing it. In short, the transition to democracy does not seem to reduce conscription intensity in our case studies and may even increase it.

## **Conclusions**

Other studies in sociology, economics and political science have found little impact of democracy on particular public policies. For example, Cutright (1965), Jackman (1975) and Pampel and Williamson (1989) observed an obvious raw correlation between democracy and the introduction of pension and welfare programs (raw correlations like these are obvious in our data, too), but pointed out that economic development likely drives social programs and is correlated with democracy. Holding constant proxies for a country's age and income, they found democracies and nondemocracies to be pretty similar in terms of their likelihood of introducing a social program. In a study of European and advanced South American countries about a century ago (when half of those countries were still not democratic), Lindert (1994) found that, holding constant country age and income, the average democracy was similar to the average nondemocracy in terms of spending on public pensions, welfare, unemployment and health. Mulligan, Gil and Sala-i-Martin (2002) found little democracy-nondemocracy difference in terms of the details of how public pension programs obtain revenue and how they disburse it.

Despite improvements in the measurement of democracy, control variables and public policies, accumulation of more years of data and examination of case studies, we still find no significant partial correlation between democracy and the amount of spending on pensions or welfare. We add several economic and social policy outcome measures, with an emphasis on instruments of rich-poor redistribution, including education spending, the corporate income tax rate, personal income tax flatness and whether the payroll tax is capped, and find only one of them to differ between economically and demographically similar democracies and nondemocracies. The one difference is that democracies tend to have flatter (and thereby less redistributive) personal income taxes.

We offer an economic interpretation of these findings. As compared with the economic and social policies in authoritarian countries, democratic policies do not seem to ignore the intensity of policy preferences nor to reflect a significantly more equal distribution of political power. Economic and social policies in all kinds of countries are to a first approximation the outcome of tradeoffs—like efficiency, or conflicts among generations, or among industries

and occupations—that are basic to human nature and not specific to particular political institutions.

This is not to say that democracies and nondemocracies should always be expected to have the same public policies. The main empirical differences—both in the cross-country regressions and in the time series for countries with dramatic regime changes—are for policies relating to the process of winning and maintaining public office, rather than the social and economic policies featured in so many positive theories of the public sector. Authoritarian regimes are more likely to torture, execute, regulate religion, censor the press and spend a lot of money on the military. Authoritarian regimes tax more, in an amount commensurate with their extra spending on the military. We suggest that democratic institutions may have important effects on the degree of competition for public office, but otherwise have effects on public policies that are insignificant or incidental to the struggle for political leadership.

Some interesting questions arise when our empirical results are combined with those of a parallel literature, looking only at democracies, finding significant cross-country correlations between public spending and measures of the “type” of democratic institution used in the country. For example, Persson, Roland and Tabellini (2000) and Milesi-Ferretti, Rostagno and Perotti (2002) find the amount of public spending to be correlated with types of executive and electoral systems. What are some possible interpretations? One interpretation—less credible in our opinion—is that public spending is independent of *whether* there is (fair) voting, but, when public officials are elected fairly, public spending then becomes dependent on the *form* of the voting institution. Second, maybe their results are sensitive to the inclusion of some of our control variables, like population, agriculture and common law. Third, maybe correlation should not be interpreted as causation. Maybe electoral rules and other democratic institutions hardly affect public policy but—like the policies themselves—are determined by basic conflicts such as efficiency (as in Weingast and Marshall, 1988) and interest group politics (as argued by Rokkan, 1970; Alesina, Glaeser and Sacerdote, 2001).

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