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POLITICS AND ECONOMIC POLICY

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authoritarian regimes. I will finally confine myself to positive analysis of policy choice, largely ignoring some interesting work on normative questions of institution design.
The short survey format does not permit deriving, or even hinting at how to derive, results. For details, I refer the reader to the original articles and research papers. A textbook–type introduction to many of the issues can also be found in Persson and Tabellini (1990).

I have grouped the survey into three sections, labeled Politics and Stabilization Policy, Politics and Fiscal Policy, and Politics and Economic Growth. In each section, I describe a few areas of research. In each of these areas, I describe the central questions posed in the literature and try to give an intuitive feel for the results. A concluding section mentions unresolved issues and speculates on where the literature may be going next.

II. Politics and Stabilization Policy

One broad topic in the literature is the determinants of traditional stabilization policies, by which I mean policies that mainly aim at managing aggregate demand through monetary or fiscal measures. Methodologically, the work in this area borrows from several other strands of work. To model the economic choices of individuals, it borrows from the literature on modern macroeconomics with rational expectations. To model the political choices of individuals, as well as the political institutions, it borrows from the political–science literature on two–candidate elections. And to model the choices of policy makers, it borrows from modern non–cooperative game–theory, particularly from the literature on repeated games.

What I want to focus on in this section is a literature which has tried to analyze the general idea that elections and possible political change may induce cycles in policy instruments and in macroeconomic outcomes.[1] This work falls into two categories, depending on which type of mechanism they stress. I deal with these in turn.

The *political business cycle* refers to the common idea of an expansion of the macro economy by the incumbent government just before elections. A first generation of political business–cycle models appeared in the seventies, following the papers by Nordhaus (1975) and Lindbeck (1976). This work started from two basic premises: politicians are office–motivated and governments are more popular when the economy is doing well. And its conclusion was that incumbents who want to get reelected, expand the economy before elections. If we insist on people making rational and forward–looking economic and political decisions, this first generation of models is a bit suspect, however, in that people make backward–looking economic and political choices and essentially get "fooled" every time there is an election.

Is forward–looking individual behavior consistent with an equilibrium outcome that includes a pre–electoral expansion? A positive answer really has to resolve three puzzles. The first one is why the incumbent's behavior before the election would influence forward–looking voters' decisions of whom to appoint to conduct future policy. The answer may be that current performance says something about prospective future performance. Such would be case if the outcome voters cared about depended on the policymakers competency and competency was serially correlated. The second puzzle is how expansionary policies before elections could expand the economy if individuals base their price and wage setting behavior on forward–looking expectations. The answer may be that incumbent governments differ in the policy they choose and that individuals are incompletely informed about the governments type. This type *cum* policy uncertainty would create some room for expansion even under rational expectations. The third puzzle then is why different office–motivated incumbents would choose different policies before

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1 More extensive surveys of this literature can be found in Alesina (1988b) and Nordhaus (1989).

elections and why at least some of them would choose to expand the economy, rather than some different policy stance. The answer here may be that if different candidates differ in their competency and voters are incompletely informed of their type, an expansionary policy may be a credible way for a competent type to signal his type to the voters.

This is basically the story in the second generation of political business–cycle models. It is related to earlier political–science work by Ferejohn (1986), but was worked out in the papers by Rogoff and Sibert (1988) and Rogoff (1990). Although these papers actually dealt with budget policy, Persson and Tabellini (1990, Ch. 5) showed how a similar argument could be extended to stabilization policy.

The literature on political business cycles stresses the similarities between political candidates and the role of elections in monitoring incumbent performance. The literature on *partisan cycles*, on the other hand, instead stresses the differences between political candidates and the role of elections in matching the incumbent to the majority's preferences. The idea of a partisan cycle rests on the notion that different parties, because of ideology or other reasons, have different preferences over macroeconomic objectives and will therefore choose different policies. Formal models based on this common–sensical idea were formulated and tested in the seventies, following the work by Hibbs (1977). According to this kind of model, changes in the party holding office lead to systematic policy shifts which trigger swings in macroeconomic variables, such as inflation, production growth and unemployment. Like the early political business–cycle models, these early partisan–cycle models rested on individuals making economic choices based on backward–looking expectations. And again it was this assumption that allowed systematic differences in the aggregate demand management—in this case between parties rather than between pre–election and post–election time—to exert systematic effects on real economic outcomes.

The next generation of partisan–cycle models, which appeared about a decade later in the work of Alesina (1987 a and b), incorporated forward–looking rational expectations.

With this modification systematically different policy preferences of parties can no longer lead to permanent differences in real macroeconomic outcomes. But different policy choices will still lead to permanent inflation differences. Such inflation differences may result, not only from different parties having different inflation targets. They may also result from different employment targets or from different relative weights on inflation and employment targets, as in the growing literature on credibility problems in monetary policy.[2] Drawing on the insights from this literature, Alesina was also able to offer an explanation—the discretionary nature of policy making—for why parties may not converge in their policy positions, as they would in the traditional political science literature which assumes that parties can commit to policy platforms.

A temporary partisan cycle in real variables is still possible, even with rational expectations, but then it will be limited to the period immediately after elections. Before an election, long–term nominal contracts for prices and wages may be signed under uncertainty about which party will win the elections and therefore about which policy will be pursued. Because of this price–wage inertia, an expansionary policy pursued by a victorious "leftist" party may cause a boom in production and employment, while a contractionary policy pursued by a victorious "rightist" party may cause a slump in these variables. But the effects are temporary and die out with the signing of new contracts, which correctly anticipate the policy followed by the newly elected government.

The different versions of the political business–cycle model and the partisan–cycle model each generate a number of testable hypotheses about how macroeconomic outcomes and policy instruments should relate to elections or to the identity of the incumbent party. What is the empirical support for these hypotheses? One problem with testing is that the number of observations on political events like elections and changes in government are quite few in any given country. The results of tests based on country–specific data are

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2 This literature is surveyed at length in Persson and Tabellini (1990).

therefore typically quite weak.

A more promising approach for investigating whether the predictions form theories on political cycles hold up may be to combine political and economic data from a number of countries, while controlling for common and country–specific factors. The recent studies by Alesina and Roubini (1992) and by Alesina, Cohen and Roubini (1991) relies precisely on such an approach: they use time series on political and economic data for the democratic OECD–countries during the post–war period. The following discussion is largely based on their results.

Broadly speaking, the second–generation partisan–cycle model is consistent with these data. Government change seems to yield significant effects on GDP–growth and unemployment: *ceteris paribus*, a new leftist government is associated with an expansion, a new rightist government with a recession. But, as the theory would predict, these effects are only temporary and die out about a year after the elections. The inflation outcome with different parties in power also correspond to the model's predictions, in that inflation rates are significantly higher under leftist than under rightist governments. Further, the results are strongest for those countries that have two–party systems or permanent coalitions of a two–party character.

The political business cycle has some—but not as strong—support in the data. One doesn't find any significant systematic effects on real variables around elections. But the inflation rate has a tendency to increase immediately after elections, which could be interpreted as the delayed effect of a pre–electoral expansion. That interpretation gets some support in the empirical results regarding policy instruments: both budget deficits and money growth rates are systematically higher in election years.

The results for both types of cycles are statistically significant only when data are combined for the OECD–countries. Thus it seems as if political cycles are important phenomena in the OECD–area, but they do not dominate the data and they do not occur in connection with every election and in every country.

The work on political cycles I have been describing is very rudimentary when it comes to modeling the details of political institutions. Theoretical work by Alesina and Rosenthal (1991) attempts to incorporate more detail of US institutions, in particular how the majority can exploit the interplay between presidential and congressional elections to moderate policy outcomes. Empirical work by Alesina, Londregan and Rosenthal (1991) demonstrate how a model, which incorporates such institutional detail and the two types of political cycles discussed above, can go a long way towards explaining election results as well as macroeconomic outcomes in US.

III. Politics and Fiscal Policy

Another broad topic in the literature is how economic and political factors shape fiscal policies like government spending on goods, government transfer payments, and the distribution of taxation across broad tax bases and across time. The methodology in this work borrows as much from public finance as from macroeconomics. A typical approach would be to formulate a simple dynamic general equilibrium model which incorporates the root to political conflict, because agents have different policy preferences due to some heterogeneity in their (primitive) preferences or in their endowments. The setup would look very much like a standard model of optimal taxation, except that policy is not chosen by a hypothetical benevolent planner, but instead determined by majority rule, as in the earlier work by Romer (1975), Roberts (1977) and Meltzer and Richard (1981). Sometimes majorities shift over time, in which case we get a dynamic policy game, where equilibrium policy choices of future prospective majorities constitute binding incentive constraints for the current majority.

The first issue I want to discuss within this broad topic of "positive public finance" has to do with how *political instability* may lead policy myopia or, more generally, to intertemporal policy distortions. Take the case where different prospective majorities disagree either over the desired size or the desired composition of government spending. Persson and Svensson (1989) and Tabellini and Alesina (1990) showed how such policy conflicts may lead to excess budget deficits, or departures from budget balance, which tilt the intertemporal profile of taxes and of utility away from its efficient path. The general idea here is that the incumbent policymaker fully internalizes the present benefits (costs) of his current policy. But he does not internalize fully the future costs (benefits) of his current policy, because somebody with quite different policy preferences may be making policy choices in the future. This introduces a shortsightedness in current policy choices. The resulting intertemporal distortion will be higher when there is more political instability, either in the form of a higher probability of the incumbent losing power, or in the form of a very polarized political situation, so that a shift in power leads to large prospective shift in policy.

Another way to understand the idea is to think of current policy affecting some state variable that enters into the policy problem of future governments. An incumbent policymaker may therefore choose current policy *strategically*, trading off an intertemporal distortion against the control of future policy. Stated in such a way, we see that this kind of argument has several applications. Cukierman, Edwards and Tabellini (1991) used a similar idea to explain why politically unstable countries may overuse the inflation tax. They also show that their hypothesis is not rejected by cross–country data.

Other empirical results also tend to support the general theoretical idea, at least indirectly. Cross–country studies of industrialized countries by Roubini and Sachs (1989) and by Grilli, Masciandro and Tabellini (1991) confirm that permanent budget deficits tend to arise in countries that are politically unstable, in the sense of having short–lived governments. However, this finding is also consistent with some recent theories of resistance to policy reform (see further below). Roubini's (1990) cross–country study of developing countries again confirms that budget deficits tend to be higher in politically unstable countries.

The above studies emphasize the "tax smoothing" aspect of government deficits. But, in the absence of Ricardian Equivalence, government deficits may also redistribute income across generations, as may other government financial policies, such as pay–as–you go social security. This scope for *intergenerational redistribution* plus the limited commitment capacity of governments poses to two interesting and challenging questions. Why does the young generation honor their obligations to the old, which they could potentially repeal at any point in time? And if they are going to honor them, why are they not exploited fully by the old?

Partial political equilibrium answers to these questions were provided by Cukierman and Meltzer (1989) and by Rotemberg (1990). Tabellini (1991a) suggested the following answer to both questions: Suppose there is altruism within families and heterogeneity across families. Then public debt redistributes not only between individuals in different generations, but also between families in the same generation. And you may get an equilibrium debt policy supported by a coalition, which consists of a majority of the old and a minority of the young in poor families. Tabellini (1991b) also found some empirical support for a similar political model in a cross–country study of social security.

In Tabellini's model the electorate choose the public debt by "direct democracy" in a simple median–voter model. In the model of public debt suggested by Aghion and Bolton (1991) the electorate instead vote for either of two parties with given preferences and the winning party sets policy. The two models are closely related in that they share a very interesting feature: today's public debt issue affects the future political equilibrium by altering the future distribution of policy preferences, potentially creating a "constituency for repayment". In a similar vein, Roland and Verdier (1991) discuss how a wide–spread privatization of state assets in the former communist countries may help induce faster reform, by reducing the risk for government appropriation of the future returns to current investment.

Other writers have also been emphasizing how political forces may shape the pace and success of large–scale *structural reform.* Dewatripont and Roland (1992) develop a model, where a large sectoral shift in the labor force is privately costly but socially beneficial and where reform must be politically acceptable. It is possible to enact full–scale reform right away by massive subsidies to mobility. Such a "big–bang" approach may become very costly, however, if individuals are heterogeneous and have private information about their outside opportunities or their mobility costs. The equilibrium outcome may instead be a "gradualist" approach, because it avoids large rent dissipation to the people with the best opportunities or the lowest mobility costs. Interestingly, a government with agenda–setting power may enforce an impopular reform by a credible threat that some members of the current majority who oppose the government's proposal will be made worse off in the absence of current reform because of policy in the future political equilibrium.

The key assumption in this argument is that private individuals know how well they will do in the post–reform economic equilibrium, but that the government cannot identify the winners and losers. Fernandez and Rodrik (1991) argue convincingly that when it comes to large–scale structural policy reform, many individuals may not know themselves whether they will belong to the winners or to the losers. They then demonstrate that such individual uncertainty may block majority support for socially beneficial reform. In that sense, the political equilibrium tends to support the *status quo.*

A different argument leading to a similar conclusion has been made by Alesina and Drazen (1991). Discussing policy reform in high–inflation economies, they ask the basic question why the road towards adopting a serious fiscal stabilization—which is necessary to stop fueling inflation by printing money—always seems to be so long and protracted. Their point is that political conflict between strong interest groups in society over who will bear the costs of the necessary stabilization provides a major obstacle to the necessary reversion in policy. Formally, they adopt a dynamic–game approach, modeling this political struggle as a war of attrition under incomplete information. A second basic question is under what circumstances such stabilization programs will indeed be successful. Applying a similar methodology, Drazen and Grilli (1991) show that "crises" may be beneficial. And Guidotti and Vegh (1992) take on the hard task of integrating this kind of political model with earlier work on how a forward–looking private sector would rationally respond to exogenous policies in a stabilization program. Whether the stabilization program will succeed essentially depends on a race between the unfolding political struggle, the building up of a balance–of–payments crisis, and the decline in the momentum of inflation inertia.

Another reason why political forces may tend to preserve the status quo is suggested by Persson and Tabellini (1992), who discuss the effects on fiscal policy of the present integration in Europe and, in particular, of the increasing mobility of capital and goods. In their model of redistributive taxation, the equilibrium response of governments with *fixed* policy preferences to higher international mobility of their tax bases is indeed to lower the tax rates, as in the literature on tax competition among local jurisdictions. But they show that the higher mobility may also trigger changes in the political equilibrium, either by altering the government appointed by the existing majority, or by altering the existing majority itself. And in both cases the full politico–economic equilibrium adjustment tends to preserve the status quo in tax rates.[3]

The last issue I want to deal with in this section is how *political institutions* affect *economic efficiency*. For instance, a recent paper by Rogoff (1991) discusses the efficiency properties of different electoral rules. The more specific question revolves around how often elections are held? Having elections more often may be costly for society, by inducing more electoral cycles or more policy myopia, as in the discussion above. But having more frequent election may also be beneficial: in particular, it may improve the monitoring function of elections, which is to provide a relatively cheap legal possibility of

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[3] I am ignoring here the political science literature, discussed by Rosenthal (1992), which shows how particular institutional details in the political system may serve to preserve the *status quo* point.

throwing out an useless government. (Witness how costly the lack of such a mechanism has been for many non–democratic societies.) So there is a trade off. And there may be similar trade offs, when it comes to other aspects of the electoral rules, such as possibility of calling early elections, having votes of no confidence, etc.

Much of work I have described, so far, rhymes with what I take to be the predominant message from the traditional public choice literature: political incentives always exert a "bad" influence on policymakers and promote all sorts of distortions. Recent theoretical work by Persson and Tabellini (1991a) shows that message may be a bit too simplistic. They formulate a model of capital taxation, where the classical capital–levy problem is present. The paper shows how a realistic political institution, namely representative democracy, allows society to resolve the capital levy problem by delegation: delegating the power of taxation to representatives who have particular interest in shielding capital income may wipe out the temptation to tax already accumulated capital. In the model, such delegation is not only possible, but the equilibrium outcome with majority rule. Another way to understand the idea is to recall "the theory of the second best". If we are in a situation where policy is affected by a binding incentive constraint—here deriving from the temptation to tax capital *ex post*—adding other incentive constraints—here deriving from the conflicting interests over the structure of taxation—may actually help foster a better outcome.

The most interesting potential for empirical work on this topic may lie in studying the history of political institutions. I may mention here an interesting research program pursued by different people at Stanford University, which partly relies on the hypothesis that institutional reform often arises endogenously so as to promote economic efficiency. A recent paper by North and Weingast (1989) on institutional reform in 17th century England is particularly relevant for the above discussion of delegation in policy. They describe how the political institutions were reformed such that the power over taxation and management of the public debt were delegated from the crown to the parliament. And they argue that this reform took place precisely to resolve an incentive problem, which derived from the discretionary powers of the King to effectively default on the outstanding public debt.

IV. Politics and Economic Growth

In the literature on economic history and economic development, social institutions and political factors have always been highlighted as very important for determining a country's growth performance. But this literature has really lacked a formal language for addressing these issues in a precise way. A very recent strand of work tries to develop a more systematic approach for analyzing which role political factors may play in promoting or hindering growth. In doing so it marries together insights derived from the literature on equilibrium policy choice—particularly the literature discussed in Section II of this paper—with insights derived from the literature on "endogenous growth".

A first set of papers deals with the relation between *income distribution and growth.*[4] A common theme in these papers is that incomplete property rights and a struggle for income shares may depress investment and thereby growth. The paper by Benhabib and Rustichini (1991) uses the theory of dynamic games to analyze the struggle over income in a society without well–established property rights. They show how this struggle may impose binding incentive constraints, which may drive the equilibrium away from the economy's first–best growth path. This may happen either at high levels of development, so that the economy stops growing, or at low levels of development, so that the economy is caught in a "growth trap".

Other papers analyze similar mechanisms, but within more specific models of policy

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4 A more extensive survey of these papers is provided by Persson and Tabellini (1991c).

formation. The general idea is that an unequal distribution of income may produce policies which entail negative incentive effects on productive accumulation. The simplest example would be redistributive taxation of the returns to physical or human capital accumulation. But other examples may be regulatory policies that fail to protect the returns to knowledge creation, or, even more broadly, crime enforcement and general protection of private property rights. Along these lines, Persson and Tabellini (1991b) formulates an overlapping–generations model where the median voter in the franchised segment of each generation is pivotal in a political equilibrium which determines redistributive policy. They derive the result that more inequality in the *size* distribution of income is harmful for growth. But political institutions also matter: a restricted franchise may be good for growth, at least in the short run. Alesina and Rodrik (1991) and Bertola (1991) derive a similar result regarding the link between inequality in the *functional* distribution of income and growth.

This literature is not only theoretical, but also attempts to confront the predictions with data. Persson and Tabellini find the predicted negative effect of inequality on growth in both in historical data and in post–war cross–country data. The link between inequality and growth is statistically significant and robust to various statistical problems. The result also seems quantitatively important: one standard deviation more inequality implies a decrease in the annual average growth rate of about half a percentage point, in both samples. Alesina and Rodrik find similar results for the post–war period, based on a somewhat different data set.

A closely related literature analyzes specifically the interaction between income distribution, redistributive policies and *human capital formation* over time. Perotti (1990) studies a model with redistributive taxation and indivisibilities in the costs of obtaining an education. Fernandez and Rogerson (1991) study a model with similar indivisibilities, but where redistribution is tied to educational subsidies, so that obtaining an education works like an "entry ticket" to the redistribution game. St Paul and Verdier (1991) study a model with a similar entry ticket but without indivisibilities. In general, these papers show that the link between income distribution and human capital formation is not so simple. In particular, there are some situations when the politico–economic equilibrium may generate more redistribution and more accumulation with an unequal distribution of income. For example, this may happen in societies with a low average level of income, where it is only the richest segment of the population that can possibly afford an extensive education.

Another important question is the relation between *political instability and growth.* It is easy to imagine how this can be a two–way relation. The work on political instability and policy myopia that I referred to in Section II suggests how political instability may cause short–sighted policies that are harmful for growth. That there could be a link from bad growth performance to political instability is also easy to imagine, but this link may be hard to model formally, at least if we think about coups and other forms of violent government overthrows in non–democratic countries.

I know of no convincing theoretical work on these issues. But there is some empirical work that suggests that the two–way relation is indeed present in the data. Londregan and Poole (1990) study cross–country panel data on income growth and coups d'etat. Their results tend to confirm that there is a joint dependence with the expected signs. Alesina, Ősler, Roubini and Swagel (1991) look at a more extensive data set, where they lump together all types of government overthrows, democratic as well as non–democratic. Again they find a statistically significant two–way relation where bad growth increases the likelihood of political instability and political instability increases the likelihood of bad growth.

V. Conclusion

I have given a selective survey of some recent work on politics and economic policy. I believe it is fair to say that this work is generating increasing interest among mainstream neoclassical economists. There is probably two reasons for this. One reason is that we now know how to write down relatively complete models of politico–economic equilibrium that make assumptions at the usual level of endowments, preferences, technologies and institutions. The other reason is that there is now an increasing body of empirical work which suggests that political factors may be important in practice.

The types of policy issues I have dealt with are "macroeconomic" in one specific sense: the costs and benefits of different policies are relatively symmetrically spread over the population as a whole. We have some relatively simple "work–horse" models—essentially voting models—of the political mechanism that may aggregate conflicting interests in those policy situations. There are other areas of policy formation, where political factors are probably equally important, but where the conflicting interests are much more asymmetric: the benefits are concentrated to individuals in a particular industry or geographic location while the costs are very dispersed. Despite some promising attempts, we don't really have any simple work–horse model with general appeal to address such "sectoral" trade, industrial or regional policies. Developing a general framework for thinking about policy formation in these areas would have a high pay–off.

In terms of ongoing research, I personally find two areas particularly exciting. One area worth thinking more about is how different forms of political institutions shape the incentives for policymakers. Can we link economic efficiency to different electoral systems, to different forms of political accountability, or to the division of labor between different branches of government. Here, I believe that much can be learnt by drawing on the insights from the literatures on contract theory and on mechanism design. The other area worth thinking more about is how political factors interact with the growth process. We still don't know very much about the dynamic interaction between income distribution, political equilibrium and growth. Although, this is an area where many people are now working and making progress, much more remains to be done.

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