Book Reviews

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Barbara J. Nelson

Faithful and Fearless: Moving Feminist Protest inside the Church and Military, by Mary Fainsod Katzenstein, Princeton, NJ: Princeton University Press, 1998, 270 pp.

Gender and Family Issues in the Workplace, edited by Francine D. Blau and Ronald G. Ehrenberg, New York: Russell Sage Foundation, 1997, 301 pp.

Rarely do the general public and scholars share a wrongheaded stereotype. Often scholars feel as though they shout into the wind of public indifference with their facts and analyses. The populace believe scholars to be indifferent to "what everyone knows." But in the case of the women's movements in the United States, or more properly said, the large-scale social action and social transformation occasioned by demands for gender equity, both scholars and citizens frequently share the same stereotyped definition of activism: street protest. When street protest is visible, then social movements are active. When all is quiet, when neither police nor cameras are present, social movements are believed to be ebbing.

Part of this definitional problem is scholarly and part has to do with the need for action in the drumbeat of reporting. A scholarly and media bias in what constitutes social movement activism remains. Scholars and journalists not only promote the street-protest definition of social movement, but also ignore the male bias implicit in this definition. Specifically, if men were the main political actors and men—really white men for much of American history—have special access to public space, then public protest by men is seen to be the *sine qua non* of social movement activism. Hence whiskey rebellions, antislavery protests, labor organizing, and even civil rights strategizing are often viewed as both mostly male and the markers of "real" social movement activism, notwithstanding the complex role of women in these movements. This emphasis obscures women's efforts, in the streets and in organizations, sometimes alone and sometimes with men, to enforce temperance, gain the franchise, express their frustration about consumer costs, and gain social and economic voice for gender equity in the last quarter of the twentieth century.

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Faithful and Fearless: Moving Feminist Protest inside the Church and Military, by Mary Fainsod Katzenstein, and *Gender and Family Issues in the Workplace,* edited by Francine D. Blau and Ronald G. Ehrenberg, offer intellectually important correctives to the public protest bias in social movement research, especially about women's movements. From very different traditions both books demonstrate the importance of work and the workplace in defining the activities, and ultimately the successes and failures, of women's activism in the "Second Wave." Both books investigate the kinds of social movement activity or consequences that occur in places less open to the easy scrutiny of scholars and the press.

These are important, well-written books because they force the reader to begin with one of the most fundamental changes in American society over the last 50 years: the large percentage of women working outside the home, the increasing variety of types of work they do, and the uncertain quality of their work participation. Fainsod is to be praised for choosing to use those workplaces that are most resistant to demands for gender equity—the Catholic Church and the U.S. military. For Catholic women, the changes begun by Vatican II functioned somewhat similarly to the legal changes for military women. For both military and religious women and their male supporters, a sophisticated understanding and use of inside and outside resources were crucial for success, as was a kind of bravery that is rarely recognized. Advocates for women's equity faced horrible choices from unsympathetic institutional leaders, including demands that vowed women leave their orders, to bad conduct reports that would end a military career. Most activists had a long-term view of success in institutions, a situation that poses new questions for the study of social movements. Does the mere existence of resistance within an organization constitute the first demonstration of success? Is the creation of formal and informal free spaces to promote next steps a definition of success? Does a change in the conversation about what constitutes a problem indicate institutional change? Do a few changes in assigned task sum to sufficient changes to make a difference? To whom? When? Why?

If *Fearless and Faithful* looks mostly at individuals and groups demanding gender changes in highly resistant "industries," *Gender and Family Issues in the Workplace* asks how women can blend paid work and family obligations. The major finding of the authors in this volume (in my words, not theirs) is that policy analysts must stop talking about the female gender gap in wages without also talking about the male care gap in families. A job that expects workers to have few if any family obligations is a job for men, regardless of the incumbents of the position.

Several examples will demonstrate the point. In "Career and Family: College Women Look to the Past," Claudia Golden asks what is the evidence that white college-educated women can have both career and family. She uses several definitions of careers of varying stringency. The most stringent requires that a woman's wage fall within 25 percent of the average wages for similarly educated men, that the woman be virtually full-time in the labor force for three years, and that she have at least one child. The findings are not heartening. In the cohort of white women who graduated from college between 1966 and 1979, the white baby-boomers, only 13 to 17 percent met this definition of having a career and family by the time they were 40. (No figures for women of color were available.) In the commentary on the chapter, Myra Strober correctly asks why such a stringent earnings level was used, given that many women are in female-dominated professions such as social work and teaching, where salaries are low relative to male-dominated professions. Nonetheless, the findings show that high-level professional work and motherhood are not easy to combine. One of the reasons may be demand for the workers' time made by professional jobs. In "Work Norms and Professional Labor Markets," Renee M. Landers, James B. Rebitzer, and

Lowell J. Taylor look at the role of long hours in the legal profession, arguing that these hours are used to the detriment of family-regarding women and men as a test of firm and professional loyalty. These tests allow for shaping the gender composition of the labor force without using explicit gender-based policies.

Do parental leave policies help alleviate some of the tension between work and family? We must begin with the recognition that even when such policies cover both women and men, very few men avail themselves of parental leave. Jacob Alex Klerman and Arleen Leibowitz show in their chapter, "Labor Supply Effects of State Maternity Leave Legislation," that public policies do increase maternity leave. Jane Woldfogel, in "Working Mothers Then and Now: A Cross-Cohort Analysis of the Effects of Maternity Leave on Women's Pay," agrees with Klerman and Leibowitz about use of parental leave and suggests that for women with "good jobs" and parental leave, long-term earnings are increased by having parental leave policies.

Reviewing these books together reveals the limitations of each. Fainsod does not seek to make general statements about the effects on family life of employment within the military or the church. (She is careful, though, to recognize a wide variety of families, including lesbian families and the intentional communities of religious orders.) Similarly, the chapters in Blau and Ehrenberg rarely engage in the analysis of institutional characteristics of workplaces or industries, or of the activism that led to the changes that made women's employment more possible and equitable. Each of these books had a single objective. The challenge for understanding the workplace as a site for activism, as well as family-friendly employment, is to engage in research that looks at both questions simultaneously.

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The New Paternalism: Supervisory Approaches to Poverty, by Lawrence Mead, Washington, DC: Brookings Institution Press, 355 pp.

In the introduction to *The New Paternalism: Supervisory Approaches to Poverty*, Lawrence Mead contends that U.S. social policy is becoming more paternalistic. By this he means that social programs and policies in this country are increasingly likely to require people to behave in certain ways in order to obtain assistance (or, in some cases, avoid prison). But it is not just the directive nature of such policies that make them paternalistic; they are paternalistic because underlying them is a presumption that individuals are being directed to do what is best for them. Thus, for example, welfare-to-work programs operating under Temporary Assistance for Needy Families (TANF) are paternalistic because, after 24 months of assistance, they require recipients to engage in work activities in order to continue receiving benefits. Furthermore, underlying this requirement is the presumption that work (moreover, *any* work) is better than dependence, for both single mothers and their children.

The aim of this edited volume, according to Mead, is to "open a serious discussion of supervisory methods in antipoverty policy." Accordingly, the 10 contributors were asked to: 1) identify paternalistic programs or policies that have appeared in their respective policy areas; 2) identify underlying social or political developments that led to the appearance of paternalistic policies; 3) assess, where possible, the achievements of paternalism; and 4) project the potential of paternalism. The contributors cover a wide spectrum of the social (and, particularly, antipoverty) policy arena; there are chapters on welfare-to-work programs, homeless shelters, programs directed at teen mothers and prevention of teen pregnancy, treatment of substance abusers and the mentally ill, enforcement of child support, and the education of disadvantaged children. There are also chapters on the extent of mental illness among the poor, and the relationship between paternalism and democracy.

The volume largely succeeds on the first and second fronts. Thus, readers who desire a broad introduction to the current nature of, and history behind, paternalistic interventions in a variety of policy areas will generally be satisfied. Of particular note is the chapter by Mark A. R. Kleiman, which provides an insightful and thoroughly engaging look at the development of paternalism in the area of drug policy. Also of note is Thomas J. Main's chapter on the evolution of paternalism in New York City shelters. Less useful is Mead's chapter on welfare employment, which rehashes a history of welfare reform policy and legislation that has been thoroughly and sufficiently described elsewhere.

The volume is less successful on the third and fourth fronts. Part of the explanation for this failure may be that the volume is premature, in the sense that solid evidence on the effectiveness of many paternalistic programs and policies is not yet available. It is true that evidence indicates that nondirective and nonpaternalistic interventions in a number of policy areas have not been successful (however this is defined), and several of the contributors discuss this evidence. In his chapter on welfare reform, Mead summarizes some of what we know about the effectiveness of voluntary welfare-to-work programs, and points out the well-known fact that voluntary programs and economic incentives have not been effective in getting many welfare recipients into the labor force. Also, Rebecca Maynard summarizes evidence on the effectiveness of programs targeted at teen-aged mothers. She shows that nondirective programs were generally unsuccessful in increasing employment or reducing repeat pregnancies (however, some of the programs were successful in increasing school enrollment). Similarly, Kleiman discusses the pitfalls of voluntary drug diversion programs.

A few of the authors do present some evidence on the effectiveness of paternalistic interventions, although the usefulness of the evidence for drawing substantive conclusions is limited. For example, Ronald B. Mincy and Hillard Pouncy discuss an experimental evaluation of a mandatory Wisconsin program with the goal of increasing child support payments among poor, noncustodial fathers. Preliminary results show that fathers in the experimental program increased the amount and number of their child support payments, but not by more than fathers in the control group did. One positive outcome was that employment among fathers in the experimental group increased relative to fathers in the control group. However, these results pertain to one program in a single U.S. city. More research is necessary for policymakers to make a determination about the efficacy of paternalistic intervention in this area.

In addition, Maynard contrasts the effectiveness of voluntary and mandatory programs for teenage mothers. She argues, convincingly, that the majority of teenage parents will not participate voluntarily in education or training programs. However, while there is some evidence that the two mandatory programs she examines were more successful in increasing employment among teen mothers, the evidence is mixed with respect to program effects on high school enrollment, educational attainment, and job training. Again, more research contrasting the effects of mandatory and voluntary programs is called for.

Also, in assessing the effectiveness of paternalistic programs, close attention should be paid to the definition of "success." For example, Mead presents estimates obtained from evaluations of mandatory welfare-to-work programs operated during the era of the Job Opportunities and Basic Skills Training Program (JOBS). He argues that the results show that recipients and society usually experienced economic gains. However, the most successful program Mead examines—the Greater Avenues for Independence (GAIN) program in Riverside, California—only raised annual income (defined as earnings plus Aid to Families with Dependent Children [AFDC]) by about $400. If costs of working were included, the average client might have been worse off as a result of the program (because AFDC benefits would be reduced). Mead also terms a success the decline in caseloads in Wisconsin, which has probably gone the furthest in requiring work of those on assistance. However, it is not necessarily the case that individuals who have left the rolls are better off. Indeed, a recent study of persons exiting the rolls in Wisconsin found that overall income fell for the average leaver. Furthermore, only 35 percent saw an increase in economic resources.[1]

The volume might also have benefited from a tighter focus on issues that must be confronted if we are to determine whether the move toward paternalism is, on balance, beneficial. To elaborate, in the concluding essay, James Q. Wilson states that "One cannot. . . simply oppose paternalism because it abridges freedom; government already abridges it in countless ways. What is necessary is to ask under what circumstances, to what ends, and in what ways government should expand the extent to which it makes demands on citizens" (p. 340). Reading this passage at the end of the book, one is left lamenting the fact that these issues were not explored more fully in the previous 330 odd pages.

The exception is Kleiman, who asserts that a necessary condition for paternalistic interventions is the existence of "large and systematic divergences between the actual behavior of some human beings, or of human beings generally under some circumstances, and the canons of rationally self-interested action as microeconomists understand them" (p. 186–187). Still, he argues, this is not a sufficient reason for the adoption of a paternalistic policy. Rather, to fully justify paternalistic interventions, it is necessary "to find instances in which the divergence of actual behavior from individual self-interest is large and systematic and the costs of intervention are smaller than the gains" (p. 191). He also makes the important point that paternalistic policies will usually not be justified if they improve individuals' short-term well-being at the expense of their future capacity for self-management. He then argues cogently for paternalistic policies (in the guise of coerced abstinence) in drug policy, and further describes in detail how such a program would work, and what its costs and benefits might be.

Finally, in addition to greater focus on the issues Wilson raises, this reader would have liked to have seen more discussion of the potential pitfalls of paternalism, especially in areas where such policies may have unintended negative consequences. For example, in the area of welfare reform, it is possible that paternalistic policies may result in fewer poor parents seeking assistance, with negative consequences for their children. A similar argument might be made in the case of shelters for the homeless.

Overall, Wilson points us in the right direction for considering the potential costs and benefits of adopting increasingly paternalistic social policies. However, this volume does not bring us far enough along that path.

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[1] See Cancian et al. (1999). The authors' calculations take into account food stamps, but not the earned income tax credit. They also exclude costs of working, such as costs of child care, Social Security taxes, transportation, and work attire.

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Steven Kelman

Corruption and Government: Causes, Consequences, and Reform, by Susan Rose-Ackerman, New York: Cambridge University Press, 1999, 266 pp.

Corruption is out of scholarly fashion these days. This was not always so. There was once a period of corruption iconoclasm, where some scholars discovered the virtues of corruption as a tool to allow otherwise-lethargic governments to get things accomplished. Edward Banfield's *Political Influence* (1961) defended the old Chicago political machine as a method for assembling political power to allow decisions to get made in a highly decentralized environment. Local machine leaders were key to assembling that power, and "if it is to survive, the machine must tolerate a certain amount of corruption at least until such time as competent precinct captains can be induced to work from other motives than personal gain. At present, in the working-class districts at least, the other motives do not exist" (p. 257). One influential article (Leff, 1964) argues that, if a country's legal framework is inefficient, bribes to avoid silly regulations or taxes that otherwise would hinder business investment can be more efficient than the status quo. Likewise, if the alternative to corruption is slothful bureaucratic inaction, then bribes may also promote useful business investment.

In recent years—and most dramatically, in terms of public discourse, in the context of discussions about "crony capitalism" in the wake of the l998 Asian economic crisis—the earlier corruption iconoclasm has disappeared, replaced by a strong consensus among development economists that corruption hurts economic development. Susan Rose-Ackerman's *Corruption and Government: Causes, Consequences, and Reform* is an important book in that new tradition, which brings together the research and theorizing of others, adds her own insights and proposals, and, in addition, shows both the strengths and the limitations of an economics-based critique of corruption.

Development economists now generally argue that public corruption involving government procurement decisions, and the enforcement of taxes and regulations, hurts economic growth. The economist's critique of corruption is simply an expansion and exemplification of the view, associated with Hayek (1960) and other students of the role of institutions in market economies, that a successfully functioning market economy needs rules that are clear, predictable, and stable, in the context of which economic actors can make investments. A rule-less environment is one that adds additional risks into the calculations of investors; they can be less certain that, for example, they will be able in the future to get access to imported resources they may need for their investment to work, that they will be able to avoid future bribes that reduce future cash flows on the investment, that they will be able to keep or repatriate an agreed-upon share of their profits, or even that their investment will not be taken over by the government when it begins to yield returns. In such an environment, otherwise-profitable investments will not be made. Corruption also hinders overall growth by encouraging corrupt governments to put excessive resources into infrastructure projects that are awarded to private contractors and are hence a rich source of bribes in connection with the procurement process. Rose-Ackerman cites a study arguing that corruption-tainted public construction projects in Italy "had little or no justification beyond their ability to produce kickbacks" (p. 31), as well as empirical studies showing that countries with higher corruption levels spend a higher proportion of GDP on infrastructure, but have lower levels of foreign investment and total investment.

In some interesting passages, Rose-Ackerman takes on corruption iconoclasm directly. "One cannot rely," she writes, "on investors to pay bribes only to avoid inefficient rules and taxes. They will, instead, want to reduce the impact of all state-imposed burdens, justified or not" (p. 22). Furthermore, "the defense of bribery as an allocative tool is static. It assumes a given set of laws and public program requirements. Instead, corrupt officials…may create scarcity, delay, and red tape to encourage bribery" (p. 26). And if certain individuals or types of people have preferred access to corrupt government officials, they will become a lobby against moves toward more sensible government policies.

A virtue of economists' accounts of social phenomena such as corruption is that they often present a relatively parsimonious, elegant theoretical structure and then proceed, quasi-deductively, to interesting conclusions. Rose-Ackerman starts by noting that opportunities for corruption arise when government makes a benefit available, but not to everyone (i.e., scarcity obtains), and where government officials have discretion about who will receive the benefit. One may reduce opportunities for corruption by changing one or more features of this environment. For example, one may privatize an activity or remove a regulation, thus eliminating the government-provided benefit. One may reduce the discretion of government officials to make decisions, by subjecting them more to rules or by reducing any given official's monopoly by giving benefit-seekers alternate places to go to get a decision made.

Rose-Ackerman uses her conceptual apparatus to take us on a guided tour of many issues associated with the economics of corruption and the politics of reform. Sometimes her conclusions are surprising, but interesting. Thus, for example, she argues that term limits promote corruption by removing a potential reward (re-election) for being uncorrupt. Why do people seldom need to pay bribes to buy postage stamps, she asks? Because the object of a bribe attempt has the choice to go to another window or another post office to get stamps.

The typical view among anti-corruption economists, as noted earlier, is that one tool against corruption is to decrease the discretion available to government officials when they make decisions. A simple connection between discretion and corruption risks is too simple, even in its own terms. Instead, in my view, decision standards and transparency are more important variables. It is relatively easy to corrupt even rule-bound officials if the grounds for decision can easily be kept secret. It is relatively difficult to corrupt officials with discretion if decisionmaking standards relate to the public good and if the grounds for the decision are transparent.

To her credit, Rose-Ackerman also recognizes, albeit too briefly and with insufficient connection to the large literature in organization theory and the new public management on this topic (e.g., Barzelay, l992), that binding government officials to rigid rules can create problems for effective government decisions, particularly in relatively complex areas. Thus, for example, rigid low-bid procurement procedures may saddle governments with contractors who are incompetent, excessively optimistic about their costs (and will create problems when they overrun), or venal (and will game the government after buying in at a low price). Her discussion of discretion-promoting procurement reform efforts in the United States during the Clinton Administration (which, as she notes, I led) is generally fair, although somewhat critical. Because corruption-fighting is an important goal, and the admonition "eliminate discretion" may impede effective government, creative thinking about managing this dilemma is important.

I saw two shortcomings in Rose-Ackerman's book. The first is that ethical considerations regarding corruption nowhere are allowed directly to speak their name. For *Corruption and Government*, corruption is a problem largely because it is economically inefficient, not because of how people get treated. However, corruption is wrong, over and above its effects on economic welfare, because people who are denied benefits they deserve because they failed to pay a bribe have been treated arbitrarily, and hence unfairly and disrespectfully. Such unfair and disrespectful treatment is all the worse because it takes place in the context of relations between people and government. Government, with its visibility, has a special responsibility to serve as a good example and teach ethical lessons about respect for persons. Government unfairness is hence more serious than disrespectful behavior by private individuals. Furthermore, use of public office for private enrichment attacks the norm of public spiritedness in public action, a norm that both teaches a lesson about the importance of an ethical point of view in human interactions in everyday life (Kelman, l993), as well as constituting an important component of a polity that tends to produce decisions promoting the public good (Kelman, l987).

To be fair, Rose-Ackerman notes, more than many economists, the negative effects of a corruption-plagued regime on the political legitimacy of a regime, i.e., citizen trust in government. But corruption hurts legitimacy because people see it as unethical. Ethical concerns underlie legitimacy problems.

I raise this problem not because Rose-Ackerman is by any means the worst offender, but because a missing chapter on ethics is common in much work by economists and policy analysts. Doubtless it seems to some too elementary—perhaps almost unworthy of a scholar—to state that we should criticize corruption simply because it is ethically wrong, independent of its effects on efficiency (it should be noted that efficiency effects are of course one kind of ethical effect, since they involve the overall level of social welfare). Arguments that corruption is economically inefficient seem more complex and hence more interesting. Scholars may rest assured that sophisticated ethical argumentation is often quite complex and intellectually challenging. More important, however, we do ourselves, our students, and our societies no favor by leaving ethical issues unstated. Just as do governments when they act, we when we write about public issues, because what we do is also visible, have a special responsibility to teach lessons about the importance of respect for persons. The corruption iconoclasm of another scholarly era is a product of a climate where ethical concerns do not get stated unless they cash out into efficiency issues. And surely a failure to confront the ethics of corruption directly does not help the struggle against corruption in a society; indeed, before the recent interest in corruption by development economists and institutions where they are influential (such as the World Bank), most battles against corruption were led by people whose main objection to corruption was ethical.

A second shortcoming in Rose-Ackerman's book is a failure sufficiently to use the literature on contagion and extinction phenomena in social life (e.g., Schelling, l978; Rogers, l995) to supplement her account of the politics of the battle against corruption. She notes the major decline of corruption in once quite-corrupt polities such as Britain and the U.S. federal government. At some point in a polity, a tipping point is reached where either corruption becomes so common that the individual at the margin believes the risks are low and the opportunity costs of being a non-corrupt "chump" becomes high—or vice versa. Some theoretical or empirical modeling of contagion and extinction as applied to corruption in a society would have been helpful.

This is a useful book, rigorous but not excessively abstract, oriented toward an economic approach but not exclusively. And hopefully it will help contribute to a goal, reduction of corruption in government, that, I'd be willing to bet, Susan Rose-Ackerman doesn't endorse only because it would increase economic growth.

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Clarence N. Stone

Civic Engagement in American Democracy, edited by Theda Skocpol and Morris P. Fiorina, Washington, DC: Brookings Institution Press and Russell Sage Foundation, 1999, 530 pp., $19.95 paper, $52.95 cloth.

Drawing on both contemporary survey data and institutional studies over time, this edited volume provides unrivaled breadth in the examination of civic engagement in the United States. Chapters cover women's groups, professions, the PTA, the effect of religious involvement, and citizens' groups, among other topics. Though the book gives some attention to the local community, it is devoted mainly to the national picture, and it is especially successful at that level in tracing broad changes in American civic life during the past century.

In the concluding chapter, Theda Skocpol does a first-rate job of spelling out the shift from cross-class membership associations to staff-led advocacy groups, while identifying causes of this shift and exploring its implications. Whereas Jeffrey Berry's chapter on citizens' groups makes an optimistic assessment of the proliferation of advocacy organizations, Skocpol offers a more balanced view. On the one hand, rights advocacy has helped make society more inclusive along racial and gender lines. On the other hand, Skocpol points out, these organizations are quite oligarchic, and in no way answerable to a mass membership base. Moreover, advocacy organizations have their own dynamic, which—in the search for the "drama and controversy" they need to sustain themselves—impels them toward narrow stances and "polarized positions" (p. 503).

In a civic life dominated by staff-led advocacy, Skocpol argues, ordinary people have little voice. Instead, under a "reconfigured class structure" the managerial and professional stratum occupies key positions. This "new class" has become more numerous in recent years and now makes up a comfortable and privileged segment of society. For members of this educated and affluent group—"busy career men and women who are choosy individualists as well"—a wide variety of staff-led advocacy groups is ideal; they are able to pick and choose causes (p. 496). And they can contribute money without engaging in time-consuming forms of cross-class interaction. It follows, then, that they do not know much about how others live. Yet the pattern of civic activity they foster nonetheless has profound effects on the non-privileged masses. To illustrate the public policy consequences of a shift from membership-based associations to elite-based advocacy, Skocpol offers a contrast between the GI Bill and Clinton's failed health-insurance initiative. She makes the telling point that the American Legion, "a vast voluntary membership federation," not only drafted a far more generous and less bureaucratic piece of social legislation than the New Deal brain trust contemplated, but local Legion posts also played a major part in the public education and lobbying campaign that ensured its passage (p. 502). By contrast, in a process dominated by "top-heavy advocacy groups," Clinton's health initiative generated bureaucratically complex policy proposals that ended in defeat and therefore no expansion in coverage (p. 503).

Though *Civic Engagement in American Democracy* unquestionably makes major contributions and advances understanding on several fronts, it comes up short on some counts. By the editors' claim, in contrast to social-capital work, particularly that of Robert Putnam, this collection offers historical-institutional and rational choice perspectives. Yet, after an opening discussion, the matter of contrasting approaches gets little explicit attention, and, as a concept, civic engagement goes largely undefined. At the same time, this collection is suffused with normative issues, but perhaps because it is a collection and not a single-authored volume, it handles these issues in somewhat disjointed fashion. In its treatment of normative questions, the book offers little development from chapter to chapter and less intra-volume debate than one might expect. Issues are certainly posed, but sometimes underlying assumptions do not receive the attention they warrant.

Posing the provocative question of whether civic engagement is a good thing, Fiorina's chapter illustrates the problem. For starters, what is civic engagement? In building a case that there is "a dark side of civic engagement," Fiorina at different points seems to equate civic engagement with social capital, with varied acts of political participation (such as voting, attending a rally, and membership on a public board), and with popular pressures generally. What, then, has a dark side? Fiorina implies that it is all forms of political participation, and that all forms are pretty much alike in falling outside what can be considered normal activity for humankind. Therefore, according to Fiorina, participation tends to be dominated by extremists, rather than typical citizens, by people who are strongly issue-oriented in contrast with those who are often uninformed, somewhat indifferent, but nevertheless collectively solid.

Seeing the citizenry as divided into two forces—the few who are driven by expressive motives to be unreasonable about issues, and the many who care mainly about everyday matters and are poorly attuned to issues—Fiorina calls for diluting "extreme voices," for giving greater weight to the many and less weight to the few. He recommends installing such nondemanding forms of participation as electronic town halls. That electronic city halls might not only increase participation but also expand the voice of extremists is not considered, though the parallel of talk radio suggests to this reviewer the possibility. Because Fiorina sees citizens as guided by differences in "taste" for participation, he treats the political challenge as one of making participation cheap enough so that those without much appetite for it will nevertheless be willing to pay the small cost entailed. By this line of reasoning, Fiorina has need to consider only the convenience or "costs" of using a forum, not the nature of the forum and its effect on the quality of participation. Therefore he sees no need to ask whether involvement in an electronic town hall is an activity that cultivates responsible citizen behavior.

Though acknowledging at one point that more might be involved than differing tastes for participation, Fiorina does not pursue that line of inquiry. He sees that conflict might beget conflict, but does not explore its causes. And a potential dialogue with historical-institutionalism goes unpursued. Does this matter? Perhaps. Among the authors in this volume, Fiorina particularly centers on behavior he sees as inappropriate in a democracy. It would seem to be important, then, to ask which behaviors come into being and why. And an open and extended dialogue between rational choice and historical institutionalism might have been illuminating on this question.

Significantly, however, without asking how they might be fostered, Fiorina identifies several behavior traits as desirable: being tolerant, moderate and not ideologically rigid, comfortable with the language of compromise, not self-righteous, realistic and willing to accept trade-offs, and inclined to avoid language about "enemies." Kay Schlozman, Sidney Verba, and Henry Grady, in their chapter, mention parallel virtues: respect for others, following norms of reciprocity and social trust (echoing Putnam), a capacity to transcend narrow points of view and an ability to see the interest of the larger community, and a capacity to conceptualize the common good. For her part, Skocpol talks about constructive relationships and the understanding these relationships might impart: interactions and associations across class lines, working with rather than for, the experience of knitting together a diverse membership, and a tradition of shared ideals about citizenship.

The book, therefore, sets forth, albeit in a somewhat offhand way, traits and skills regarded as appropriate for the practice of citizenship in a democracy, and they are not greatly different from those identified by Putnam. Unlike Putnam's work, however, *Civic Engagement in American Democracy* does not focus on the question of how to cultivate appropriate traits and skills. Skocpol gives some attention to the matter, and her positive valuation of membership associations sounds quite similar to Putnam's assessment of face-to-face voluntary groups. But the similarity is not pursued.

Relatedly, though Skocpol's concluding chapter addresses what could be done to foster desirable citizenship traits and relationships, it might have gone further by giving more attention to the local scene. With professional expertise gaining greater sway at the national level through the dominant position of advocacy organizations, a potentially significant counter-trend may be taking hold at the local level. Professionals charged with management responsibilities in planning, law-enforcement, education, and municipal government generally have begun to link themselves in new ways with citizens, and foundations are funding community organization and neighborhood capacity-building. While Skocpol acknowledges grass-roots groups, the book overall gives them scant attention. For example, the chapter on the PTA treats mainly the evolution of the national organization, and devotes only one paragraph to parent participation in local schools. Yet parent engagement in schools along with such practices as community policing and neighborhood involvement in planning and service provision might be means through which healthy forms of citizenship can be cultivated and the "new class" of professional managers can be educated about the needs and hopes of ordinary people. Perhaps these recent practices are only passing fads, but schools, public safety, and neighborhood conditions are central concerns for most families, and involvement in those issues need not rest on an abnormal "taste" for politics. Whether significant constructive change is under way cannot be fully answered without greater heed to the local community and the conditions under which contacts between professional managers and citizens can be promoted. In sum, the volume edited by Skocpol and Fiorina expands our understanding of civic engagement and raises important questions. It also leaves important work to be done in the future.

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The Gordian Knot: Political Gridlock on the Information Highway, by W. Russell Neuman, Lee McKnight, and Richard J. Solomon. Cambridge: MIT Press, 1997, 324 pp., $16.00 paper.

Coordinating the Internet, edited by Brian Kahin and James Keller, Cambridge: MIT Press, 1997, 500 pp., $31.95 paper.

We live in a wondrous time for students of economic institutions. The rapid rate of innovation in communications and computing technologies in general and the rise of the Internet as a major new communications infrastructure in particular are spurring major changes in regulatory, legal, and private market arrangements. Traditional regulatory regimes that neatly separated telephone, cable television, broadcasting, and computer markets are stretched to their limit in a world in which it is possible to watch a TV show over a phone line, hook up to the Internet with a cable modem, use broadcast spectrum to provide wireless telecommunications, and have a conversation over the Internet. We are creating new or redefining property rights for the electromagnetic spectrum and intellectual property. These technologies are also blurring the boundaries between firms and markets. As Williamson (1991) has argued, economic activities that require coordinated adaptation have been managed best within firms or bureaucracies while those requiring autonomous adaptation have been managed best in markets. New technologies, however, have both expanded the possibilities for autonomous adaptation, shrinking the boundaries of firms, and have increased the need for interfirm coordination to manage networks and technical standards, creating pressures for new, intermediate forms of governance.

The area of study is fascinating because the future state of these institutions remains in flux. For example, will tensions between competition and coordination be resolved in favor of monopoly or oligopoly as has occurred so often in the past? Will governance institutions develop that enable the Internet to continue thriving as a set of interconnected and interoperative networks or will coordination be too weak to prevent it from fracturing into separate, differentiated subnetworks? Moreover, if history is an indicator, we know that early and seemingly inconsequential decisions made in the coming years will have significant and long-lasting effects on the structure of these industries, the benefits we derive from these technologies, and the role media play in democratic and policymaking processes.

The two books under review, *The Gordian Knot: Political Gridlock on the Information Highway* and *Coordinating the Internet*, are early and complementary contributions to what is sure to be an extended debate. *The Gordian Knot* is a broad and ambitious work. It places current communications policy debates in sweeping historical perspective stretching back to the influence of Roman cart design on railroad gauges. It also takes an expansive perspective concerning the issues at stake, addressing industrial policy, economic growth, international trade, standard setting, and broadcasting policy. Finally, it proposes a policy framework called open communication infrastructure (OCI), which the authors claim is necessary to resolve the contradictions and constraints created by current regulatory institutions. *Coordinating the Internet*, in contrast, is more focused and less ambitious. It strives to describe and understand several of the specific governance problems that have arisen as the Internet has been privatized. Both of these approaches have strengths and weaknesses, but in the end, the whole is greater than the sum of the parts, providing both historical and theoretical perspective as well as detailed insider knowledge of institutions as they develop.

The authors of *The Gordian Knot* are an interdisciplinary team comprised of a sociologist, a political scientist, and a technologist. They met at MIT where they joined forces in the battles over the development of high-definition television standards. Their book is enriched both by their eclectic perspectives and their policymaking experience. The central argument of the book is that the current state of communications policy has reached a state of gridlock that inhibits innovation and inefficiently perpetuates the power of existing regulators and industry giants. They use the metaphor of the Gordian Knot to emphasize their belief that successful resolution of these problems requires "a bold stroke and fresh thinking" (p.*xi*) to cut through the gridlock. The sword they propose for the job is a set of four principles: open architecture, open access, universal service, and flexible access.

Their statement of the central problem is followed by chapters that provide broad overviews of the main issues as they perceive them. First, they examine the nature of networks. Building on historical examples of railroad, telegraph, and telephone networks, they demonstrate the importance of standards and interconnection among networks in the development of these industries. Second, they examine the government's historical role in the design and development of networks. Despite the deadlock that they argue plagues policy, they find that government has had a complex and often positive relationship with the private sector, encouraging investment, interconnection, and standards setting. Third, they argue that getting policy right in this sector is particularly important because investments in computers and networks produce great benefits in terms of productivity and economic growth. The penultimate chapter makes their case for gridlock based on an analysis of recent developments in telephone, broadcasting, and cable regulation.

These chapters are enlivened with a wealth of historical and technical insights and would be a valuable read for any student who wishes to gain perspective on current institution-building processes. They remind us that battles between open and closed networks have a long and ugly history. In the early days of railroads, some interfirm disputes were settled with gunslingers instead of high-powered lawyers. Interconnection—or more accurately the refusal to interconnect—has been aggressively deployed as a competitive weapon throughout the history of networks: by railroads against each other, by Western Union against upstart telephone companies, and by IBM against competitive computer manufacturers, just to name three examples. In addition, they remind us of regulators' constant struggles with rapidly advancing technology. From the 1960s onward, regulators fought a Sisyphean battle to delineate the boundaries between regulated telecommunications services and competitive computer services. Yet, as soon as the ink dried on each set of rules, technology found new ways to blur the distinctions, frustrating competitors and regulators alike.

This volume, however, works better on the level of problem definition than prescription. Their argument that communications industries are impeded by political gridlock is overstated. On many levels, they are correct. Regulators, and state regulators in particular, have been slow to abandon entry regulations and direct price controls. In addition, one cannot deny their contention that the Telecommunications Act of 1996 is a flawed piece of legislation, largely forged by the political muscle of industry heavyweights. It has spawned more litigation than new entrance in local telephone markets and failed to tear down outdated regulatory barriers between different communication services. Gridlock, however, implies a lack of progress, and that assessment could not be further from the truth. In the last decade policymakers have privatized the Internet, employed auctions to reallocate the electromagnetic spectrum to higher value uses such as cellular telephony, and eliminated all prohibitions on entry into local telephony. These actions constitute a record in which they can take pride.

More importantly, their stark assessment places too much blame on existing regulatory institutions and not enough on the inherent difficulty of the problem at hand. Managing a transition to competition in markets with high levels of technological uncertainty and controlled by incumbents with overwhelming market power is inherently contentious. While promoting the goals of open architecture and open access may seem to resolve these difficulties, they do not. Incumbents have incentives to impede open access, and openness is not an absolute goal because innovations at times require closed, proprietary networks. Thus, implementing their principles requires the elaboration of a set of rules on when and how open access will be provided, and this implementation would be no less complex and contentious than the policy gridlock they decry. They try to overcome this hurdle by arguing for a change in institutions: a greater reliance on antitrust enforcement and the courts and less reliance on Federal Communication Commission rulemaking. This proposal may have merit, but it is far from convincing, given that antitrust enforcement is a blunt instrument easily politicized.

Coordinating the Internet is the product of one in a series of Harvard Information Infrastructure Project workshops held to chart the policy implications of the Internet. This collection of essays includes contributions from an eclectic group of technologists, lawyers, policymakers, academics, and managers. The one quality that unifies them is that they have all been closely involved in the early evolution of the Internet. As such, this volume lacks a unifying theoretical focus, but what it lacks in theoretical and historical perspective, it more than makes up in rich institutional detail.

The volume is divided into five sections of loosely connected essays. The first examines what institutions govern the Internet, and the next four cover specific policy problems: the domain name registration system and its conflicts with trademark law, the allocation of network numbers, the interconnection of networks, and the coordination required to assure service quality over multiple networks. All of the essays, however, are motivated by the same set of events. In the early 1990s the government moved to privatize the Internet by eliminating its acceptable-use policy, which restricted the National Science Foundation (NSF) "backbone" to research and academic uses, eliminating funding for the NSF backbone network that connected regional networks, and contracting out the registration of domain names. These changes converted the Internet from a centralized, hierarchical, and cooperative academic network to a collection of decentralized and competitive commercial networks. This transition raised the questions of whether and how the coordination previously provided by government leadership should be replaced.

The essays offer much good news, certainly when compared to the cumbersome and contentious policymaking found in traditional telephone regulation. Competing networks can coordinate their activities in a decentralized manner to provide interconnected and interoperable services without the need for heavy-handed, centralized oversight. Much of the success is purely technological. Common acceptance of the TCP/IP protocols that run the Internet eases interconnection and permits providers and software developers to innovate without forging systemic bargains with other networks. Success is also due to the inherent flexibility of markets as network providers have developed a range of contracting forms to address interconnection issues. These developments are the most positive evidence that a competitive and interconnected communications industry can develop with further deregulation.

Nevertheless, the essays also describe numerous issues that have or will soon create pressures for the elaboration and refinement of institutional capabilities to provide coordination and resolve disputes. Protocols resolve coordination problems, but only once they have been agreed upon. New protocols must be adopted and this process may become increasingly contentious as competition diminishes the Internet's culture of interoperability. Competitive pressures have already placed much stress on interconnection arrangements, and competitive equilibria of network markets do not necessarily lead all networks to interconnect. How open and universal access will be promoted if market incentives prove insufficient remains uncertain. There are also common resource problems. Like the post office, computer networks must agree on the physical addresses of computers (e.g., IP addresses) and on the logical names connected to these physical addresses (e.g., domain names). These resources are scarce, and the manner in which they are allocated remains controversial.

Interestingly, the most publicized governance issue, the battle over domain names, has little to do with technology. Firms were quick to understand the value of domain names (e.g., microsoft.com) as recognizable identifiers and sought to protect them under traditional trademark law. The problem is that there is not a simple correspondence between domain names and trademarks. The most popular domain, .com, is international while trademarks are national, and firms in different industries can use the same name (for example, a pillow company could use the name Microsoft) while there is only room for one microsoft.com in the domain registry. The essays paint a fascinating story of a search for a forum in which to resolve the inevitable disputes. Most interestingly, conflict has been driven by an artificial scarcity. The conflict could be resolved by reworking the domain name structure (e.g., create domains like software.com and pillows.com) or by eliminating domain names in favor of a form of yellow pages for IP numbers. We can learn much about the political economy of institution-building based on how these choices are made.

Written in 1996 the volume is already a bit dated. There have been major developments especially concerning domain names and interconnection. Nevertheless, the essays remain vital mainly because the issues with which they grapple remain constant. In addition, in a valuable use of Internet capabilities the Harvard Information Infrastructure Project maintains a site of updates located at http://www.ksg.harvard.edu/iip/cai/cisupp.html.

Both these books portray momentous shifts in the institutional governance of communications industries. They hint at the range of institutional mechanisms that can manage the competition new technologies permit and the cooperation they require: antitrust, new property rights for spectrum and IP numbers, international organizations, etc. These choices will have to be made carefully. Although there is clearly a need for centralized coordination, at least at times, all the authors implicitly agree with David's (1986) depiction of government as a blind giant. It can be a powerful actor if it acts early in the development of new technologies, but it is hindered at that stage by a lack of information. If it waits, however, markets tend to lock-in on technologies, constraining government's ability to influence future technical choices.

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James R. Barth

Reforming Financial Systems: Historical Implications for Policy, edited by Gerald Caprio and Dimitri Vittas, New York: Cambridge University Press, 1997, 222 pp., $49.95 cloth.

Money and the Nation State: The Financial Revolution, Government and the World Monetary System, edited by Kevin Dowd and Richard Timberlake, New Brunswick, NJ: Transaction Publishers, 1998, 453 pp., $19.95 paper.

The world has witnessed an unprecedented number of costly financial crises over the past two decades. Indeed, more than two-thirds of the 182 member countries of the International Monetary Fund (IMF) have experienced banking crises during this relatively short time period. The cost to resolve these crises has ranged from about 3 percent of gross domestic product (GDP) in the United States, to 15 percent in Mexico, and to a much higher 42 percent in Thailand. These crises, moreover, have occurred in countries in all parts of the world and at all stages of economic development. This alarming situation has more than ever emphasized the importance of financial systems and focused attention on ways to be sure they function properly.

These two books are timely and excellent guides to understanding what has been happening to financial systems over time and in various parts of the world. Furthermore, they discuss specific ways in which to reform these systems to promote stable economic growth and development. The editors of both books have themselves made important contributions elsewhere that address these issues. Here they have assembled an impressive group of scholars who provide informative analyses of ways to restructure financial systems around the globe.

In *Reforming Financial Systems* the editors Gerald Caprio and Dimitri Vittas in the very first paper—as if anticipating the Southeast Asia crisis—state that "developing effective and independent regulators is necessary to prevent bankers from engaging in self-lending and excessively risky ventures, as well as relying on government bailouts in the event of failure,"(p.20). This is certainly true and sets the tone for the other 10 papers in the book. All of the papers assess various ways in which governments intervene in financial systems from both historical and contemporary perspectives. Forrest Capie focuses on central banks and argues that they "…perform best when left alone to provide for the long-term stability of the price level" (p.33). Randall

Kroszner goes even further and argues that the Scottish free banking experience of 200 years ago demonstrates that there are "private alternatives… to a central bank to maintain confidence in, and foster the stability of, the financial system." He adds that "exploring such alternatives [like free banking] could hold great promises for emerging and transition economies" (p.56).

Michael Bordo focuses on the way in which governments regulate banking systems and contends that "…restricting nationwide branch banking is a mistake…" Indeed, "efficiency can be promoted by permitting competition from foreign banking systems, as well as domestic and foreign nonbank financial intermediaries" (p.79). Interestingly, the United States and many other countries have recently been moving in just this direction. In a similar vein, Eugene White concludes that deposit insurance "...was the peculiar creation of the U.S. banking experience…" and "…is inappropriate for developing or transition economies." It "…presents enormous incentive problems…" and "…may demand too much from bank regulators…(p.96)." Nonetheless, it remains a fact that most countries around the world have adopted deposit insurance schemes. Given this situation, Anthony Saunders and Berry Wilson argue "…that extended owner liability along with deposit insurance may help minimize these [adverse] incentives" (p.110).

An issue always arises as to which activities banks should be allowed to engage in. Charles Calomiris argues that universal banking as practiced in Germany has many benefits as compared with banking as practiced in the United States. Indeed, he concludes, "The lesson for developing countries seeking to design their financial systems seems clear, avoid the lengthy and costly detour of U.S. financial fragmentation" (p.124). In fact, the United States has recently joined Germany and other countries by repealing the law that separated banks, securities firms, and insurance companies. Yet, some may argue that even if universal banking becomes widespread throughout the world it may not satisfy the demands of some constituencies. In this respect, Dimitri Vittas examines the role thrift deposit institutions have played in several countries. He concludes that "given the prevalence of poverty and the underdevelopment of a financial infrastructure in most developing countries, the need for institutions that specialize in offering banking services to the poor is likely to persist for a long time" (p.177).

Banks and nonbanks, of course, are only a part of financial systems. Richard Sylla focuses on the development of the securities market in the United States. He concludes that "if the World Bank and other modern institutions are interested in stimulating securities market development in developing and transitional economies, they would do well to… put fiscal, monetary, and banking practices on a solid ground and then mandate disclosure of pertinent financial information to investors." He adds that "This is important because these measures will stimulate both the emergence of securities markets and the demand for securities" (p. 210). This, in turn, is important because there is persuasive evidence that banks, nonbanks, and securities markets all complement one another with respect to promoting economic growth and development.

Money and the Nation State contains 13 papers in addition to a forward by Nobel Laureate Merton Miller and an introduction by the editors, Kevin Dowd and Richard Timberlake. The contributions are grouped into three categories: the history of the modern international monetary system; modern money and central banking; and foundations for monetary and banking reform. In the first category, David Glasner considers alternative explanations for state monopoly over money. He argues that one should view "the state monopoly over money as contributing to the security of the state against internal and external threats." Such an approach is better than "one that views the monopoly as required by the technology of money creation" (p.40).

More generally, Frank van Dun assesses “the prospects of an international monetary regime under political control” and concludes that it “presents little that is new, or appealing.” The reason is that “International or supranational government is still government. If not dictatorial it will be democratic, and if democratic it will be under high pressure to finance all kinds of political projects by whatever means possible, including manipulation of the money supply” (p.66).

After providing an insightful retrospective on the international monetary system, Leland Yeager “illustrates what a fundamental monetary reform would be….” In particular, he argues that “one promising approach would privatize the monetary system.” This could lead, “under the discipline of competition,” to the “automatic equilibration of demand for and supply of media of exchange at a stable price level” so as to “prevent price inflation and major recessions” (p.102). In a reassessment of “the retreat from the classical gold standard,” Murray Rothbard states that “politicians, financial manipulators, and demagogues who spent their careers attempting to free themselves and their home nations from the constraints imposed by an international gold standard system finally got their wish.” He adds that “the consequences of that wish...have been both dire and unnecessary.” In his view, “Discipline is difficult precisely because of human weaknesses, and the story of gold is no different from many other, similar human proclivities to thwart natural or artificial constraints” (p.156). Richard Timberlake similarly argues that “gold is the most attractive vehicle for uncoupling money from governmental manipulation.” He allows, however, “Gold has been the money of ages, but innovations such as money market deposit accounts would perhaps in time supplant gold.” The important point to him, however, is that “all the federal government connections to and controls over money and monetary institutions would have to be severed so that the system would revert to the status quo of the Constitution” (p.188).

In the second category of papers, Thomas Cargill argues that “U.S. policy makers have hindered the transition process toward more open and competitive financial structures and generated a financial structure that is weak and imposes a serious deadweight loss on the economy” (p.195). He believes “the lesson to be learned from the U.S. experience is that government should not only avoid hindering market forces, but just as importantly, government needs to reform its deposit guarantee system to reduce incentives to assume risk by issuers of the nation’s money supply.” And “ultimately, market forces via depositor and equity-holder discipline provide the best means toward this end” (p.209). In a similar vein, Genie Short and Kenneth Robinson argue that “rather than minimizing deposit market instability, the expanded role given to the financial safety net has contributed to and exacerbated financial sector problems throughout the world” (p.214). In their view, “Banks do not need 100 percent deposit guarantees to operate, and governments do not need a too-big-to-fail doctrine to maintain a safe and sound banking system” (p.233).

Alan Reynolds focuses on the role of the IMF in crises. He specifically argues that experience “shows quite clearly that economic crises are curable and that the cure always involves quite similar policies.” Yet, “the IMF still sanctions and underwrites the policies of perpetual failure.” In particular, its “recipe has been one of destructive devaluation and suffocating taxation, often accompanied by wage controls and high tariffs” (p.297–298). Robert Keleher relatedly argues that “virtually the only examples of persistent successful international policy coordination” are based upon “microeconomic structural policies” and “certain policy rules, standards, and legal conventions,” not “discretionary macroeconomic stabilization policies among centralized decision makers” (p.321–322).

In the last category of papers, Richard Burdekin, Gillen Westbrook, and Thomas Willett evaluate two options for improving a nation's inflation performance. They argue that "exchange-rate pegging by itself is unlikely to have a sustained impact…," whereas "there is much stronger evidence in favor of central bank independence" (p.345). Kevin Dowd examines recent financial developments in Europe and argues that "the proponents of a common European Currency and a European Central Bank have never seriously tried to argue the case for them…." Worse yet, he adds, "even if one were sympathetic to the idea of a common currency and a continental central bank, it would still be difficult to argue that the Maastricht Treaty provided a sensible way to archive them" (p.369). More generally, Lawrence White argues "that the major defects in the performance of the historical gold standard are remediable by allowing greater international integration of banking." He adds that "if persuasive, [the argument may] remove some important misgivings about a common international money" (p.380). His conclusion is that "the fundamental obstacle to a sound international monetary system is not so much monetary nationalism as it is monetary statism" (p.398).

In the last paper, Steve Hanke and Kurt Schuler point out that "three types of monetary systems have predominated [in this century]: central banking, free banking, and currency boards" (p.403). They "think the currency board system is well suited for many countries today" (p.405). They note that "currency boards originally arose to replace free banking." And "despite the success of currency boards, most countries replaced it with central banking in the 1950s and 1960s" (p.406). They argue that "currency boards and free banking are similar in their intent to depoliticize the supply of money. The currency board system does so by subjecting government issue of money to strict rules, whereas free banking did so by eliminating or at least marginalizing government issue of money" (p.410). They conclude that "in many countries, the currency board system has been a transitional stage between free banking and central banking. The result of central banking has been high inflation and frequently economic ruin. The currency board system deserves another look, this time as a transitional system from central banking to free banking that is particularly appropriate for countries that lack the conditions for establishing free banking immediately" (p.419).

All in all, these two books provide a wealth of information essential for understanding the importance of financial and monetary systems and potential ways in which these systems could be restructured to promote stable economic growth and development. Regardless of whether one agrees with any specific proposals, the papers collectively force one to reassess more clearly than ever one's views about the timely and important issues being addressed.

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Political Cycles and the Macroeconomy, by Alberto Alesina and Nouriel Roubini with Gerald D. Cohen, Cambridge, MA: MIT Press, 1997, 302 pp., $60.00 paper.

In *Political Cycles and the Macroeconomy*, Alesina and Roubini with Cohen (*ARwC*) present the culmination of their more than 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 subfield 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: the book...

> ...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)

> ...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)

> 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 review, I summarize their arguments and findings and then explore some of the remaining theoretical and empirical anomalies. Critics may find some relief in this, but I intend it 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: politicians 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 Organization for Economic Cooperation and Development (OECD) countries, including the United

States, together, *ARwC* explore the evidence regarding these models' subtly differing predictions. They conclude that the evidence is remarkably consistent:

- It favors the later, rational-expectations models.
- It indicates strong partisan but little discernible election-year effects on macroeconomic outcomes.
- It suggests both election and partisan effects on macroeconomic policies, and, subsidiarily.
- It suggests that partisan policy and outcome effects are clearer in two-party or two-bloc than in multi-party systems.
- It suggests that two-party/bloc governments adjust fiscally to deficit-inducing shocks more quickly than do coalition governments
- It suggests that 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 low unemployment) with reelection. Democratic policymakers thus routinely attempt to time their use of fiscal and monetary policies to exploit delays from expansionary policies to their inflationary consequences, securing high growth and low inflation and unemployment before elections, leaving the inflationary consequence to arise 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 reelect 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 U.S. 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 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 that this necessarily validates the RE model should have come with caveats.

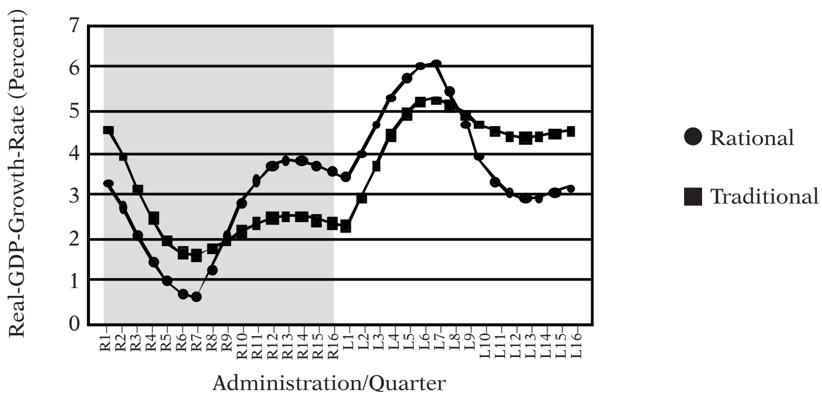

Figure 1. Comparison of rational and traditional partisan real-GDP-growth cycles in the United States.

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. *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).

But the described policy-pattern[2] would produce the shorter-term outcome-pattern in either the RE or non-RE models. The long-noted "honeymoon" effect, which refers to the 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

[1] They report results for six quarters and that those for four and eight quarters differ little substantively. The indicators are lagged one quarter for growth and two quarters for unemployment to reflect delays in outcome responses to policies.
[2] Neither model allows right administrations to pursue anti-inflation and growth in the second half as claimed.

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 (t≈–0.58) or low unemployment (t ≈ 1.15) pre-election or of higher inflation post-election (t≈0.31) (Figure 2). 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 United States since congressional (presidential) elections occur every second/fourth November. Depending on the method, seasonal adjustment could therefore have reduced the size of electoral effects by 25 to 50 percent.

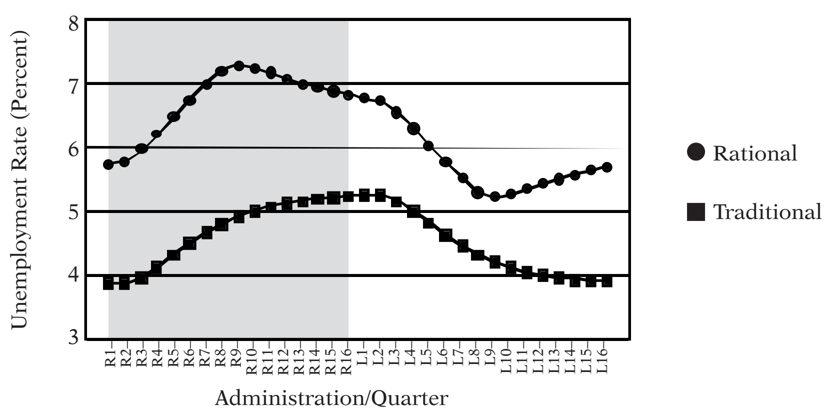

Figure 2. Comparison of rational and traditional partisan unemployment cycles in the United States.

For policies in the United States, *ARwC* explore money growth, nominal interest-rates, budget deficits, and transfers. They find weak evidence of partisan differences in money growth (t≈1.1–1.2), though stronger in a 1949–1982 sample (t≈1.8–2.4), and stronger evidence of partisan differences in nominal interest-rates (t≈2.2–3.3). They again find no indication of pre-electoral effects on monetary policy (t<0.5 in all cases). Oddly, they do not report differences by exchange-rate regime and, more oddly, lag the partisan indicator two quarters. The latter is somewhat problematic because the real effects were assumed to lag one to two 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 two quarters. Furthermore, if Bretton Woods dampened partisan differences in monetary policy, as their inflation results suggest, then the stronger 1949–1982 monetary-policy results indicate a narrow window of partisan differences in U.S. monetary policy, only or primarily occurring in 1973–82.

For fiscal policy in the United States, *ARwC* again find little evidence of preelectoral effects in deficits (t≈0.3) or in transfers (t≈0.4–0.7), or of partisan effects on transfers (t≈0.7), 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 (1999) finds statistically significant the opposite of what those theories predict, and Lambertini (1999) finds insignificance.

Thus, *ARwC* clearly establish that the real effects of partisan U.S. administrations follow a short-term pattern illustrated in Figures 1 and 2, 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 U.S. 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, forthcoming). 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 (1996), 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 U.S. 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 U.S. 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 (http://k7moa.gsia.cmu.edu/default.htm) offers data from congressional voting records of most presidential candidates 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 or two-block countries, intuitive in any partisan model, and some indication preelectoral 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), suggesting 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 (European Monetary Unit) 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: They offer no evidence to support the claim that reduced political variance explains the lack of CBI correlation with output variability. 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 to 21 OECD countries, insignificance which should hardly lead anyone to unequivocal conclusions that the true correlation is everywhere zero. 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 delegation to a conservative central bank 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 (forthcoming) 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 evidence also supports 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 indisputedly important partisan effects on macroeconomic policies and outcomes. If it 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 (1978) and Hibbs (1987), 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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[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 *(Franzese, forthcoming) for full citation of these and others.

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Kathryn McDermott

Market Education: The Unknown History, by Andrew J. Coulson, New Brunswick, NJ: Transaction Publishers, 1999, 471 pp.

School Choice in Chile: Two Decades of Educational Reform, by Varun Gauri, Pittsburgh, PA: University of Pittsburgh Press, 1998, 147 pp.

Andrew J. Coulson's *Market Education: The Unknown History* brings a libertarian perspective to bear on the debate about school choice, as well as linking the familiar economic arguments for educational privatization to a more novel historical account of why some school systems outperform others. Coulson begins by presenting survey data on "What We Want" in an educational system: Around the world, parents express consensus on the need for basic academics and job skills, but have different priorities for more advanced training and differ profoundly on the moral, religious, and ideological content of schooling (p.33). Part II of the book, "What's Been Tried," ranges broadly across time and space from ancient Athens to contemporary public education in the United States, Europe, and Japan. Coulson compares past societies in which education was decentralized and market-driven with those in which control of school systems was centralized in the hands of state or ecclesiastical authorities. He also contrasts contemporary examples of public and independent schools, arguing that in all times and places, markets do better than command-and-control systems at giving the public what it wants—high levels of educational attainment. He concludes with an analysis of "What Works" in education that emphasizes the importance of independence and responsiveness to public preferences, and which leads him to an original proposition for privatization and school choice. The book is clearly written, and the concluding policy recommendation is intriguing. However, Coulson leaves out significant details in his account of educational history. Consequently, his argument will reinforce the beliefs of those who share his assumptions, but will fail to convince those who do not.

Coulson concludes, "The modern and historical evidence points inexorably to the fact that government involvement in education tends to interfere with the very principles it is meant to advance. If the reader takes only one idea away from this book, let it be that" (p.391). His ideals are classical Athens and the Muslim world during the eighth through tenth centuries C.E., in which citizens contracted with independent teachers who specialized in particular subjects, rather than attending "schools" as we know them today. In contrast with Sparta, or with later centralized educational systems, parents were "free to attend to their children's education in whatever way they saw fit," and "this educational responsibility appears to have been discharged with care and wisdom" (p.72). Scientific and technological discovery flourished.

Despite his claim to be telling "the unknown history" of market education, Coulson draws selectively upon secondary sources rather than marshalling primary evidence in support of this position. The primary weakness of Coulson's historical comparisons between "market" and "nonmarket" societies is that he assumes the "market–nonmarket" dichotomy to be the main, or at least most relevant, difference between them. This is a highly contestable claim. Even Athens and Sparta, which from a modern perspective are both "ancient Greek," were societies radically different from each other in more than just their educational systems. Coulson also does not explain why the societies he chooses to compare are the most relevant to the question of how well markets work in education. For example, why not compare present-day Cuba and Chile—two former Spanish colonies in the Western Hemisphere, one Communist and the other with a recent history of market-based educational reform? According to UNICEF figures, the two nations enjoy rough parity in terms of adult literacy and primary school enrollments, and Cuba has a slight edge in terms of secondary school enrollments (UNICEF, 2000).

Coulson's account of the progressive movement in American education exemplifies his selective reading of history. According to Coulson, anti-intellectual progressive educators captured the minds of education professors, administrators, and teachers in a "*coup d'ecole*" (pp.108–119) and used the power inherent in a centralized system of public education to force their agenda upon the public. Coulson's critique of the progressives' excesses is neither new nor controversial. This is particularly true of the section on "Life Adjustment Education," which replaced the traditional academic curriculum with "practical" instruction about how to write polite letters, make effective use of leisure time, and behave on dates (see Cremin, 1961, pp. 332–347; Hofstadter, 1962, ch.13; Powell, Farrar, and Cohen, 1985, ch. 5).

It is not clear, however, that educational bureaucrats ever succeeded in implementing progressive educational ideas to the extent that Coulson assumes (the same is true for outcomes-based education and whole-language reading instruction, two more practices he implies are prevalent in contemporary public schools). Far from the monolith that Coulson portrays, public education in the United States exemplifies the "loosely coupled system," in which authority is decentralized and top-down administration is difficult if not impossible (Weick, 1976). School systems are famous for adopting the vocabulary of innovation without actually altering their practice, and all "reforms" are ultimately implemented by classroom teachers who often have the discretion to subvert policies they oppose. The "progressive education" model appears to some historians to have been no exception to the general trend. Some teaching practices were altered by the progressive movement, while others largely remained stable (Cuban, 1993; Zilversmit, 1993). In the area of curriculum, the diversity of American public education allowed both academic traditionalists and Life Adjustment proponents to win the battle at the same time, mostly through the adoption of multiple "tracks" within secondary schools. In many American comprehensive high schools, rigorous college-preparatory curricula coexist with less demanding (sometimes criminally so) programs (Oakes, 1985; Powell, Farrar, and Cohen, 1985).

Despite the support for traditional academics Americans display in the survey data Coulson cites (pp. 8–11), greater responsiveness to market forces would not necessarily have produced a more academically rigorous outcome. At least one respected (and highly critical) account of mid-twentieth-century educational innovation (Hofstadter, 1962) claims that the Life Adjustment curricula were popular because they resonated with a long-standing American skepticism about "book learning" and other forms of intellectualism. More recently, the 1996 and 1997 Gallup–Phi Delta Kappa surveys on public attitudes about education captured Americans' ambivalence about the importance of academic achievement. In 1996, 60 percent of respondents said that if they had to choose, they would rather their children be active in extracurricular activities and earn "C" grades than earn "A" grades and not be active. In 1997, when asked "What would be the lowest grade a child of yours could bring home on a report card without upsetting or concerning you," 63 percent of public school parents and 64 percent of private school parents said they would not be upset or concerned about a grade of "C" (Rose, Gallup, and Elam, 1997, p. 51). Rather than enhancing quality, a free market in schooling might very well have led to an outcome much like the status quo: excellence for some, and mediocrity for far too many.

Although Coulson's historical and contemporary analysis of markets is flawed, one component of his policy recommendations is original and worth serious consideration. He calls for a "competitive education industry driven by the needs and preferences of families" (p.367). For Coulson, this means a system in which all families, even the poorest, spend their own money on their children's schooling, thus becoming both more likely to pay close attention to educational quality and more likely to get a satisfactory response from school operators. Scholarship funds for children whose families could not afford to spend much on their education would come either from tax revenues or from private scholarship organizations funded by donations from people who support religious education (pp.374–380).

In a society that is increasingly divided over the use of public funds in religious institutions, Coulson's proposal for privately financed scholarships is worth arguing about, even though it is unclear that competition among schools would improve academic outcomes. Indeed, from the libertarian perspective Coulson often takes, a school choice program that included private schools might be preferable to the status quo, as an increase in individual liberty, even if it had no effect on educational quality.[1] According to Coulson, the use of private scholarships would avoid the current controversy about whether tax money may be used at religious schools. Here is where I wish he had begun to go into more detail about issues of equity: Can we be sure that people would voluntarily provide sufficient funds for educating the poor? What of poor people who wish to educate their children in accordance with unusual or unpopular religious convictions for which scholarship funds are less likely to exist? Despite these problems, privately funded vouchers for religious education might be a reasonable way out of the current church–state dilemma. Several private voucher programs are currently under way, and they may provide useful insights into how people behave in real-world educational markets.

The strength of Varun Gauri's *School Choice in Chile* is that it adds to the very small set of evidence about private-school choice proposals that have actually been implemented. Choice among public schools has become fairly widespread in the contemporary United States, but only a few jurisdictions have allowed anything like the pure voucher model in which public money pays children's tuition at private schools. Chile's privatization of education was inspired by the same sort of economic analysis that underlies the work of U.S. choice advocates like Coulson and can be regarded as an empirical test of whether competition improves schools.

In 1980, the Chilean military regime led by General Augusto Pinochet transferred control of public schools from the Ministry of Education in Santiago to municipal governments. It also made public financing available to private schools that did not

[1] If one makes this sort of claim on behalf of choice, however, it is necessary to address the questions of whether children whose educational choices have been made by their parents are better off in terms of their liberty and autonomy than children whose educational choices have been made by the state. This question is exceedingly complex, and Coulson understandably did not take it up in a book whose primary focus was elsewhere.

charge additional tuition. Some private schools, generally serving the Chilean elite, continue to charge tuition and thus remain outside this system. Families decide where to send their children to school, and the public money follows the children to the chosen institutions. The Chileans do not call these funds "school vouchers," but for all intents and purposes that is what they are. Although the military regime was replaced by an elected democratic government in 1990, the basic framework of school choice and privatization remains in place.

Gauri begins with a brief survey of the privatization of Chile's formerly highly centralized educational system and of the theoretical underpinnings of market-based reform. The original part of Gauri's analysis is a survey of Santiago–area households, which examined the links between various social and educational characteristics of families and the likelihood that their children attended a school in the top, middle, or bottom range of quality.

According to Gauri, some evidence indicates that the Chilean experiment has been successful: Student test scores show modest increases, enrollment and retention rates have risen, and new private schools have been founded to take advantage of increased effective demand for educational services (pp.2–3). This second finding is particularly interesting, since it seems to vindicate U.S. choice advocates' claim that a move to school vouchers would increase the diversity of educational offerings. Overall, however, Gauri is more critical than supportive of school choice in Chile, claiming that Chile's experience demonstrates the limitations of applying the market model to education. In particular, he finds evidence that school choice increases educational stratification because schools compete by "creaming" the best students and most advantaged families rather than by striving to improve the quality of instruction (pp.54–55). Also, many families have difficulty obtaining enough information to make good educational choices (pp.101–102).

Although Gauri intends to raise skepticism about choice, the complexities of the Chilean case make it possible for a choice advocate to read the situation as one in which choice has done remarkably well in terms of test score improvement and enrollment increases despite being badly compromised by politics and the remains of an overcentralized public system. School choice in Chile takes place within an extensive regulatory structure, including an official curriculum that even private schools receiving state funds must use (pp.26–29). Teachers may not be laid off or transferred when enrollments decrease. New private schools may open only if their founders can demonstrate to public authorities that the "educational needs of the region and locality justify the existence of new establishments" (p.43). According to Gauri, even the Pinochet regime felt a need to compromise its free-market principles in the field of education.

Both sides in the U.S. debate over educational privatization can thus claim that Gauri's work supports their position and calls into question that of their opponents. The antiprivatization side will welcome Gauri's claim about the need to appreciate the complexity of social policy aims and highlight his caution about the inegalitarian effects of choice, but will be challenged by the apparently beneficial educational outcomes of the Chilean reforms. Privatization advocates will note the new private schools and achievement score increases with approval, but should take caution from the continued importance of politics even in a market system. If a military government committed to privatization could not produce a pure market in education, then it stands to reason that political power would also influence a privatized system in the United States. Market advocates often contrast the failings of the educational status quo with what could ideally be the case in a privatized system. More relevant would be comparison of existing public schools to a market system as it would actually be implemented, with regulations and compromises in place. Although we often speak of "the markets" as though they were natural forces, they are human creations shaped by political interactions (Henig, 1994; Polanyi, 1944). Shifting from government provision to a publicly funded market will certainly change the balance of power in educational politics, but will not make politics go away. The more contemporary studies of market-based education are produced, the more we will understand what might be gained and lost in making the shift.

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