Alberto Alesina and Nouriel Roubini with Gerald D. Cohen, Political Cycles and the Macroeconomy, The MIT Press, 1997.

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In Political Cycles and the Macroeconomy, Alesina and Roubini with Cohen (*ARwC*) present the culmination of their 10+ years of research into the effects of democratic politics, i.e., primarily of central-government elections and partisanship, on macroeconomic policies and performance. The authors are among the protagonists in, and this and their previous work lie at the core of, the return of macroeconomics to its political-economic roots. That return has spawned a strong and growing research area within economics, provoked at least as much interest among political economists in political science departments, and drawn as much attention in policymaking audiences as has virtually any sub-field in either discipline. Regarding the place of this book in that field, there could be little disputing and less improving upon the endorsements on the book's cover: it...

> ...will surely become the standard reference on how the political process influences the economies of advanced industrial nations... (Howard Rosenthal, Professor of Politics, Princeton University)
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> ...clearly and convincingly explains how partisan differences, re-election motives, budgetary procedures, and central bank charters may shape monetary and fiscal policy. [They] provide a lucid survey of existing theory; but first and foremost they integrate and extend existing empirical evidence on political cycles, policies, and macroeconomic outcomes in postwar industrial democracies. [It] is destined to become a standard reference, both for students and researchers in the field. (Torsten Persson, Professor of Economics, Stockholm University)
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> A fundamental contribution that marks a huge step forward in our understanding of political business cycles. [It] is a remarkable achievement: it combines the rigor of economic theory, the originality of a comprehensive empirical analysis, a rich new data set, and a marvelous clarity of exposition. It should be on the shelves of anyone interested in political economics. (Guido Tabellini, Professor of Economics, Bocconni University)

ARwC's achievement in Political Cycles and the Macroeconomy fully deserves all this

rich praise. More than a mere compilation of their previous theoretical and empirical work, it provides a coherent survey of those theories and collection of that (and some new) evidence, which will be invaluable to believers, agnostics, and critics, even while it makes life for especially the last more difficult. In this brief review, I summarize their arguments and findings and then explore some of the remaining theoretical and empirical anomalies, in which critics may find some relief, but which I intend more as grounds for some continued agnosticism as *ARwC* and others in both fields continue to explore this exciting research agenda and as policymakers wonder how to interpret the insights from these academic efforts. None of the issues raised here much detract from the impressive overall achievement of the book, truly a modern milestone in political economics.

"This book studies how the timing of elections [... and ...] the ideological orientation of governments ... influence unemployment, economic growth, inflation, and various monetary and fiscal policy instruments [in developed capitalist democracies]" (p. 1). It contrasts models of political cycles in which politicians are motivated primarily by the desire to remain in office, caring little about the policies they enact and the outcomes those engender *per se* (*opportunistic*) with those in which politicians care about policies and outcomes directly and exhibit strong ideological differences in those preferences across parties (*partisan*), while recognizing the possibility that politicians care about both. Within each of these, it contrasts first-generation models, which relied on stable and exploitable Phillips curves and relatively naive voters (*non-rational expectations*), from subsequent iterations, which emphasize rational expectations of all economic and political actors (*rational expectations*). Using aggregate political and economic data over the postwar period from the United States separately and from many OECD countries, including the United

States, together, *ARwC* explore the historical evidence regarding these models' subtly differing predictions. They conclude that the evidence remarkably consistently (a) favors the later, rational-expectations models, (b) indicates strong partisan but little discernible election-year effects on macroeconomic outcomes, (c) suggests both election and partisan effects on macroeconomic policies, and, subsidiarily, that (d) partisan policy and outcome effects are clearer in two-party or two-bloc than in multi-party systems, (e) two-party/bloc governments adjust fiscally to deficit-inducing shocks more quickly than do coalition governments, and (f) the net economic benefits of credible delegation of monetary authority to conservative policymakers (e.g., central bank independence) are larger than one would conclude ignoring the incidence of electoral and partisan policymaking cycles. I will suggest here only that the empirical case they present is less unambiguous than they claim.

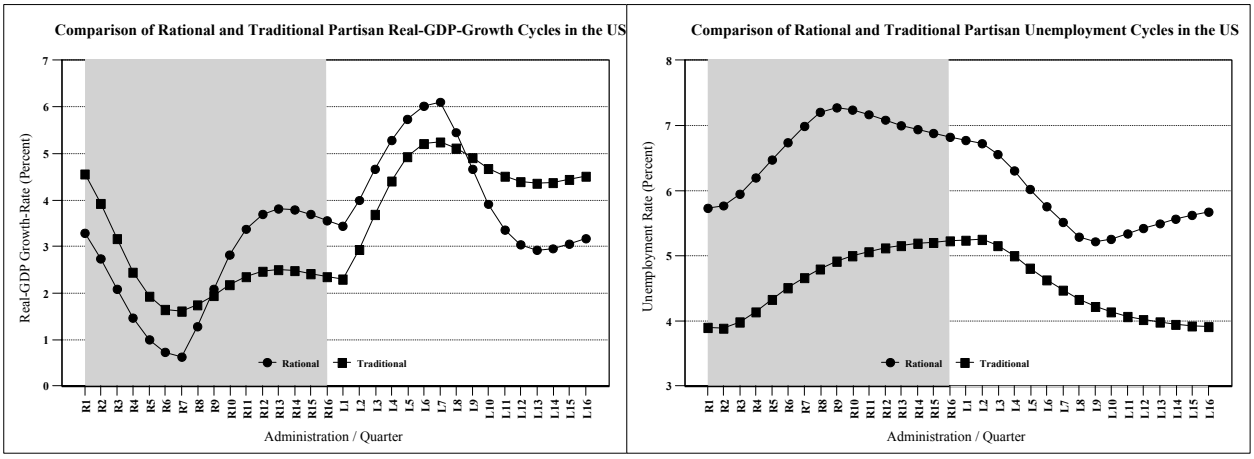
In Chapters 2 and 3, *ARwC* summarize the rational-expectations (RE) and non-RE versions of electoral- and partisan-cycle theories. In non-RE electoral theory, policymakers control policies with which they can exploit a stable Phillips curve, and voters naively and with short memory reward incumbents presiding over strong economies (high growth, low inflation and unemployment) with re-election. Democratic policymakers thus routinely attempt to time their use of fiscal and monetary policies to exploit delays from expansionary policies to their inflationary consequences to secure high growth and low inflation and unemployment before elections, with the inflationary effect arising post-election. In RE versions, Phillips curves and voters are less exploitable. Instead, policymakers exploit variations in when certain policies become clear to rational voters and private information on their own competence—say, to provide more public goods at less tax cost—to the same electoral effect. If competence is random but persists over time, voters will try to re-elect

incumbents who have recently shown competence. If voters can see some public outputs before they can evaluate their full costs, incumbents will try to signal or fake competence by providing more such goods at lower taxes before elections, delaying the inflation or other tax increases or reduced spending until after elections as the relevant information gets to voters. Thus, the implications of RE and non-RE opportunistic theory are fairly similar, although voter rationality will limit the size, consistency, and/or duration of election cycles in the RE relative to the non-RE version. In non-RE partisan theory, left policymakers target higher growth and lower unemployment and are willing to tolerate higher inflation than the right, who more-desire the opposite configuration. With exploitable Phillips curves, they use their policy control to shift economic outcomes in these directions over their term. In RE partisan theory, only unexpected monetary and fiscal policies can create such real-economic effects, so when left (right) governments are elected, to the degree this was not completely foreseen, inflation is higher (lower) and growth, employment, and inflation rise (fall). However, as time elapses, new nominal contracts expect the higher (lower) inflation, so growth and employment return to their natural rates, while inflation remains higher (lower). Thus, the primary differences in the RE and non-RE versions of partisan theory, *ARwC* claim, are whether the real effects of partisan shifts in government persist or fade over the term of the government.

In seasonally-adjusted, quarterly US data on macroeconomic outcomes from 1947:1-1993:4, they find an indicator variable equal to 1 (-1) in the first few quarters[1] of Republican (Democratic) administrations and 0 in other quarters, empirically dominates a

[1] They report results for 6 quarters and that those for 4 and 8 quarters differ little substantively. The indicators are lagged 1 quarter for growth and 2 quarters for unemployment to reflect delays in outcome responses to policies.

traditional indicator, which would equal 1 (or -1) over whole administrations. The former specification is interpreted to represent the shorter-term real effects of the surprising component of policy-moves post-election in the RE model. (Inflation, contrarily, is permanently higher under left than under right governments in both the RE and non-RE models, and the data support that as well.) The empirical dominance of the short-term dummy seems indisputable; nonetheless, *ARwC*'s strong conclusion should have come with caveats.



First, as Figure 1 demonstrates, the substantive difference in their reported results is not great. Second, more importantly, RE is not the only explanation for the shorter duration of partisan effects. As *ARwC* themselves note:

> Democratic administrations, which are expansionary in the first half, observe by midterm a significant increase in the inflation rate. Because a high inflation rate may become a significant electoral liability, Democratic administrations contract the economy so that by the election year one observes a growth slowdown and a reduction in the inflation rate. Conversely, Republican administrations that had anti-inflationary recessions in their first half pursue low inflation and accelerating growth in the second half, a combination that may give them an electoral benefit (p. 62).

In either the RE or non-RE models, the described policy-pattern[2] would produce the shorter-

[2] Neither model allows right administrations to pursue anti-inflation and growth in the second half as claimed.

term outcome-pattern. The long-noted "honeymoon" effect, which refers to the historically greater ability of new administrations to enact policy changes in their first few months than in later months, would also produce this pattern under either theory. So would any diminishing returns to stimulation and anti-inflation policies. Third, and worst of all for RE partisan theories, *ARwC* report substantively and statistically stronger real-growth partisan-cycles in the pre-1972 (Bretton Woods) than in the post-1972 period (p. 87), yet they also find that the inflation differences across right and left administrations emerged only in the *post*-1972 period (p. 90). Since the rational theory holds that the inflation surprises induced by elections cause the short-term real partisan cycles, this is suspicious.

Meanwhile, *ARwC* uncovered little to no evidence of higher growth (wrong sign, t.58) or low unemployment (correct sign, t1.15) pre-election or of higher inflation post-election (correct sign, t.31). Unfortunately, they do not report results with controls for real-supply shocks, nor do they attempt to discern pre- from post-Bretton Woods eras, as they did for the partisan theories. Also, the use of seasonally adjusted data is somewhat problematic in seeking electoral effects in the US since congressional (presidential) elections occur every other (fourth) November. Depending on the method, seasonal adjustment could therefore have 25-50% reduced the size of electoral effects.

For policies in the US, *ARwC* explore money growth, nominal interest-rates, budget deficits, and transfers. They find weak evidence of partisan differences in money growth (t1.1-1.2), though stronger in a 1949-82 sample (t1.8-2.4), and stronger evidence of partisan differences in nominal interest-rates (t2.2-3.3). They again find no indication of pre-electoral effects on monetary policy (t<.5 in all cases). Oddly, they do not report differences by exchange-rate regime and, more-oddly, lag the partisan indicator 2 quarters.

The latter is somewhat problematic because the real effects were assumed to lag 1-2 quarters, but also to arise from differences between expected and actual inflation, which could not have emerged that soon if monetary-policy changes already lag new administrations by 2 quarters. Furthermore, if Bretton Woods dampened partisan differences in monetary policy, as their inflation results suggest, then the stronger 1949-82 monetary-policy results indicate a narrow window of partisan differences in US monetary policy, only or primarily occurring in 1973-82.

For fiscal policy in the US, *ARwC* again find little evidence of pre-electoral effects in deficits (t .3) or in transfers (t .4-.7), or of partisan effects on transfers (t .7), and now find statistically significant effects of right administrations in *increasing* deficits (t 2.1). This last apparently stems solely from the Reagan and Bush administrations, regarding which they point to theories that predict right governments to increase debt to reduce future left governments' fiscal maneuverability. Early empirical indications for such theories are not promising though. Franzese (1999b) finds statistically significantly the opposite of what those theories predict, and Lambertini (1999) finds insignificance.

Thus, *ARwC* clearly establish that the real effects of partisan US administrations follow a short-term pattern illustrated in Figure 1, but the RE explanation for that short-term pattern is less fully established by this evidence than they claim. First, little substantive difference emerges in the estimated effects. Second, many other explanations for short-term patterns are at least as consistent with evidence and intuition. Third, based on their own evidence, the monetary- and fiscal-policy pattern, especially across pre- and post-Bretton Woods samples, cannot explain the outcome pattern within the RE framework. Likewise, the lack of evidence for either outcome or policy effects of US elections is weakened by the

failure to consider exchange-rate regimes, by the seasonal adjustment of the outcome data, and by the complete ignoring of congressional elections (e.g., fiscally, congress is at least as influential as the president). Moreover, others have shown that electoral effects incur where incumbents are willing to risk being caught at such cynical maneuvering, i.e., when elections are expected to be close (Schultz 1995), and to incur in the immediate pre- *and post*-election period (Franzese 1999b). The latter could reflect continuing differences between calendar-year measured electoral data and fiscal-year measured economic data, or policy-implementation momentum, or the impact of challengers, whom both RE and non-RE opportunistic theories ignore. Thus, the non-finding probably reflects as much on the simplicity of the political theory underlying the versions of electoral cycles reflected in the empirical models as on any lack of electoral effects.

ARwC's innovation in the next chapter, which follows work by Hibbs and colleagues, is more theoretically interesting. There, they acknowledge Hibbs' point that the RE partisan theory predicts that partisan effects on real outcomes should be proportional to the surprise reflected in the election outcome. Using a clever variation of option-pricing theory to measure the electoral surprise, they find the electoral-surprise measure to correlate with unemployment in monthly US data, most strongly using 24-36 month surprise measures (t 3.5-3.8). They find this conclusive for the RE version, but again one may remain agnostic. First, the longer-duration finding further diminishes the substantive difference from RE to non-RE versions. Second, *ARwC* test these surprise measures only against their absence. I.e., the alternative hypothesis is zero partisan effect. What one needed to know was whether the surprise measurement improves on the simple indicator. This cannot be discerned from the reported results because going to monthly data tripled the sample and so would have

produced higher *t* statistics under almost any circumstances. Third, the theory actually states that the electoral surprise *times the expected difference in inflation across incumbent and challenger* produces the real effects. The empirical model implicitly assumes the latter difference was equal in all US elections. This is false, of course, and produces biased estimates if, e.g., the probability the left or right wins is related to the ideological distance between them, which it should be. The direction of the bias is hard to predict, especially given the small number of presidential elections in the sample, which also suggests the impact on estimated results could have been large. Poole offers data from congressional voting records of most presidential candidates (see http://k7moa.gsia.cmu.edu/default.htm), which could be used to derive the requisite measures. Third, the complications noted above—the missing policy-links and congressional-influence and exchange-regime effects—plague this estimation also, but were not explored. Again, more-cautious conclusions may have been warranted.

The next two chapters explore partisan and electoral cycles in outcomes and monetary and fiscal (only budget policies in a broader sample of OECD democracies. They again find no evidence of pre-electoral growth or unemployment effects, although now some post-electoral inflation effects emerge, and they again find shorter-term partisan-cycle specifications dominate longer-term ones. (They also find strongest partisan effects in two-party/block countries, intuitive in any partisan model, and some indication pre-electoral manipulation of taxes and, weaker, of spending.) All of the reasons for cautious interpretation mentioned above are replicated here, plus some new ones. E.g., they find no significant partisan-effects on real interest-rates (p. 196), implying that real effects of partisan monetary-policy differences must originate in wage rigidity and differences from

expected to actual inflation. Yet, partisan differences in inflation were statistically weak and concentrated in a post-Bretton Woods/pre-EMU window, whereas partisan real-outcome differences were not. Again, policy effects consistent with producing RE partisan cycles were not found though short-term real partisan-cycles were, suggesting the jury is still out on the source of the latter.

ARwC then extend the standard theory of how central bank autonomy and conservatism (CBI) should reduce inflation biases from discretionary control at the cost of increased output variation due to the sacrificed use of monetary-stabilization policies. They show that, since CBI also mitigates partisan monetary cycles, which have destabilizing effects, the theoretically-expected correlation of CBI and output variability is ambiguous. They conclude that CBI should lower inflation, at no on-average real costs, as before, and, now, with less output-variability cost than commonly expected. Again, continued caution is warranted for several reasons. (1) They offer no evidence to support the claim that reduced political variance explains the lack of CBI correlation with output variability. (2) The cited evidence for the lack of "on-average" real effects emerges from a mere cross-section of postwar-average real outcomes on postwar-average CBI in 18-21 OECD countries, insignificance in which should hardly lead anyone to unequivocal conclusions that the true correlation is zero everywhere. (3) Most critically, the model on which this claim is based is now challenged for several strong theoretical and empirical reasons. First, the political authorities who might delegate monetary policy to conservative agents also dislike inflation, so, if they also control structural-reform policies, which could have real benefits that would lower discretionary inflation-biases, then such delegation diminishes their incentives to undertake these structural reforms and so has real effects. Second, the standard model

inconsistently assumes policymakers dislike inflation although no other economic actor does. If any sizable private actor also dislikes inflation, then CBI has real, RE-equilibrium effects on average. Third, CBI alters the real- and relative-wage effects of nominal-wage increases differently, again implying on-average real effects dependent on the structure of bargaining. Fourth, likewise, the impact of CBI on optimal nominal-settlements differs across traded and public sectors, again implying on-average real effects. Fifth, if CBI affects domestic-price inflation differently than CPI-inflation, this a relative-price and therefore real-equilibrium effect. Franzese (1999a) reviews these emerging critiques, most of which indicate that the real effects of CBI vary with labor-market institutions and structure. The available data, queried in a way that allows the real effects of CBI to depend on market institutions and structure, generally supports such critiques.[3]

Lastly, *ARwC* explore the impacts of coalition governments and government partisanship on public debt, finding the former to delay fiscal stabilization and the latter to adjust more quickly but to produce partisan fiscal cycles. Thus, they find a trade-off between too little action with low variability and too much action with high variability. These results bring fewer caveats, other than that the data also support many of the other (mostly non-competing) political-economic explanations of public-debt evolution in developed democracies, which they lightly dismiss (see Franzese 1999b).

In sum, Political Cycles and the Macroeconomy is a remarkable achievement and a valuable reference to all students of democratic political economy. It demonstrates undisputably important partisan effects on macroeconomic policies and outcomes. If it

[3] N.b., postwar-average cross-sections would miss this evidence. Contributors to this literature include, e.g., L. Calmfors, A. Sibert, T. Gylfason and A. Lindbeck, A. Cukierman and F. Lippi, A. Velasco and V. Guzzo, D. Soskice and T. Iversen, P. Hall, R. Franzese, P. Hall and R. Franzese. See the cited review for full citation of these and others.

leaves the explanation for the form of these effects less-conclusively answered than the authors acknowledge, that too is a healthy and encouraging development (for political economists). Even for electoral-cycle theory, which emerges scathed, reasons yet persist for continued hope and research. In my view, a careful reading of *ARwC* finds a field ripe for political scientists to revisit these venerable theories. The rational-expectations revolution has rekindled economists' interest in this political-economic venue and advanced the field greatly, but parallel advances in political theory have been less-fully brought to bear. Since Tufte and Hibbs, political scientists seem to have assumed the political side of electoral and partisan cycles resolved and only the incorporation of those rational-expectations advances to remain. Untrue! E.g., policymakers have many policies at their disposal, they are differently constrained in the use of those by international (e.g., exchange-rate) and domestic (e.g., government structure) institutions, and those instruments are differently effective under these and other institutions (e.g., alternative Mundel-Fleming configurations, labor-market institutions). Political scientists can and should enter the fray to offer further insights on what policies will be manipulated for electoral and partisan purposes under what sets of conditions. Kudos to *ARwC* from an admiring observer on the other side of the disciplinary divide for (hopefully) re-invigorating the discussion on both sides!

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