The ‘New Political Economy’: Two Recent Books[1]

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Abstract

This paper reviews two recent books on Political Economy by Allan Drazen and Torsten Persson and Guido Tabellini. It discusses some problems of the recent Political Economy literature.

The last …fteen years have seen an explosion of contributions in the …eld of political economy. This literature is characterized by two features. First, it chie‡y aims at explaining actual economic policies, rather than taking it as exogenous, as do "conventional economics". Second, it departs from the assumption often made in conventional economics that policy is determined by maximizing a social welfare function. It explicitly takes into account that policy is determined by a political mechanism, and therefore will re‡ect the interests of the most powerful groups in society. It has tackled a wide variety of topics, such as the determination of redistributive taxation, in‡ation, budget de…cits, school …nance, labor market policies, capital taxation, trade liberalization, privatization and restructuring in transition economies, and so on. It typically generates predictions about how policies that are actually pursued will depend on the distribution of agent's incomes and endowments, and on political institutions.

Given the size of the literature, it has become crucial to organize it and evaluate what has been learned. Two new books, Political Economy in Macroeconomics by Allan Drazen, and Political Economy: Explaining Economic Policy by Torsten Persson and Guido Tabellini, achieve this goal. Each is best considered as a textbook, and while they cover many common topics, their focus is di¤erent. Drazen mostly focuses on applications for macroeconomic policy, while Persson and Tabellini pay more attention to the institutional details of decision-making. Both books are very well written and concise, and I highly recommend them to any student interested in political economy, while they will greatly simplify the task of any professor teaching that topic.

The book by Drazen deals with most of the aspects a macroeconomist would like to know about. It is mostly theoretical in the sense that it presents a variety of models in detail, but for each topic it systematically reviews the empirical literature. It has fourteen chapters, themselves aggregated into four parts. It starts by introducing some basic tools of economics (overlapping generations, principal-agent theory, optimization) and politics (median voter theorem, Downsian competition, lobbying, etc.). While the economic tools are analyzed explicitly, and the corresponding models are solved for, the discussion of political economy tools is much more cursory. This re‡ects the book's emphasis on economic outcomes rather than political institutions. The next part of the book reviews the large literature on credibility, commitment, and reputation. A large chapter then discusses the role of elections, and is mostly devoted to the analysis of political business cycle (in a broad sense) models. This includes the old and more recent literature on the synchronization between the timing of policy and the timing of elections–i.e. Nordhaus's (1975) opportunistic PBC model, Hibbs's (1977) partisan PBC, and Alesina's (1987) rational partisan PBC; and models of rational prospective voting where the incumbent government stimulates the economy for strategic reasons, whether to signal its type as in Rogoff and Siebert (1988), or to tie the next government's hands as in Persson and Svensson (1989) and Alesina and Tabellini (1990). The next chapter is devoted to redistribution, where the literature can be split between median-voter based explanations (as exemplified by Meltzer and Richard (1981)), and models where redistribution benefits special interest groups (as in Weingast et al. (1981)). The author then deals with public goods, mostly focusing on the free rider problem in public good provision. The next chapter, Chapter 10, analyzes problems of inaction, delay, and crises, that are especially relevant to issues of stabilization in developing countries. It asks how uncertainty and vested interests may lead to delay in policy decisions, and what role crises may play in triggering reform. These are fascinating, but complex topics, in particular because such notions as "uncertainty" or "crisis" may encompass a variety of very different phenomena. Chapter 11 deals with the most studied topic of factor accumulation and growth, where there is conflict of interest over policies that affect the economy's long-run growth rate; many of these models generate predictions about how the growth rate depends on the distribution of income and the extent of democratization. Chapter 12 is entirely devoted to international aspects, and there only macroeconomic issues are discussed; for example, the political economy of trade restrictions is not discussed. The role of exchange rate arrangements is first discussed, with applications to pegging, currency unions, and balance of payments crises. The author then turns to international coordination of monetary policies, and finally discusses international capital flows. As the chapter makes clear, most of this literature is political economy only in a "weak" sense. While one often gets interesting implications about which institutional arrangement is optimal, and which monetary policy will be pursued, few of these models relate outcomes to con‡icts of interests and political institutions. Many important directions of research are still far from completely explored. For example, how do interest groups a¤ect the allocation of foreign capital, and how does that a¤ect the likelihood and anatomy of balance-of-payment crises? Chapter 13 deals with the political economy of reform and transition. This literature has largely been motivated by the reform process in former Communist countries. It has made important insights into how political constraints may shape the timing and design of economic reforms. Finally, the last chapter reviews the literature about the size of government, and the more ambitious one about the frontier of nations. These two literatures are gathered in the same chapter because they insist on …scal con‡icts and institutions as a key determinant of the size of governments and nations. This emphasis has led to quite valuable empirical predictions, but one may feel that it is somewhat excessive. One might like to see more work on the determinants of the scope of governments, which has considerably evolved along history (some literature is brie‡y reviewed in the chapter), while cultural and historical issues surely play a big role in the determination of nations—and economic theory may well o¤er some insights about these aspects as well.

The book by Persson and Tabellini has 19 chapters grouped in four parts, and it starts with a very useful presentation of the various tools that can be used in order to analyze the political process. These tools allow to go beyond the standard median voter theorem, and to capture phenomena that cannot be understood with that basic result. What is very nice is that the models presented are all as simple and easy to use as the median voter theorem. That is, they are equally operational for the economist who is interested in generating predictions about actual outcomes. Hence, in Chapters 2 and 3, the reader is shown how single-crossing preferences may be used instead of single-peaked ones get a Condorcet winner—a very useful property that arises quite naturally in a variety of models; how intermediate preferences may be used to increase the dimension of the policy space; how probabilistic voting may be obtained by assuming a stochastic, speci…c taste for a given party—with the fundamental prediction that groups who care less have a more elastic voting behavior, and are therefore more likely to be favored by policy; and how one can also introduce campaign contributions by organized lobbies. Clearly, all these approaches only apply to problems speci…ed in an adequate way; otherwise, the basic impossibility results appear again. But they are nevertheless extremely useful. In chapters 4 and 5, the authors present models that are meant to represent more accurately some real-world phenomena such as legislative bargaining , agenda-setting, etc. These models are then fully used in Part III, "Comparative Politics", which studies how di¤erent political constitutions a¤ect economic outcomes such as taxes, the provision of public goods, transfers to speci…c groups, and dissipation of government receipts in the form of politicans' rents. In that part, of particular interest is Chapter 10, which analyzes the strength and weaknesses of presidential vs. parliamentary regimes. Before that, Part II deals with redistributive politics, which is split between general interest politics and special interest politics. While the distinction between the two is not so clear to me (in a democracy of self-interested citizens, all politics is about special interests), in Persson and Tabellini this refers to the di¤erence between models where all voters directly vote on policy and models where it is determined by more complex procedures and by representatives rather than the people themselves. In that part the material covered can also be found in Drazen. The authors deal with redistribution, pensions, regional transfers, and unemployment bene…ts, this latter topic not being covered by Drazen. Finally, part IV and V overlap a lot with Drazen. They deal with similar macroeconomic issues: public debt determination, growth, political business cycles, capital taxation, credibility and time consistency of monetary policy, and international policy coordination. The last chapter, Chapter 19, is a short put penetrating and honest assessment of what has been achieved and suggests many directions of further research.

If one criticism can be made with respect to the contents of both books, it is that given their size (763 pages and 627 pages), they could have dispensed with dealing with the traditional literature on credibility and time consistency, which is already well reviewed in most macroeconomics textbooks and in some excellent surveys such as Cukierman (1992), for example. Furthermore, this literature has achieved little in the last ten years and it is not obvious to me whether it …ts in a political economy text. True, policy is endogenous, and often ine¢cient; but there are no con‡icts of interest. More generally, it could be useful to distinguish agency problems, that are well understood and associated with traditional issues of commitment, observability, and incentives, from "true political economy" where conflicts of interests and political institutions play a key role.

Both books are well representative of the considerable progress made by the field in the last decade of the 20th century; they are also very successful at pointing out new research directions, and at highlighting the main shortcomings of the approach. I now want to discuss these shortcomings and illustrate them with examples drawn from both books.

The first problem, although some may think otherwise, is that political economy pushes the rationality assumption even farther than economics. It follows the established practice of economics of assuming rational expectations, but that assumption is much more questionable in the case of political economy.

In a macroeconomic model, for the rational expectations assumption to be valid, it is only necessary that agents understand the determinants of the variables of interest in the equilibrium in which the economy happens to be. For example, the functioning of the economy may be quite complicated, but if its dynamics, in a reduced form, are linear and first-order, to forecast prices it is only necessary to know the coefficients describing how prices at t depend on the vector of state variables at $t - 1$: Admittedly, this is already asking for a lot, particularly if one introduces nonlinearities or assumes that the economy is away from its stochastic steady state.

However, when one uses rational expectations in political economy models, one goes one step further. In order to be able to compute their gains from a policy change, agents must fully understand how such a policy change affects the behavior of the economy. In particular, they need to fully take into account general equilibrium effects. If such policy changes were taking place randomly and regularly just like other shocks, one would just need to know, again, their reduced form effects on the variables of interest. This is plausible in some cases, for example when one is considering common policy shifts such as a tax cut or a rise in interest rates.

But in most cases, this is clearly not the case. Policy changes do not occur frequently and are the outcome of a constantly evolving set of ideologies. These ideologies themselves reflect the evolution of knowledge about how the economy works. Proposals that were common recipes thirty years ago are no longer on the agenda, and vice-versa. Therefore, many reforms are unique and in many cases they are the response to a crisis which is itself unique. In such cases, to properly evaluate the effect of the reform one would need a complete, structural, general equilibrium model of the economy, with a confident knowledge that the model will work in an environment that never prevailed in the past. While this is doable for the virtual agents and the simple economies of our models, in practice this is pushing rationality very far. In fact, many political conflicts are not only conflicts of interest but also conflicts about how the economy functions.

For example, the proposal to cure unemployment via working time reduction, which is so popular in some European countries, reflects the belief of many politicians that total hours worked are fixed, or at least sluggish, and that there are no prospects of job creation in new sectors. Despite its theoretical shortcomings and all the evidence to the contrary, it is too easy to dismiss this argument as a cynical excuse for a measure that redistributes in favor of insiders. It is actually connected to an intellectual tradition which regards aggregate quantities as quite unresponsive to prices, and considers that wage moderation has little impact on employment. This tradition is discredited in Anglo-Saxon countries but still quite influential in many French circles.

Another example of the same sort is pointed out by Piketty (1995), who argues that people favor or not redistribution depending on their belief on its distortionary effects. These beliefs are formed on the basis of one's own experience. Thus, a "right-winger" is not a rich person but somebody who experienced upward mobility as the result of his effort, while a "left-winger" could be quite rich as the outcome of raw luck, and hence believe that income bears little relation to effort, so that redistribution is not very distortionary. In that paper, conflicts are entirely due to different beliefs about how the economy works. But the paper also illustrates how difficult it is to make these arguments plausible in the context of a simple mathematical model. In the model, it would be very easy for individual agents to learn the true distortionary impact of taxation, at an arbitrarily small cost, by slightly varying their effort level and see what comes out. The point is that one cannot represent a phenomenon associated with the world's complexity in a model where the world is simple.

This problem is particularly salient when one is dealing with issues such as the political economy of transition. How can voters in a country that has not experienced market institutions for half a century or more figure out the effect of introducing these institutions on their welfare? One may argue that the key motivation to move from communism to capitalism was a naive comparison with the West's living standards, which ignored all issues associated with transition. This suggests one possible, simple route for thinking about how individuals with limited rationality evaluate their gains from reform, i.e. by simple comparisons with other economies.

Another way is illustrated by Drazen's discussion of the impact of unemployment benefits on the viability of reform. As argued by Aghion and Blanchard (1994), high unemployment benefits increases the political support for reform to the extent that it makes people more willing to give up their job in the public sector to find one in the new private sector. This result obtains in a simple model where general equilibrium effects are shut down. However, as Atkeson and Kehoe (1995) argue, in general equilibrium unemployment benefits may increase aggregate consumption demand by reducing the need for precautionary savings. This in turn leads to an increase in interest rates which makes people less willing to engage in investment activities, including job search. With this example, we clearly see that taking into account such general equilibrium effects assumes that agents are far more sophisticated than in Aghion and Blanchard. Indeed, my intuition would have suggested that higher unemployment benefits reduce interest rates in the short run, since they reduce employment and thus the marginal product of capital. Other general equilibrium effects of unemployment benefits may also overturn Aghion and Blanchard's results. For example, in Saint-Paul (2000, ch. 5), I give an example where the value of being unemployed serves as an outside option of incumbent employees in wage determination, so that higher unemployment benefits increase wages in such a way that the utility loss associated with losing one's job is unaffected. In such a world, the Aghion/Blanchard effect is entirely offset by general equilibrium effects, and, in fact, there is no way one can insure oneself against job loss in general equilibrium. If this is understood, no social demand for unemployment benefits will arise. Such general equilibrium effects may well be quite relevant.

For example, Cohen (1999), inspired by a recent study of Flinn (1997), finds that the present discounted value of earnings lost by a worker who becomes unemployed is higher in France than in the U.S., despite much more generous unemployment benefits in France. So, in equilibrium, American workers are actually better insured by the functioning of their labor market than their French counterpart by their generous social welfare system. The question, however, is whether they are willing to bet on that when voting on a reduction in unemployment benefits.

Another example surveyed by Drazen, also in the context of the political economy of transition, is a paper by van Wijnbergen (1992), who argues that gradual price liberalization may backlash and be abandoned by voters prior to completion. The argument is that, once prices have been partially liberalized, expectations of further increase in prices may induce speculators to store goods rather than supplying them to the market. Consumers then wrongly infer a low, or even negative, supply response to prices, which may induce them to vote in favor of abandoning the reform. This argument certainly carries much empirical relevance, but it is based on consumers being imperfectly rational. If they understood that the low supply response is only a short-run phenomenon due to speculative behavior, i.e. if they understood the economy's functioning as well as speculators, they would not draw that inference. Indeed, a brutal fall in supply would signal a very flat marginal cost schedule, and they should deduct from it that supply would rise a lot in response to a permanent increase in prices.

This discussion suggests that perhaps voters base their decision much more on the direct impact of the proposed policy on their welfare than on its general equilibrium effects, that are much more difficult to evaluate. An important step in that direction is a recent paper by Gersbach and Schniewind (1999), who considers a model of wage formation by centralized bargaining where unions take into account the direct impact of their decisions on their members but only gradually learn their general equilibrium effects once policy has been implemented. The key difficulty is to establish rigorous criteria in order to define which effects are taken into account by voters and which are not, rather than make arbitrary assumptions about them.

A second problem of the 'New Political Economy' is that theory is well ahead of measurement. One is already well aware of empirical problems in conventional economics, where one tries to relate outcomes to policies. In political economy, one wants to explain policies by more fundamental determinants such as the distribution of voters preferences and the structure of political institutions. There is very little time variation in these variables. The constitution of the United States, for example, has been amended but has never changed. The French constitution has been unchanged since 1958, the Spanish one since 1978. This forces to use cross-country studies, with the associated problems of data comparability, measurement problems, and outliers. These problems are reinforced by the fact that many variables—such as political instability, prominence of coalition governments, or the electoral system—are not easily quanti…able. One is often left with few observations and many explanatory variables being "indices" whose construction involves a lot of subjective judgement.

Although both books are mostly theoretical, they both provided detailed surveys of the empirical literature. Let us brie‡y illustrate the di¢culties of empirical research in political economy by discussing two strands of literature, one of which is a plain failure, the other a modest success.

The failure is the political economy of growth. A fairly accepted empirical fact is that greater inequality reduces growth; the political economy literature has suggested a very natural, and plausible mechanism, to explain that. The idea is that in a democracy, greater inequality is associated with greater incentives for redistributive taxation; in other words, the median becomes poorer relative to the mean. Taxation in turn is distortionary, and typically reduces the incentives for capital accumulation. Countries with higher inequality therefore grow less fast, although whether this is a true growth e¤ect or a level e¤ect ultimately depends on which assumptions are made regarding technology.

Now, this very basic story is rejected twice by the data (see Perotti, 1996). First, there is no evidence that taxation is higher in more inegalitarian countries—the typical tax champion is a very egalitarian European country, while taxes are low in high inequality countries such as Brazil. Second, more redistribution does not necessarily reduce growth.

The …rst failure looks like a clear rejection of the standard median voter theory. In fact, one may try to amend that theory to reconcile it with the data[1], by relaxing assumptions about linearity of the tax system, the usefulness of public expenditures, the localization of increases in inequality across income centiles, and the distribution of political participation. But the presumption remains that the median voter theory is probably the wrong approach to understand redistribution. Redistribution consists of a large number of programs, and the median voter is unlikely to benefit from these individual programs. It is probably better to understand these policies as targeted to those groups whose voting behavior is most elastic, in accordance with the prediction of the stochastic voting theories well summarized in Chapter 3 of Persson and Tabellini. This would probably help to explain redistribution in favor of groups such as farmers and retirees. At the same time, some redistribution benefits small groups that are able to spread the costs over society at large, as suggested by Weingast et al. (1981). Finally, some redistribution towards the poorest reflects genuine altruism or concern to alleviate negative externalities such as crime and insurrection (See e.g. Grossman (1995)). This discussion suggests a much richer set of explanations, but is likely to call for much more detailed empirical analysis than the simple prediction that "greater inequality increases redistribution". But the quality and availability of data may make that goal unreachable.

The moderate success is the modern partisan political business cycle literature. This literature predicts that left-wing governments will stimulate the economy, while right-wing ones will cool it down. It is supported by the behavior of GDP growth rates following elections in the U.S. and other countries: elections won by conservative governments are usually followed by slumps, while elections won by left-wing governments are typically followed by recessions.

However, here some problems remain. First, partisan theories have a variety of predictions, and some of them are much less supported by the data. As Drazen (p. 265) points out, these models also predict a burst of inflation after the election of a left-wing government, and the evidence is not supportive. Second, the various brands of PBC models differ in some subtle ways, but there is probably too much noise in macroeconomic data to allow us to exploit these differences in order to discriminate between models. These difficulties are nicely

[1] See e.g. Saint-Paul and Verdier (1996) for a discussion.

illustrated by Drazen's discussion of the empirical evidence (p. 260-268). For example, the basic partisan PBC models predicts a boom following a left-wing victory no matter what, while the rational partisan PBC model predicts that, since only unanticipated inflation matters, the boom is short-lived and its intensity is proportional to pre-election uncertainty about the outcome. While Alesina et al. (1997) find that this prediction is borne by the data, Hibbs (1992) disagrees. Since some specific episodes are at clear variance with this "surprise" hypothesis, one is led to suspect that the results will depend heavily on the way the surprise indicator is constructed.

Finally, another shortcoming of the new Political Economy literature is that it relies heavily on specific examples in the context of simple abstract models. This raises two issues. First, many results may be overturned by simply changing an assumption, and given the model's abstraction it is not obvious to assess which modelling choice is more plausible. The literature runs the risk of having the same fate as the theoretical IO literature, which was cynically summarized by Schmalensee (1988, p.676) in the following way: "Anything is possible"—indeed, Political Economy has borrowed many tools and metaphors from theoretical IO, such as wars of attrition, spatial competition, principal-agent models, dynamic games, and so on. Second, the mapping of the models' results to the real world phenomenon it is supposed to represent is often far from obvious. When one writes down a simple model of say, monetary policy, there is a large background of research that one can use to build the specification. Hence, for example, one may start with a simple output-inflation trade-off, and be confident that it is indeed representing the way monetary policy works. By contrast, when one uses, say, a two-period game to model the budgetary process in a "presidential system", it is far less obvious that the assumptions being made are a reasonable representation of such a process.

Let us use some specific examples borrowed from the two books under review to better illustrate these points.

Part of the literature on the political economy of economic reform is interested in "gradualism". Some of these papers argue that a gradual reform strategy may be successful where "big bang" may fail. Typically, this is because the coalition in favor of the last stages of the reform will be different from the coalition in favor of the big-bang reform. Let us consider the following example, borrowed from Wei (1997), and analyzed by Drazen in his Chapter 13. There are two equal-sized groups, A, B and C, and reform consists of 2 pieces, labelled 1 and 2. The following table gives the net payoff to each group of each bit of the reform.

	A	B	C
1	1	-1.5	1
2	1	1	-1.5

It is then argued that a big-bang reform would be turned down since both groups B and C would lose, while gradualism, i.e. voting on reform 1 first and on reform 2 next, would implement the reform.[2] Why? In the second stage groups A and B support reform 2, regardless of what happened in the first stage. Knowing that their vote does not influence the outcome in the second stage, in the first stage groups A and C support reform 1. Therefore, gradualism achieves complete reform, while big bang was unable to do so.

This example has two shortcomings. First, one can simply reverse the conclusion by changing the numbers. Suppose net gains are now given by the following table:

	A	B	C
1	-1	2.5	-1
2	-1	-1	2.5

Gradualism now clearly fails to implement reform, while big bang would pass with the support of groups B and C. So, what have we learned?

Second, the comparison between gradualism and big bang in the previous example is somewhat confusing. The result that big bang fails implicitly rests on the assumption that, if the complete reform is turned down, then the government cannot propose a partial reform in the second stage, while this option is assumed available when one considers the case of gradualism. That is, we are really considering two different models, a one-shot game and a two-period game, rather than two different strategies within the same model. If one were allowing a partial reform to be proposed once a complete reform has been turned down, and if it is known which partial reform will be proposed in the second stage,

[2] Note that the total reform increases aggregate welfare by 2 - 0.5 - 0.5 = 1.

then there is simply no substantial difference between gradualism and big bang; each leads to the same outcome.

This example illustrates how results are fragile, and also how difficult it is to map them to the real world. For real-world policy makers facing the complex problem of reforming an economy in transition, big bang may simply be inconceivable. Gradualism may simply be imposed by the need to act urgently and the scarcity of the human capital needed to put together a complex reform package. And, given that reform can be broken into a large number of pieces in a somewhat arbitrary way, gradualism means considering the impact of a large number of alternative sequencings. For n reform items, there are $n!$ alternative sequencing strategies, a number that grows very quickly with n. That is, the problem faced by policy makers is very different than suggested by models such as the above example.

Another well-known example is the argument by Fernandez and Rodrik (1991) that uncertainty about the distribution of gains and losses from reform may lead to status-quo bias. The argument is as follows. Consider a reform which yields a gain of +1 to 60 % of the voters and a loss of -1 to the remaining 40 %. This reform increases aggregate welfare, and if people knew exactly whether they gain or lose, a majority of them (60 %) would support the reform. Assume now that two thirds of the gainers, i.e. 40 % of voters, know exactly that they will gain +1, while other voters are randomly allocated between the remaining 20 % of gainers and the pool of losers. Therefore, their probability of gaining from the reform is 20 % / (20 % + 40 %) = 1/3. Then the expected gain from reform of those who do not know for sure they will win is equal to $+1 \times 1/3 + (-1) \times 2/3 = -1/3 < 0$. Clearly, their expected gain is negative, so that they oppose the reform. Since they are 60 % of the population, reform is blocked and the economy remains at the status quo.

This is a very nice example, and may be relevant to specific policy problems such as trade reform. But there is clearly no general result about uncertainty creating a bias in favor of the status quo. Going back to that example, suppose now that half of the losers know for sure that they lose 1, while the remainder of voters are again allocated randomly. Their probability of gaining is now 60 % /(60 % + 20 %) = 3/4, so that their expected gain is $+1 \times 3/4 + (-1) \times 1/4 =$

$1=2 > 0$: Uncertainty now increases the support for reform from 60 % to 80 % of the electorate.

A little bit of re‡ection suggests that uncertainty generates resistance to reform if it redistributes gains away from the decisive voter, but that the converse holds if gains are redistributed in favor of him/her. Again, we have an example where the result can easily be overturned.

Our last example will be borrowed from Chapter 10 in Persson and Tabellini. In this chapter, which builds on their earlier work, the authors have an ambitious task; namely, analyzing how various constitutional provisions for checks and balances and the division of power a¤ect economic outcomes. They are interested in understanding how congress representatives appointed by voters will set policy under alternative legislative systems. Politicians decide on the provision of a public good; they can also reward their constituency with a transfer; …nally, they can divert tax money for their own consumption purpose. Thus, policy consists of a vector of numbers summarizing all these choices. After policy is selected, a popular election takes place where voters may punish their representatives by not reelecting them.

The authors consider three systems.

In what they call "pure legislature", an agenda setter is randomly selected to propose a whole policy vector. He must bring together a minimum winning coalition in parliament for his proposal to be accepted. The members of such a coalition have themselves to bring enough bene…ts to their constituency so as to be reelected. The authors show that the agenda setter, by playing potential members of the winning coalition against each other, manages to bring transfers to constituencies other than his own down to zero. They also show that there is an underprovision of public goods and that the agenda setter is able to obtain a positive rent.

Then, they consider a "Presidential-Congressional" (PC) system, where taxes and expenditures are proposed by two separate agenda setters—labelled congressional committees— representing di¤erent constituencies, and are voted separately. This introduces a system of checks and balances: By selecting a low enough tax rate, the "tax committee" will refrain the "expenditure committee" from setting high rents for itself and high transfers for its constituency. The authors then show that politicians don't get any rents and that transfers to the constituency of the expenditure committee are lower than in a "pure legislature" system. It is still true, however, that public goods are underprovided.

Finally, the authors consider a "Parliamentary" regime, where there are again two agenda setters, now interpreted as cabinet ministers, but where voting on each item does not take place separately. Rather, a crude form of bargaining takes place where each minister can veto the other's proposal, in which case a default policy is set; if this does not take place, the joint proposal is implemented. This assumption introduces the possibility of collusion between the two agenda-setters, so that they are able to extract higher rents from the tax payer than in the previous system. The authors also show, accordingly, that taxes are higher than in the PC regime. The good side of collusion, however, is that the public good provision problem is partially solved, so that public good provision is higher, and thus closer to optimum, in the parliamentary regime than in the PC regime.

The great beauty of this model is that it …ts well with the observation that taxes as well as public goods provision are higher in parliamentary regimes (typi…ed by many European countries) than in PC regimes such as the U.S. The contribution is illustrative of the best contemporary research in theoretical comparative political economy.

Yet, it has many shortcomings. In order to reach clear-cut, testable conclusions while maintaining tractability, one has to make assumptions that are di¢cult to relate to real world constitutional features. For example, what does it mean that agenda-setters are randomly selected? In many cases the constitution speci…es how the agenda setter is selected following an election—for example, the head of state (King, president) is in charge of selecting a candidate prime minister to form a government. The assumptions about the timing are arbitrary rather than re‡ecting what is speci…ed in constitutions. It is also assumed that within each constituency voters coordinate on a punishment strategy in the next election, which is questionable. The PC regime does not account for the role of the president, which is important in countries such as France and the U.S. where he is elected directly. More generally, strictly speaking there is no government in the model, only a parliament. Finally, the bargaining which takes place in the parliamentary regime is ruled out by assumption in the PC regime, but in that case too the two policymakers would have an incentive to collude.

Another issue is that it is not straightforward to apply the proposed taxonomy to real world arrangements. How should France, for example, be classi…ed? The president is directly elected and, in addition to selecting the prime minister, has a number of executive powers. The government must have the support of a majority in parliament and proposes laws. How can this be matched to the regimes described above? This constitution has lived through di¤erent types of periods. Sometimes the president's party was clearly dominant in both government and parliament. This suggests a functioning similar to "direct legislature". Sometimes it has been less dominant, which opens the door for legislative and executive bargaining, thus making it somewhat similar to a parliamentary system. Finally, one has witnessed periods where the president was not a member of the parliamentary majority, which makes the system more similar to the "Presidential-Congressional" one. That is, the way a country should be classi…ed not only depends on its constitution but also on election outcomes.

I hope that with these examples the reader will get the ‡avor of the di¢culties associated with research in Political Economy. They should not deter him, however, from entering this exciting and promising …eld of investigation, now that he can use two excellent textbooks to learn more about it.

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