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Book Reviews

Editor's Note: Guidelines for Selecting Books to Review

Occasionally, we receive questions regarding the selection of books reviewed in the *Journal of Economic Literature*. A statement of our guidelines for book selection might therefore be useful.

The general purpose of our book reviews is to help keep members of the American Economic Association informed of significant English-language publications in economics research. We also review significant books in related social sciences that might be of special interest to economists. On occasion, we review books that are written for the public at large if these books speak to issues that are of interest to economists. Finally, we review some reports or publications that have significant policy impact. Annotations are published for all books received. However, we receive many more books than we are able to review so choices must be made in selecting books for review.

We try to identify for review scholarly, well-researched books that embody serious and original research on a particular topic. We do not review textbooks. Other things being equal, we avoid volumes of collected papers such as festschriften and conference volumes. Often such volumes pose difficult problems for the reviewer who may find herself having to describe and evaluate many different contributions. Among such volumes, we prefer those on a single, well-defined theme that a typical reviewer may develop in his review.

We avoid volumes that collect previously published papers unless there is some material value added from bringing the papers together. Also, we refrain from reviewing second or revised editions unless the revisions of the original edition are really substantial.

Our policy is not to accept offers to review (and unsolicited reviews of) particular books. Coauthorship of reviews is not forbidden but it is unusual and we ask our invited reviewers to discuss with us first any changes in the authorship or assigned length of a review.

B Schools of Economic Thought and Methodology

John Kenneth Galbraith: His Life, His Politics, His Economics. By Richard Parker. New York: Farrar, Straus and Giroux, 2005. Pp. x, 820. $35.00. ISBN 0–374–28168–7.

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For twenty years in the middle of the last century, John Kenneth Galbraith was the "best known" living economist. But he was not then, and will never be, regarded as a great economist by economists, because he produced no theories, which is what great economists are supposed to do. Like his hero Keynes, he was an eminent public figure—moralist, controversialist, stylist, wit—who happened to profess economics. But he lacked the theoretical brilliance, or perhaps merely interest, which made Keynes a great economist as well as a public intellectual. Galbraith's sphere was political economy. His interest was not in refining economic theory but in analyzing the way the dominant power structures shape economic behaviour. He believed that by abstracting from politics, economic theory gave an unreal picture of economic life; it also shielded the existing power system from scrutiny, and denied the beneficent possibilities of collective action. The more mathematical, and therefore abstract, economics became, the

more it served the interests of the power holders. Its very precision made it trivially true, but substantively false.

The great value of Richard Parker's admiring, though not uncritical, biography lies in the case he makes for Galbraith's approach to economics, which has long been out of fashion. In truth it adds little to our knowledge of Galbraith the man, who has already written about himself in *A Life in Our Times*, published in 1981. One senses that the desire to pay homage to the still living Galbraith overcame the natural instinct of a biographer to discover more than his subject is willing to tell. Parker falls back on the strategy of offering large chunks of "times" separated by thin slices of "life." This works quite efficiently. The reader who wants a scholarly review of the history of American politics, economics, and economic policy over the near century of Galbraith's life, written from a Democratic Party standpoint, will be well rewarded. He might find out more than he wants to about the politics of Harvard University, but this too was part of the age of Galbraith.

John Kenneth Galbraith was born in Ontario, Canada, in 1908, of reasonably prosperous farming stock. His father's ancestry was Scottish, the name Galbraith being apparently derived from the Celtic "Gail Breaton" or "strange Briton." Parker follows the biographical convention of telling us a lot about Galbraith's father's, but almost nothing about his mother's, forbears, forgetting that ancestral voices come from both sides of the family. In his memoirs, Galbraith skips through his childhood in a couple of pages. Parker tries to make more of it. He suggests that his father, whom Galbraith described as a "moderately well compensated township and country official" set Galbraith a model of political involvement and public service. This may be so, but the real legacy of his childhood seems to have been a revulsion against what Marx called "rural idiocy." He couldn't get out of farming fast enough. One does not have to be a psychological genius to see in the mature Galbraith's provocative but disarming style a way of coping with the effects of early social humiliation—on account of his being a country "hick," of being conspicuously ungainly (he was 6 ft. 10 inches tall), of having an unimpressive speaking voice, and of the inferiority of agricultural economics in the economists' pecking order.

For agricultural economics was Galbraith's way out of farming. He was trained as an economist at Ontario Agricultural College, an institution for which he formed a considerable dislike, and at Berkeley, where he studied the prices of cling peaches, came under radical influences, and discovered Thorstein Veblen. In a passage in his memoirs not quoted by Parker, Galbraith wrote: "Veblen's scholarship was an eruption against all who, in consequence of wealth, occupation, ethnic origin or elegance of manner, made invidious claim . . . to superior worldly position. I knew the mood" (Galbraith 1981, p. 30).

Parker rightly sees the Great Depression as the catalyst for Galbraith's career. In the summer of 1934, it took him into temporary government service at FDR's Agricultural Adjustment Administration, where he got a first-hand lesson in the importance of power in shaping economic outcomes—in this case the power of Southern Democrat landowners to veto New Deal efforts to protect sharecroppers and tenants against eviction. As a young instructor at Harvard, he was nurtured by John D. Black, the head of agricultural economics, his first major patron, who, more than anyone else, was responsible for getting him a permanent berth at Harvard. With Black's encouragement, he wrote a series of papers on agricultural topics which displayed some of his later themes, though not style: the importance of marketing in stimulating consumer demand, and of "lumpy" supply costs in explaining agricultural overproduction. Parker claims that Galbraith's early research into the structure of agriculture left an abiding conviction that "blackboard" models of smoothly adjusting supply and demand curves fail to capture real life relations.

The main effect of the Depression was to give Galbraith, like most economists of his generation, the passionate conviction that it was the job of government to remedy the failings of private enterprise. This became the leitmotif of his career. He was confronted with two alternative explanations of the Depression, which Parker usefully labels "structuralist" and "macroeconomic." The first stemmed from the work of Chamberlin and Robinson on oligopolistic competition, and Berle and Means's analysis of the shift in corporate power from owners to managers. Both could be used to show why oligopolistic, manager-run firms preferred output to

price cuts in a depression. The second influence was John Maynard Keynes. Keynes's *General Theory of Employment, Interest, and Money* (1936) showed how a failure in aggregate demand could produce "underemployment equilibrium." This suggested a major role for government in stimulating total spending, either directly or by cutting taxes.

The way Galbraith interpreted and combined these influences is the key to all his subsequent work. Galbraith accepted Keynes's fundamental insight, but substituted a more disaggregated approach, which emphasised the influence of uneven changes in sectoral wages and prices on the behaviour of aggregate demand. Keynes also influenced Galbraith as a political operator. He saw him as a model of the economist "as an engaged and politically purposive intellectual" (Parker, p. 96). The example of Keynes strengthened his inclination to work through elite social and political networks. He learnt how to get on with the famous, the wealthy, the powerful. He would become the "subversive insider," the licensed critic. He also learnt from Keynes the importance of expressing arguments in good, clear prose.

In 1939, Galbraith got a job at Princeton when Harvard failed to make him assistant professor, but soon got leave of absence to enter Roosevelt's war administration, ending up in 1941 as Commissioner of Prices under Leon Henderson. Two days after Pearl Harbour "he . . . imposed a nation-wide freeze on the sale of new tires and then hastily put in place a rationing scheme . . . " (Parker, p. 145). Eventually he established control over all prices in the United States, with a staff of over 4,000 (one of them was Richard Nixon). It was a heady experience for someone still in his mid-thirties, which did nothing to diminish Galbraith's natural sense of superiority. (He later had a cushion embroidered "Modesty is a Vastly Overrated Virtue.") His paper "The Selection and Timing of Inflation Controls" clearly marks out his distance from Keynes. In "How to Pay for the War," Keynes had argued for a fiscal approach to wartime inflation. Galbraith's line was that structural imbalances in the economy required that price controls and rationing—which Keynes had denounced as instruments of a slave state—had to come before fiscal policy. On the analytic point, Galbraith was right to point out that fiscal policy on its own would be too blunt (and probably too deflationary); what was absent from his paper was Keynes's ardent wish to preserve free prices and freedom of consumer choice even in wartime. With the Republicans strengthened in mid-term Congressional elections and the business press denouncing Galbraith's "communistic tendencies," Galbraith got the sack in May 1943. Price controls were a success, the consumer price index barely budging in the war years. Galbraith wrote up his experience in a short book, *A Theory of Price Control*, which he regards as "one of my more important books." Its failure to get noticed decided him to write in future for a wider audience (Galbraith 1981, pp. 174–75).

Peace brought little diminution in the Democratic Administration's demand for Galbraith's services. As a member of the United States Strategic Bombing Survey, he headed a team of economists set up to assess the Allied air attacks on the German war effort. Its report endorsed the brilliant hypothesis of his assistant Nicky Kaldor that "Allied bombing . . . by destroying civilian enterprises, released labour for the [German] war effort"—a classic instance of the law of unintended consequences. Soon Galbraith was off to Japan to assess the effect of aerial destruction there. In 1946, he got a job at the State Department to help reconstruct Germany. He quit after six months because he disapproved of the move to the Cold War and confrontation with the Soviet Union. His experience provoked an amusing diatribe about the "secular priesthood" which controlled foreign policy (Galbraith 1981, pp. 242–44). Service on a further commission, in 1950, on German refugees, convinced Galbraith that migrations were the key to the solution of the world's poverty problem; a curious vestige of belief in the efficacy of market forces.

In between, Galbraith had served as a feature writer on *Fortune* magazine. Its millionaire proprietor Henry Luce, a visceral opponent of FDR, but an admirer of Keynes and a visionary advocate of "The American Century" (now postponed to the present one), was his second major sponsor. He later told J. F. Kennedy "I taught Kenneth Galbraith to write. And I can tell you I've certainly regretted it ever since" (Parker, p.152). Galbraith educated businessmen in Keynesian economics and *Fortune* educated Galbraith in the realities of corporate power. This, he said, immunized him forever from "the mythology of

the neoclassical textbook economics and its image of competitive firms" (Galbraith 1981, p. 268). Parker argues, though, that in helping to convert many of the nation's most powerful business leaders to Keynesian economics and "The American Century," Luce and Galbraith helped seal the capitulation of Keynesian economics to business and military priorities.

In 1949, with John Black again pulling the strings, Galbraith finally got a Harvard chair in economics, over the strenuous opposition of Gottfried Haberler and several of the Overseers, who thought he was much too left-wing. (He got his revenge years later when, in a speech, he attributed Austria's postwar economic success to the flight to the United States of distinguished members of the Austrian School.) With Eisenhower and the Republicans back in power, Galbraith used his enforced rest from government service to write his three most important books and a minor gem, *The Great Crash* (1954).

Galbraith's writings from this point onward cease to be "academic" and start to deal with big themes, like the future of American capitalism. He was an admirer, critic, and analyst of that quintessential American institution, the business corporation, and set out to show how corporations interacted with other institutions to shape the "American system," and indeed America's role in the world. Although he came to praise, not bury capitalism, he was also a left-wing Keynesian who believed that large public expenditures were needed not just to balance aggregate supply and demand at high employment, but to tilt the balance of demand toward public goods. In Britain and western Europe he would have been a non-Marxist social democrat. In the United States this slot did not exist and Galbraith had to stake out his own position within the "liberal" camp.

He rejected "Eisenhower" Keynesianism, whose limited aim was to stabilize the business cycle, and which sanctioned increases in public spending only for defense purposes. As Parker rightly says, it was government defense contracts which reconciled corporations to Keynes. Galbraith wanted liberals to regain control over the public agenda.

Galbraith also stood apart from the "theoretical" Keynesians led by "Harvard wunderkind" Paul Samuelson. This group's two goals, as described by Parker, were "to bring postwar economics to an entirely new level of mathematical sophistication by incorporating econometrics, linear programming, computers, and game theory," and to "hybridize Keynesian macroeconomics with the older Marshallian microeconomic legacy into what Samuelson christened the 'Neo-Classical Synthesis.'" Galbraith rejected this attempt to make economics like physics. Mathematical Keynesianism, he thought, was grossly oversold. Because of its sophistication and precision, its advocates convinced themselves that they could deliver whatever they wanted, forgetting that Keynes had treated uncertainty as a given condition of economic life. Apart from this, Galbraith was chiefly distinguished from other Keynesians by stressing the problems the oligopolistic structure of the U.S. economy posed for macroeconomic managment. This explains his persistent advocacy of wage and price controls: he never believed that fiscal policy alone could deliver full employment without inflation.

The aim of his first big book, *American Capitalism* (1952), was to put the defense of free enterprise on a more secure basis. Traditional defenses of the private market system emphasised its integral connection with liberty and efficiency. Galbraith denounced these claims as "cant," because the economy of small, intensely competitive, privately owned firms which it assumed had long been killed off by the growth of oligopoly and managerial control. But this did not mean that a pragmatic defense of decentralized decision-making could not be offered. Bigness offered clear advantages in achieving technological innovation, and the system of large institutions generated its own corrective in the form of "countervailing power." Galbraith explained that "in the typical modern market of few sellers, the active restraint [on the exercise of economic power] is provided not by competitors but from the other side of the market by strong buyers." Corporate power summoned into existence the countervailing power of labor unions, retail chains, and other large producers. Power was checked by power. This brilliant insight, with its simultaneous refutation of Marx and Marshall, owed much to the characteristic American political theory of pluralism. Parker points out that it seemed to offer little scope for government leadership, a failing which Galbraith later recognized. He also came to feel that it had been too complacent: countervailing power often did not emerge; producers colluded to push up prices. But the

book had enormous influence on younger social democrats in Europe trying to wean their parties off dogmatic faith in nationalization.

Galbraith's attempt to display the free enterprise system as it really was and not as textbooks said it was owes a great deal to his Harvard colleague, Joseph Alois Schumpeter. Schumpeter's importance for Galbraith was to de-link the defense of capitalism from the competitive, neoclassical faith. His hero was the creative entrepreneur; the capitalist system was marked not by smooth adjustments but by "gales of creative destruction." "Even monopoly," ran Galbraith's commentary on Schumpeter, "could be tolerated, for it allowed its possessors the rewards of their innovation as the competitive model did not." (Galbraith 1981, p. 50) Schumpeter's influence on Galbraith's own strategy for defending capitalism is clear. Critics replied that he was either wrong on his facts (business concentration was less than he claimed) or that his facts did not invalidate the competitive model; often simultaneously. However, the real reason why Galbraith's "defense" of capitalism left true believers cold was that the "cant" which links private enterprise to freedom and efficiency is an essential, and not contingent, part of their faith. True believers prefer dogma to pragmatism, because pragmatism is necessarily provisional. By the time he came to write *Economics and the Public Purpose* (1973), a disillusioned Galbraith had embraced socialism as the answer to America's problems. *American Capitalism* offered no principled reason why he should not.

Galbraith's best book, *The Affluent Society*, was written in the Swiss ski resort of Gstaad in 1956, a charming place to ponder the paradoxes of affluence and register a protest against the "growthmanship"—the absolute importance attached to increasing the size of GNP—that was starting to dominate the thinking of economists. To satirise this thinking, Galbraith invented the phrase "the conventional wisdom." He conveyed the contrast between "private affluence and public squalor" in highly mannered prose:

> The family which takes its mauve and cerise, air-conditioned, power-steered, and power-braked car out for a tour passes through cities that are badly paved, made hideous by litter, blighted buildings, billboards, and posts for wires that should long since have been put underground. They pass on into a countryside which has been rendered largely invisible by commercial art . . . They picnic on exquisitely packaged food from a portable icebox by a polluted stream and go on to spend the night at a park which is menace to public health and morals. Just before dozing off on an air-mattress, beneath a nylon tent, amid the stench of decaying refuse, they may reflect vaguely on the curious unevenness of their blessings (Galbraith 1956, p. 253).

The main idea of the book was not new. The time would soon come, Keynes had argued in 1930 (in his essay "Economic Possibilities for Our Grandchildren"), when society would need to accommodate its psychology to plenty, not scarcity. Given affluence, more and more people would (or should) opt for leisure, free time, and intellectual achievement rather than more consumption. Galbraith simply said this "stationary state" had come—at least in America. America was suffering from the ills of affluence, not poverty. With a declining marginal propensity to consume, new wants had to be constantly created by advertising. Galbraith's most incisive idea, again not original, was that growing private consumption required matching spending on public services—what he called a "social balance" if it was not to foul up cities and countryside and reduce people to idiocy. An increasing fraction of growing national income should be raised in taxes and spent in correcting the "social imbalance" created by the private consumption boom. Production should be tested for its environmental effects.

The Affluent Society was praised for its ethics, but not for its economics. The late Harry Johnson argued that Galbraith had made the "gross theoretical error" of assuming that "the marginal utility of present aggregate output, ex advertising and salesmanship, is zero" (Parker, p. 295). This is absurd. Galbraith was not arguing that all wants were contrived, but that the balance between private and public consumption needed to shift. Ronald Reagan and George W. Bush took the opposite, tax-cutting road. As hurricane Katrina reveals the folly of starving public services, public opinion may shift back to Galbraith's way of thinking.

Events have dealt an even more intriguing blow to Galbraith's hopes than presidential policy. Like a good classical economist, he believed in the law of diminishing marginal utility. As people

got richer each extra dollar of income would give them less pleasure. How then is one to explain the fact that hours of work in Britain and the United States have scarcely fallen since 1960, though their societies have grown much wealthier? Is it because economists are simply wrong about human nature: that as people's consumption expands they want more not less? Or is it because advertising turns us into consumption addicts? Or is it because globalization has made affluence too insecure and too uneven in its spread for most people to ease off work?

Galbraith's fertile decade of enforced rest from government service culminated in his third major book, *The New Industrial State*, largely written in the late 1950s, though not published till 1967. This is his most ambitious, systematic attempt to describe the American system as it was, rather than as depicted in textbook models of perfect or imperfect competition. Its main propositions, as summarized by Robert Solow in a critical review cited by Parker (Parker, pp. 439–41), were:

1. The U.S. landscape is dominated by giant corporations because of advantages due to size (economies of scale) and because the cost of advanced technological investment requires extensive bureaucratic organization. Economists had failed to realize that these behemoths undermined their idealized model of competition.
2. Managers ("the technostructure"), not shareholders, shape the culture and goals of the corporation.
3. The technostructure substitutes for profit maximization "planning for security" through manipulating wants, supplies, and the regulatory system.
4. Planning and risk reduction dictates vertical integration, internal financing, and the cooption of labor unions (no longer a "countervailing power").
5. The constant stimulation of consumer wants mis-shapes the demand for private and public spending.
6. The giant corporation suborns the "educational and scientific estate," which should be independent, to its goals.

The New Industrial State was a noble failure—too wordy, too sweeping in its claims, too much a product of temporary conditions. Solow's attack concentrated on Galbraith's central claim that the corporation was independent of the market. Here Parker concedes reluctantly that "Solow had the advantage" even though neither economist anticipated America's "financialization" or the sweep of globalization. Investment funds of all sorts "have come to define a new competitive market-place that is in some ways more traditional than the one described in *The New Industrial State*" (Parker, p. 443). In *The Good Society* (1996), a chastened Galbraith implicitly acknowledged that his description of American capitalism was obsolete. "Monopoly power" had "surrendered to international competition and the explosive force of technological change" (Galbraith 1996, p. 16). Modern corporate management is "committed to . . . profit maximisation." Far from being immortal, even the largest firm is subject to the discipline of actual and prospective bankruptcy. Contrary to the literature on the managerial revolution, managers have started behaving like owners again because much of their compensation now comes in the form of stock grants and options.

In his reply to Solow, Galbraith brought the debate back to the question of how to do economics. Solow wanted it to be viewed as a scientific project, consisting of testable mathematical hypotheses. This depended, Galbraith insisted, on the assumption that wants originate in the individual, and are efficiently translated by the market to profit-maximizing firms, leaving the consumer sovereign. If in fact, the individual is not sovereign, but "the instrument or vessel of those who supply him" (Parker, p. 448), economics will need to be different. "The work of economists will will be far less precise, far less elegant, seemingly far less scientific than those who are fitting pieces into the [accepted] structure" (Parker, p. 449). There is more than an echo of Keynes's plea that pioneers of thought should be given the benefit of a sympathetic hearing.

Parker uses the Galbraith archives to greatest effect to document his subject's involvement in Democratic politics. In particular, he makes a strong case that Galbraith had more influence on President Kennedy than has been acknowledged. This is hard to reconcile with Kennedy posting him to India as ambassador from 1961 to 1963, which Galbraith describes, with commendable candour, as an enjoyable interlude of disguised unemployment.

Galbraith wrote speeches for Adlai Stevenson in 1952 and 1956. After the first defeat, he helped

form a brains trust to educate Stevenson in the "new economics." The so-called Finletter Group had little effect on Stevenson, but is credited with supplying the thinking for the New Frontier and Great Society programmes of Kennedy and Johnson. Following the Soviet success in launching Sputnik in 1957, Kennedy jumped onto the "growth" bandwagon. Talk of the "output gap" became all the rage, and Kennedy recruited Democratic economists to tell him how to close it. He spent more and more time with Galbraith in Boston and Cambridge. Galbraith, Parker writes, became part of the inner circle, just outside the Irish mafia. He knew how to play the courtier, and was master of the entertaining letter. He once wrote to Kennedy that communicating with him through the State Department was like "trying to fornicate through a mattress" (Parker, p. 351). In more elevated mood, he wrote President Kennedy's famous inaugural speech line: "Let us never negotiate out of fear, but let us never fear to negotiate" (Parker, p. 347).

In fact, Galbraith could not give Kennedy what he wanted. He was never entranced by "growthmanship," thinking, correctly, that its single-minded pursuit would lead to inflation. "Growth with balance" was the theme of speeches he wrote for Kennedy. He criticized Kennedy's "New Economists" (Samuelson, Tobin, etc.) for ignoring the significance of the armed forces in federal spending (Parker calls it their "self-censorship on instrumentalities") and for their mathematical hubris, particularly their reliance on a stable trade-off between inflation and unemployment. As Tobin put it, "We thought we could not only make the bicycle go faster but keep it at high speed and determine the path it would follow" (Parker, p. 344).

Galbraith tried to steer the President away from reliance on military-led growth to the ideas of *The Affluent Sociey*. Tax cuts should be resisted as they prevented targetted investment. "Once we start talking about tax cuts we will take the pressure off the rest of your program . . . We now say that housing, school building and urban renewal are needed both for themselves and for their effect on employment. Given the tax cut conservatives will not be slow to say that this will do the job" (Parker, p. 360). However, with the weight of solidly reputable economic opinion on the other side, the outcome was never really in doubt and taxes were reduced in 1964.

Parker's case for Galbraith's influence rests not on his economic advice, but in causing Kennedy to hesitate over commiting U.S. troops to Vietnam.

Galbraith's economic strategy, involving reduction of U.S. overseas military spending, was incompatible with military escalation in Vietnam. An important thread running through Parker's book is that Galbraith never abandoned the Roosevelt position of active negotiations with the Soviets. Galbraith's dissent from the "secular priesthood" on this issue determined his opposition to "military" Keynesianism. For Galbraith, there were no differences so intractable, threats so mortal, as to justify the diversion of huge resources from social programs to the arms race. His rejection of Cold War thinking was influenced by the fashionable left-wing theory of convergence. Following a visit to the Soviet Union in 1959, Galbraith became convinced that the industrial systems of the United States and the USSR were becoming increasingly similar. Detroit and Sverdlosk, he thought, were twin expressions of a common engineering culture which would force the development of compatible political forms—a striking example of misplaced technological determinism. The idea that the United States should spend its lives and treasure to prop up a gangster government in Saigon simply on the ground that it was anticommunist struck him as politically inept and morally outrageous.

So from the start of the Kennedy administration, Galbraith resisted arguments from State and Defense and top military personnel like General Maxwell Taylor that 100,000 or 200,000 troops needed to be sent to Vietnam. This resistance fitted Kennedy's instincts. While Kennedy was president, the most he would agree to was to dispatch 8,000–10,000 noncombatants called "flood relief specialists." But the pressure for combatant involvement never abated. "The situation in South Vietnam is perilously close to the point of no return" Galbraith minuted the President on 3 November 1961, saying that the regime of Ngo Dinh Diem was hopeless and that the United States should probe the Russians for the possibility of a negotiated peace. In April 1962, he sent another memorandum to Kennedy urging him to abandon the strategic hamlets and chemical defoliant approach and, above all, resist "all steps which commit American troops to combat roles" (Parker, p. 389). Not the least interesting part of

Parker's narrative is the disclosure of how Kennedy's instructions to limit U.S. involvement were continually undermined by officials who wanted to expand it. Parker believes that, by 1963, Galbraith's views had persuaded Kennedy who, in the month before his assassination, made U.S. withdrawal from Vietnam a formal policy objective. President Johnson reversed Kennedy's policy, committing hundreds of thousands of American troops to Vietnam. Galbraith tried once more. In a private memorandum to LBJ in January 1966, he urged abandonment any attempt to reconquer Vietnam, withdrawal to defendable "enclaves," and the start of negotiations.

When Johnson rejected this advice, Galbraith came out openly against the Vietnam war. His 1967 paperback, *How to Get Out of Vietnam*, sold 250,000 copies. As chairman of the Americans for Democratic Action, he campaigned vigorously on a platform of immediate negotiations and supported Eugene McCarthy as the antiwar candidate for the Democratic nomination. Galbraith's alternative was respectable, rather than heroic. In his memoirs, he writes: "There was need to have a solution that was . . . acceptable to the largest possible political constituency in our country. Simply to leave was not" (Galbraith 1981, p. 491). There was another motive, the desire to protect the South Vietnamese loyalists: "We cannot simply write them off; even by majority vote we do not assign people to the sanguinary attention of their enemies" (Galbraith 1981, p. 491). Unhappily wrong: democracies do it the whole time.

Galbraith's opposition to the Vietnam war was his finest public stand. There was no one able or willing to play this role at the court of Bush or Blair in 2002–03 where the invasion of Iraq was plotted. It is not that such people were not around. But Bush and Blair wanted to hear arguments for the war they wanted, while Kennedy wanted arguments against the war he dreaded. So he solicited Galbraith's views, and used them to confront his official advisers. Had he not been killed, history might have been decisively different.

After 1967, Galbraith faded away politically and intellectually. Johnson was the last president to ask for his advice. (LBJ threw away one of his drafts for a speech with the remark: "Did y'ever think, Ken, that making a speech on economics is a lot like pissing down your leg? It seems hot to you, but it never does to anyone else"). With the election of Nixon in 1968, politics shifted to the right and the era of Republican dominance was inaugurated by Ronald Reagan's two victories in 1980 and 1984. Although Nixon announced mysteriously in 1971 he was "now a Keynesian in economics," the Keynesian moment had passed, never to return, buried by the Vietnam war, inflation, and civil unrest. Monetarism and "rational expectations" were the new doctrines which mainstream economics used to cut Keynesianism out of its system. Galbraith was elected President of the American Economic Association in 1971. In his valedictory address, "Power and the Useful Economist," he restated his credo. The problem of economics was its "wilful denial of the presence of power and political interests." By insisting that consumer sovereignty and a sovereign citizenry rule the roost, "conventional economists have presumed away the most compelling questions of our time — whether about growth, inequality, global development, the environment, resource use, or consumer protection" (Parker, p. 505). Galbraith's remarks were warmly received, but the new guru was Milton Friedman. Galbraith's issues did not go away, but they were now handled by different tools. He continued to write books and coin phrases — "the culture of contentment" was vintage late Galbraith—but ceased to count.

With such a towering figure as Galbraith, it is hard to avoid reviewing the man rather than the biography. Let me end with some final thoughts about both.

Parker admires Galbraith as a stylist. He rightly says that economists took as much exception to his style as to his substance, and particularly to the book sales which it commanded. But although Parker gives an adequate sprinkling of examples, there is disappointingly little analysis of Galbraith's technique of writing, and the kind of authorial presence it was intended to convey. Of the genesis of "conventional wisdom," Galbraith has written: "I needed a phrase that was overtly respectful but with an undertone of disdain, even amusement. Something nicely balanced between approval and ridicule" (Galbraith 1981, p. 339). In other words, Galbraith's style was crafted to give scope for extensive criticism without surrendering his place in the elite. Disdain, sugared with humour, gave it its familiar urbanity. One does not expect an economist to be

a literary critic, but Deirdre McCloskey has written persuasively about economics as a form of rhetoric, and it would have been useful to have had a discussion of Galbraith as a rhetorician.

Parker amply documents Galbraith's dislike of mathematical models, but gives us no idea of whether he was any good at math. While biographers feel free to discuss the most intimate secrets of the body (at least when their subjects are dead), the secrets of the mind are rarely analyzed. In fact, it is considered bad taste to raise the subject of intellectual competence. It is assumed that all major thinkers are intelligent enough, but in what exactly does their intelligence lie? With economists a special problem arises, since math is at the top of their pecking order of esteem. The economists' jibe that critics of mathematical models are so only because they are incompetent at mathematics is partly true; but it is also true that those who are good at math seek to emphasize their comparative advantage by vastly exaggerating its importance for economic understanding. At least, I have never come across any mathematical economist who was good at political economy, and vice versa. So we may assume that Galbraith, who was certainly very intelligent, lacked the particular facility in math that would have given him a more benevolent view of mathematical economists.

Parker is sympathetic to the attempt to reconstruct economics along sociological lines. This is obviously doomed to failure. Without abstraction (organising principles) there can be no sociology, just random description. Without economics there can be no political economy, just politics, or ethics. Galbraith's depictions of contemporary American capitalism are not theoretical constructs. But without the economic theory of the market, he would not have known how to shape his analysis. The danger against which Galbraith rightly warned is excessive mathematicization, which produces the illusion that all the significant data of economic, and even human, experience can be fitted into a mathematical model. Fortunately, popular mistrust or ignorance of mathematics is enough to ensure vigorous resistance to this tendency.

Galbraith has often been compared to Keynes, and certainly they had much in common. But the way they did their economics was very different. Keynes produced theories, Galbraith, theoretically-inspired sociology. Keynes thought that in the end ideas ruled the roost, Galbraith thought it was structures of power. He looked for amelioration of social conditions not to the triumph of the better idea, but to the coming into power of his class—the university-based "educational and scientific estate." While Keynes thought of the battle of ideas, Galbraith thought in terms of the battle between the "educational and scientific estate" and the corporate technostructure for the control of the state. His is a non-Marxist version of class struggle with the intelligentsia substituted for the proletariat as the engine of "social innovation" and carrier of the "public purpose."

This reductive attitude explains why Galbraith cannot write well about ideas. For him they are simply sociological variables, which fit the needs, circumstances or technological conditions of the day. Herbert Spencer's sociology fitted the needs of nineteenth-century American capitalism; a certain version of Keynesianism fitted the needs of big business in Galbraith's day, and so on. And this low esteem in which Galbraith held ideas is despite the fact that, as one colleague put it, "Ken's major attribute . . . is ideas."

In much the same way, Galbraith treats freedom as a sociological variable. There is, of course, a fundamental connection between belief that ideas are freely produced and belief in freedom in general. Galbraith took the freedom of his own circumstances for granted, but no passion for freedom runs through his work. There is nothing in his writing on the Soviet Union to indicate that freedom rather than efficiency is the issue between the two sides. In supporting Galbraith's foreign policy positions, Parker gives figures of the huge cost of the Cold War, but does not say whether or how the Cold War might have been averted.

Parker writes of Galbraith's political position: "A pragmatic balance between the opposing ideological ideals lay at the heart of Galbraith's reasoning. The market has value, but so does the state when it acts in the democratic interest" (p. 133). There is a huge unexamined assumption in that last clause. What is that interest? How is it to be discerned? How is the state to be held to it? Galbraith was clear that the existing power holders distorted the economy for their own ends; but he was blind to the "public choice" critiques of so-called democratic policy-making, and even blinder to the fact that governments have vastly greater powers over the lives of citizens than do

private bodies. His position seemed to be that as long as the Democrats are in power and advised by the right people the state can be trusted. This is dangerously close to the Marxist belief that the problem of the abuse of power, and the need to build safeguards against it, would disappear when the dictatorship of the proletariat was established.

This cavalier attitude to the problem of power marks Galbraith as an authentic product of the first half of the last century, for it was widely shared by that generation of statist-minded intellectuals. Their worldview was shaped by the contrast between the transformative possibilities of science and the incompetence and obsolete ideas of the power holders—an ineptitude which had produced the calamities of the first world war and the Great Depression. Their goal was to improve the condition of the people by expanding the reach and raising the mental efficiency of the state. It was a worthy aim and its pursuit was attended with considerable success as well as notable failures. But conditions have changed, and with it the relevance of much of this thinking and therefore of Galbraith's writings. *The Affluent Society* will live on because the questions it discusses are timeless. The wit and wisdom of Galbraith will continue to delight. To those who aspire to public service he will remain a model of a "public intellectual." But all things considered, he was a man both in and for his time.

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F International Economics

Building a Dynamic Europe: The Key Policy Debates. Edited by Jordi Gual. Cambridge; New York and Melbourne: Cambridge University Press, 2004. Pp. ix, 170. $45.00. ISBN 0–521–82734–5. *JEL 2004–0921*

The European Council meeting in Lisbon in 2000 adopted a strategy with the aim to make the European economy the world's most innovative and dynamic knowledge-based economy within a decade. This volume consists of an introduction and five chapters, and brings together five papers from a conference in the fall of 2001 that purport to address key policy issues for achieving the goal set at the Lisbon meeting.

It is somewhat unfortunate that this review comes four years after the conference and five years after the Lisbon meeting. If the Lisbon strategy had any credibility when it was adopted, that credibility has by now evaporated. The performance of the European economy has been dismal compared to its yardstick competitor, the U.S. economy. Moreover, the new constitution has been effectively stopped by French voters, and the European Union has recently failed to agree on a proposed long-term budget, which, incidentally, would have meant a very substantial cut of the research budget in favor of maintained agricultural and regional subsidies.

Gérard Roland's contribution to the volume is a discussion of the tradeoffs between presidential and parliamentary regimes in the context of the European Union. The inherent advantage of a parliamentary system compared to a presidential system is that it tends to produce stable and broad coalitions, whereas a presidential system tends to produce shifting and more narrow coalitions. A parliamentary system therefore tends to be more efficient in providing Europe-wide public goods. Its inherent disadvantage is that it is less effective in carrying out executive tasks. One contribution of the chapter is to place this discussion in the precise context of EU governance, and to suggest measures in that context to mitigate various disadvantages of a parliamentary and a presidential regime respectively. The chapter was written before the proposal of new European constitution by the Constitutional Convention; the reader would have liked to know if and how the proposal could have helped to achieve the goals of the Lisbon meeting.

Assar Lindbeck contributes a nuanced and balanced discussion of welfare systems in Europe, from child care to employment and income security, sick-leave insurance and health-care, and pensions and care for the elderly. The common theme is that the welfare systems are vulnerable to financial stability in response to shocks, undesirable behavioral adjustments in response to the arrangements themselves, and changes in socioeconomic conditions. He discusses various measures to make the systems less sensitive to shocks and socioeconomic conditions, and thus to make changes less politically sensitive, and the need to design systems that minimize moral hazard problems and benefit cheating. Lindbeck notes that the possibilities to improve European welfare

systems are great, but that the question is whether politicians have the courage and skill to obtain support to make the improvements.

David Newbury's chapter deals with the creation of an integrated and liberalized market for gas and electricity in Europe. He is concerned about the balance between liberalization on the one hand and adequate capacity and investment on the other. Investments in productive capacity are large and long-term, and investors may require retail franchises to ensure long-term sales. Consumers want stable prices and security of supplies. These things are not automatically provided by a fully liberalized market. He is at the same time concerned that national competition authorities will be unable or unwilling to resist the ongoing vertical and horizontal integration taking place in Europe, which may be detrimental to competition.

Francesco Giavazzi discusses four questions about macroeconomic policy in the EMU: Should monetary and fiscal policies be coordinated? The answer is that no coordination is needed if monetary and fiscal authorities do what they are supposed to do. Should we worry about inflation differentials and current account imbalances? The answer again is no; inflation differentials are part of the adjustment process, and current account imbalances reflect welfare increasing intertemporal reallocations or intergenerational redistribution and should be left to member states. Should the Growth and Stability Pact be reformed, particularly to allow larger fiscal deficits caused by public investment? The answer here is yes. It seems that Giavazzi has succeeded in convincing policymakers. The changes in 2005 to the GSP, following the failure of Germany and other countries to keep deficits within limits, allows for greater deficits when caused by public investment. Finally, Giavazzi argues for changed voting rules in ECB before the enlargement in 2004, on the assumption that a larger number of members will seriously hamper decision making.

Jordi Gual looks at the effects of EU policies to integrate the banking market. He finds that, at the retail level, the effect has not been improved performance, but an increase of the concentration in national markets by domestic banks, as a way to cut costs and protect market power from outside competition. He observes that in some instances (Italy?) political interference has prevented cross-border consolidation and helped domestic concentration. Also, exceptions to the mutual recognition principle have been allowed for the protection of the common good. The exceptions may prevent cross-border integration by creating barriers to standardization of products and can be used to protect the domestic market. Gual thinks that further integration is unlikely as long as important differences in company law, contract law and fiscal matters remain.

The authors of the five chapters are leading in their respective areas of expertise and the policy issues they deal with are important for the European economy. One may, however, question whether these are *the* key issues, as the title says. If I were to choose five key issues, the labor market and the way it functions in Europe would certainly be on the list. This volume is silent on the distorted labor markets in Europe (with the minor exception of Lindbeck's contribution) and the barriers to labor mobility within the Union itself. Another issue that is missing from my list is education, particularly at the university level. Europe has a long way to go before its university system becomes as dynamic and successful as that of the United States. The Bologna model, which is designed to increase national and international mobility of university students, is a step in the right direction, but not sufficient to close the gap. More efficient labor markets, including full mobility within the European Union, and more efficient university education are probably key to reach the goals set at Lisbon as much as some of the issues dealt with in this volume.

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Dollar Adjustment: How Far? Against What? Edited by C. Fred Bergsten and John Williamson. Special Report 17. Washington, D.C.: Institute for International Economics, 2004. Pp. viii, 294. $21.56, paper. ISBN 0–88132–378–0.

JEL 2005–0569

In today's times, the single most debated question in international policy circles is the fate of the U.S. dollar and the U.S current account. This book is a collection of essays on this debate. The papers were presented at a conference in Washington, D.C., on May 25, 2004, organized by the Institute for International Economics. The main questions that were asked at the time of the conference were the following: (1) How much further does the dollar need to depreciate? (2) Against which currencies does it need to depreciate? The general

consensus among the authors is that there is a serious misalignment of key national currencies in the world. The main conclusion is that the dollar needs to decline by an additional 15 percent or so and most of this adjustment needs to take place against Asian currencies. This will require China to revalue its exchange rate against the dollar by around 20 percent.

Chapters 2–4 deal with the question of the size of the dollar adjustment needed. To provide a precise answer to this question is clearly impossible. This is more so given that there is very little guidance from theory about what it means to have a misaligned exchange rate. There are two approaches taken here, which are common in the literature. The first is to define a "sustainable" current account. Next, the idea is to calculate bilateral exchange rates that are consistent with exogenous measures of a "sustainable" current account. A second approach is to empirically estimate a relation between the real exchange rate and its fundamentals using historical data. The misalignment is then the difference between the actual exchange rate and the exchange rate provided by the permanent part of the model.

Since there is no agreed upon number for the sustainable level of the U.S. current account deficit, the authors consider several possible target values for the current account. The numbers involve reducing U.S. current account deficits from over 4.6 percent of GDP to 3 percent, 2.5 percent, 2 percent, and 1 percent of GDP. The next question of how much should the dollar depreciate by, bilaterally, depends on one's conjectures of which countries should face the brunt of the current account adjustment and convert their surplus positions into smaller surpluses or even deficits. Again this calculation involves considering several possible scenarios for adjustment. The point that is made here is that the adjustment should be spread across several countries and should not be borne solely by Europe. Moreover, China and other countries in Asia, especially the newly industrialized countries, need to play a central role in this adjustment.

The second set of essays deals with the impact of a large dollar adjustment on the United States and its trading partners. Once again, there are several back of the envelope calculations made and suggestions that the U.S. deficit be brought down to around 2 percent of GDP. This would require fiscal consolidation in the United States and an effort to raise demand growth in Japan and Europe. There are several case studies of countries. Chapter 7 discusses the consequences for Canada of a U.S. current account and currency realignment. Chapter 8 discusses the Japanese macro economy and its exchange rate policy and monetary policy in the period 2003–04. Japan's motivations for intervening in its currency markets are highlighted, including the need to prevent a premature appreciation of the yen in the midst of a weak economy. A nice effort is made to calculate the effect of an appreciation in the yen's real effective exchange rate on Japan's trade. They arrive at the following estimate: a 10 percent appreciation in REER in the medium run will cause a decline of 0.1 percent in the export/GDP ratio.

In chapter 9, there is a discussion of China and the Renminbi exchange rate. The Renminbi is found to be significantly undervalued, by around 15 to 25 percent. An argument is made for the need to allow the currency to appreciate, not just to benefit the rest of the world, but also China. This is so as to prevent the economy from overheating and to hasten the much needed banking reform in China. The last set of essays discusses the use of sterilized intervention to achieve desired currency values. There is some positive evidence in this regard for yen interventions.

The essays are a valuable source for students and researchers interested in policy level discussions of currency and current account misalignments. There are several tables and back of the envelope calculations that provide a useful understanding of these issues. The country studies of Canada, China, and Japan are useful for those seeking quick and easy facts on the exchange rate and current account policies of these economies.

The essays clearly are not meant to break new theoretical ground on our understanding of these issues. Also, it is not the place to look to for a compelling study of what a "sustainable" current account is or what its means to have a "misaligned" currency. To this extent, the methodologies used in the various chapters are idiosyncratic and vary depending on the authors' persuasion. In the absence of a theoretical framework to discuss the estimates generated, there is room for some skepticism regarding the conclusions.

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Exchange Rates Under the East Asian Dollar Standard: Living with Conflicted Virtue. By Ronald I. McKinnon. Cambridge and London: MIT Press, 2005. Pp. x, 279. $35.00. ISBN 0–262–13451–9. *JEL 2005–0971*

This book consists of a set of essays on East Asian exchange rate regimes, mostly written jointly by Ron McKinnon with younger colleagues and previously published. In the preface, he writes "This book develops a conceptual framework to show where conventional economic thinking on these important exchange rate financial issues has gone off the rails." It is not easy to summarize his main arguments, but perhaps the following comes close to a reasonable summary.

The East Asian countries—including China and Japan—are now highly integrated in both trade and capital movements, and are thus an optimum currency area. But the area is not ready for monetary union, and a currency area similar to the European Monetary System (which was anchored on the DMark) would have the problem that neither the yen nor the remninbi are suitable anchors. The dollar has thus become the anchor.

While welcoming the policies of those authorities that fix their currency to the dollar (Hong Kong, Malaysia, and, at the time he was writing, China) he does not advocate necessarily fixing the exchange rates with "hard pegs." He favors a concept of "soft pegs," which seems to allow for limited exchange rate commitment only. One reason is that fixing an exchange rate to the dollar is only desirable if other currencies in the region are also fixed to the dollar, and that cannot be assured. But he does want currencies to be committed over a longer period to be stable relative to the dollar—so that expectations will be regressive.

The principal reason for his recommendations has to do with the financial system. He says that "this book focuses on cross-border financial claims." Developing countries normally cannot borrow or lend other than in terms of the dollar (or other major currency); they cannot denominate their cross-border debts or claims in terms of their own currencies. Furthermore, hedging for the private sector is difficult. This means that both appreciation and depreciation of the exchange rate will create problems for the private financial sector. Appreciation will reduce the domestic-currency value of foreign assets and depreciation will increase the domestic-currency value of foreign liabilities. It is well known that the latter created a big problem in the Asian crisis. All this means that exchange rate instability generated by volatile expectations destabilises the private financial sector and inhibits capital mobility.

There is of course much more to be said, but this argument for exchange rate stability is central to the book. In fact, since the recovery from the Asian crisis, most of the countries in the region other than Japan have tended to establish "soft pegging" regimes using the dollar as the anchor, just as they had before the crisis, and McKinnon's argument may give us one explanation. Thus McKinnon and his collaborators are providing a rationale for what can be observed. But there is a problem (or one of a number of problems).

The "loose cannon," as they put it, is the yen–dollar rate, which has certainly not been stable or stabilized. Several chapters in the book deal with Japan. The yen is the only currency in the region that really floats, and they would like it not to do so. Japan certainly does not fit into the story. Indeed, a variable yen–dollar rate has created difficulties for the regimes of the other East Asian countries, since trade with Japan is very important and fixing, or near-fixing, their currencies to the dollar certainly does not stabilize the various exchange rates relative to the yen. They advocate that the yen–dollar rate should be "permanently tethered." That would have two benefits. It would make it easier for the dollar standard of the other countries to have a stabilizing effect. In addition, it would, they argue, benefit Japan by eliminating expectations of yen appreciation, and thus will keep nominal interest rates in Japan high, or at least equal to U.S. rates. That, in turn, will help to eliminate Japan's "liquidity trap." I regard this idea of "tethering" the yen as an unrealistic recommendation.

The book can be read as an advocacy of a particular kind of exchange rate regime—one which does not necessarily involve a "hard peg" but definitely does not involve floating. Alternatively, it can be read as an interesting account of East Asian exchange rate policies and related matters, with emphasis on the financial sector and, above all, on cross-border borrowing and lending. It is not easy to read this book, but my main problem with it is that there is no explicit discussion of why regimes other than the one advocated are not desirable. What is wrong with "conventional" views?

Policy decisions in this and other fields usually involve a balance of considerations and it would help if the authors had explicitly explained where "conventional thinking" has "gone off the rails." For a large economy such as that of China or Japan, or even the Republic of Korea, is there not some argument for monetary independence? Should policy never try to influence the real exchange rate through policy variations in the nominal rate? I also have some other questions. What are the qualifications implied in the "soft peg" recommendation compared with a "hard peg"? Do they believe that the "hard peg" choices of Hong Kong and Malaysia are unwise? How should countries react to shocks? I think that for short-term shocks the authors are ready to allow countries to give up pegging, provided that eventually the pegs are restored. This would then lead, desirably, to regressive expectations that will avoid destabilizing capital movements. They draw a parallel here with the gold standard. Therefore the recommendations must be regarded in this respect as quite pragmatic. But the yen–dollar problem remains, and eventually the remninbi is also likely to float.

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Globalization: What's New? Edited by Michael M. Weinstein. A Council on Foreign Relations Book. New York: Columbia University Press, 2005. Pp. 279. $64.50, cloth; $22.50, paper. ISBN 0–231–13458–4, cloth; 0–231–13459–2, pbk.

JEL 2005–0544

The title *Globalization: What's New?* suggests two possibilities to the potential reader, either of which, given the book's all-star cast, should be a pleasure. It could describe a serious attempt to distinguish current or future globalization from previous manifestations, say, the nineteenth century or even the 1980s. Or it could offer new analysis or new insights into globalization, especially given that its chapters are billed as "original contributions." Original contributions by Michael Weinstein, Douglas Irwin, Charles Calomiris, George Borjas, Robert Scrivner, David Dollar, Jeffrey Frankel, William Easterly, Dani Rodrik, Jeffrey Sachs, and Joseph Stigliz would certainly be worth the price of admission.

It is tempting to say that Michael Weinstein's book falls between these two stools, for its chapters neither consistently identify new phenomena nor consistently offer new analysis, but in fact the explanation for why it does not live up to its billing is more prosaic. The intention, according to the blurb, was to "cut through the confusion and rhetoric to offer straightforward, incisive analyses of globalization." This suggests that the aim was a clear and accessible survey of the field and that the title was either an afterthought or just a bit of hubris. Clear and accessible surveys are very important—as a profession, economists must always try to reach outside their immediate circle to policymakers, businessmen, civil society and the general public.

Unfortunately, however, even in terms of reaching out, the book is only a mixed success, particularly if we take seriously the bits about originality and cutting through the rhetoric. Several of the chapters are closely based on other accessible expositions by their authors, several are highly rhetorical, most show clear evidence of dating from around 2002 despite the 2005 publication date, and the whole thing is casually put together. For example, the introduction lists the chapters in a different order from the book and does not always accurately summarize the chapters it describes, in chapter 10 the footnotes and footnote references do not match, one author acknowledges research assistance from himself (or at least from an incumbent of his job), in chapter 9 the figures and the text differ over whether China is a "proximate" economy, chapter 7 has incomplete references. And, since I am not a careful reader, I infer that these are the tip of an iceberg.

Within the confusion, however, some chapters stand out. Doug Irwin, for example, gives a clear and very concise account of international trade over several centuries. He takes seriously the identification of "new phenomena" and flags the sheer volume of trade currently, the boom in components and in services trade, and the "more muscular" WTO as the important differences from previous globalizations.

Charles Calomiris offers a lengthier and informative essay on international capital flows. He shows that whereas in previous eras private capital flows tended to mitigate economic crises by lending to stricken countries, now they tend to exacerbate the crises. The difference lies partly in the now weaker commitment to fixed exchange rates. It also lies in the way that explicit and implicit subsidies to lenders via the IMF have led to imprudent lending that sows the seeds of crises

and encourages lenders to abandon clients early enough in the crisis that officialdom will take over their debts.

Jeff Frankel provides another of the longer chapters, this time on globalization and the environment. He provides a comprehensive and fairly measured account of the connections between the two from which he concludes that there is no necessary or even presumptive link from globalization to environmental degradation. On the other hand, there are clearly challenges ahead that will almost certainly require greater international cooperation to solve.

Bill Easterly's chapter excoriates the development establishment for its ineffectiveness, focus on bureaucratic well-being, lack of attention to feedback and evaluation, and general heavy-handedness. To emphasize the magnitude of its failure, he cites a "World Bank" result that it costs "$1 billion of aid to lift 284 thousand people a year out of extreme poverty. Thus it takes about $3,500 a year to raise a poor person's income above $365 a year." Easterly does not give the source for these figures. They actually come from Paul Collier and David Dollar (2002), but refer to a $2 a day poverty line ($730 per year), to poverty alleviation *only* via increasing the growth rate not via any targeted policy, and at least implicitly to aid lifting 284 thousand different people permanently above the poverty line each year. Still bad, but not that bad. Thus, like some other chapters in this book, I would class Bill Easterly's chapter as advocacy or rhetoric. There is much truth in his complaints and they are always made in a hugely entertaining way, although it is clearer what Bill is against than what he is for. But ultimately this is not the promised "straightforward incisive analysis."

My reactions to Easterly's chapter mirror those to the whole book. Despite it's shortcomings, it does contain a lot of interesting information and opinion. And even if one has heard it before, it does give one a sense of part of the debate on globalization. Overall, however, given its billing and its cast, I remain disappointed.

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G Financial Economics

Handbook of the Economics of Finance. Volume 1A. *Corporate Finance*. Edited by George M. Constantinides, Milton Harris, and René Stulz. Handbooks in Economics, vol. 21. Amsterdam; London and New York: Elsevier, North Holland, 2003. Pp. xxiv, 604. $135.00. ISBN 0–444–51362–0. *JEL 2004–0144*

Handbook of the Economics of Finance. Volume 1B. *Financial Markets and Asset Pricing*. Edited by George M. Constantinides, Milton Harris, and René Stulz. Handbooks in Economics, vol. 21. Amsterdam; London and New York: Elsevier, North Holland, 2003. Pp. xxiv, 605–1246. $135.00. ISBN 0–444–51363–9. *JEL 2004–0135*

This installment of the Handbook in Economics series published by Elsevier contains twenty comprehensive surveys on the most widely followed areas of financial economics. At nearly 1,250 pages, the two-volume set is an impressive summary of research for readers interested in understanding both the development of thought and the current state of knowledge in modern finance. The editors should be commended for pulling together such a set of authoritative authors for each one of the chapters. The surveys are written for serious students of the field, yet despite finance's reputation for mathematical sophistication, most of the surveys are written to be accessible to readers with limited exposure to or recollection of financial mathematics.

The collection is divided into two volumes, the first containing nine surveys on topics related to corporate finance and the second containing eleven surveys on topics related to asset pricing and financial markets/instruments. The chapters are comprehensive but well structured and each survey begins with a detailed outline enabling a reader interested in a specific issue to easily find that discussion without having to skim the entire chapter. As one might expect in survey articles, the footnoting and citations are extensive providing the additional benefit of a comprehensive and up-to-date bibliography (as of 2003) on each topic, in some cases with references that run as many as twenty-five pages.

The corporate finance volume contains surveys of the fundamental issues relating to the economic functioning of a firm. To lay the groundwork for understanding how firms make decisions, the first chapter, by Marco Becht, Patrick Bolton, and Ailsa Röell, thoroughly details the topic of corporate governance and the theoretical and empirical work on corporate control. This is followed by two chapters on corporate investment, one by Jeremy Stein dealing with the topic of agency problems and uncertainty on the allocation of investment for both external and internal capital markets, and the other by Michael Brennan carefully reviewing methods for evaluating investment projects. Consistent with the common progression in corporate finance, chapters 4 and 5 examine the financing decision. First, Stewart Myers covers the standard theories of capital structure and the choice of instruments. He argues that there need to be multiple theories of capital structure to explain the decisions of different types of firms. Jay Ritter then looks at the equity issuance process and reviews the theories and evidence on market reaction to equity issuances, both initial and seasoned, in the short and long run. He discusses how the market reaction to these offers shed light on broader issues of firms' investment and financing activity. After a short chapter by Peter Tufano that surveys the literature and theories on sources of financial innovation, chapter 7, by Franklin Allen and Roni Michaely, surveys the issue of corporate payout policy. They present the stylized facts about modern payout policies and review the set of theories involving taxes, asymmetric information, incomplete contracting, and transaction costs. At the end, they suggest that the payout decision interacts with the capital structure decision and point to this as an interesting area for further research. The last two chapters in the corporate finance volume deal with financial intermediation. In chapter 8, Gary Gorton and Andrew Winton cover the role of financial intermediaries—mostly banks. They cover the theory for the existence of banks and examine various features of the bank–borrower relation, and then finish with the review of bank runs and bank regulation. The final chapter, by Hans Stoll, is on pure intermediation—the market microstructure of trading financial assets. He examines the determinants of the costs of trading securities—reflected in the bid–ask spread—and the impact on these costs on the behavior of security prices.

The second volume, entitled *Financial Markets and Asset Pricing*, is more mathematical than the first volume as it focuses on asset pricing either for equity, fixed income, or derivative instruments. Chapter 10 by Philip Dybvig and Stephen Ross is a nice starting point as they carefully develop the standard single-period, perfect-market, neoclassical asset pricing model and discuss all of the fundamental portfolio theory concepts associated with this class of models, including the CAPM and the APT models. In chapter 11, Darrell Duffie provides a survey of intertemporal asset pricing theory, focusing heavily on the concept of state prices. He covers both discrete and continuous time version of these models and their application to a variety of term structure, derivative, and corporate security pricing situations. Wayne Ferson provides a highly accessible review of the multifactor asset pricing models from an empiricist's perspective in chapter 12. He compares generalized methods of moments and cross sectional regression approaches to testing these models and concludes with an examination of conditional performance evaluation. The behavior of financial prices in relation to consumption and short-term interest rates is the focus of John Campbell in chapter 13. After presenting the set of stylized facts for market returns, short-term interest rates and consumptions from the United States and other major countries, he discusses the characteristics of market behavior and the models that contain these characteristics in order for consumption based asset pricing models to fit the data. Chapters 14 and 15 switch gears and focus on some of the asset pricing anomalies that have puzzled financial economists over the years. Rajnish Mehra and Edward Prescott review the literature on the equity premium puzzle in chapter 14, and William Schwert summarizes the other common unanticipated empirical regularities found in returns and their implication for the concept of market efficiency in chapter 15. Chapter 16 is the lone chapter relating specifically to international issues in the two volumes. In it, G. Andrew Karolyi and René Stulz review the international finance literature and consider the influence of international factors on asset pricing. They argue that the existing research suggests that a country's market

risk premium depends on its covariance with the world market portfolio and that there is growing evidence that exchange rates affect expected returns. They also summarize the literature on the home bias and question the strength of the empirical evidence for financial contagion among countries. Chapter 17 is a short chapter by David Easly and Maureen O'Hara examining the link between market microstructure and asset pricing, particularly volume and liquidity. Chapter 18 steps out of the rational behavior framework and surveys the literature in behavioral finance. Nicholas Barberis and Richard Thaler summarize research on the impact of deviations from rationality on asset pricing behavior, namely aggregate stock market behavior, cross section of average return, individual trading behavior, and corporate finance. The final two chapters in the handbook deal with derivative financial instruments and fixed income securities. In chapter 19, Robert Whaley provides a detailed coverage of the history, pricing relations, and valuation models for standard derivatives, with the expected emphasis on options. He concludes with an interesting section on the social impacts that arise from derivative trading. Qiang Dai and Kenneth Singleton fill chapter 20 with a detailed examination of fixed income pricing in variety of dynamic term structure and interest rate sensitive derivative pricing models. This chapter is the most mathematical and seemingly well done, but for a fatigued reviewer after 1,200 pages of financial economics, this is a challenging chapter to conclude with.

While I would not recommend the entire handbook as a pleasure read all at once, it is a great resource book and a convenient way to get an introduction to certain areas of financial economics or get brought up to date on what has gone on in these fields since one's graduate school days. It can also be a great resource for teaching. The separate volumes are designed to be used as readers for corporate or asset pricing finance courses at the advanced MBA or doctoral level. Finally, in addition to giving the reader an authoritative understanding of the state of thought in these areas of finance, the volumes are an amazing set of references for important research citations.

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I Health, Education, and Welfare

The Macroeconomics of HIV/AIDS. Edited by Markus Haacker. Washington, D.C.: International Monetary Fund, 2004. Pp. xvii, 344. $28.00, paper. ISBN 1–58906–360–0.
JEL 2005–0613

I teach a graduate class at the University of Cape Town on the Economics of AIDS in Africa. This year I asked my students to review Markus Haacker's new book "The Macroeconomics of HIV/AIDS" (published by the International Monetary Fund, December 2004). This edited collection comprises ten substantial chapters covering various microeconomic impacts of AIDS (e.g., on firms, households, and investment decisions) and the resulting overall macroeconomic impact on human capital accumulation, welfare, and growth. My students were impressed by its breadth, but suspicious of what they saw as its somewhat tunnel-visioned story-line: "AIDS is an economic disaster, period." They characterized Haacker's book more as a self-conscious mobilization of arguments to this effect, rather than as a carefully considered academic exploration.

This struck me initially as a rather uncharitable assessment because the book is readable, well structured, and extensively researched and presents some innovative and integrating approaches to thinking about the economic impact of AIDS. It is also up-to-date and reflects sensibly on various policy challenges, including the provision of anti-retroviral therapy. More importantly for students, it contains several very useful summary chapters—for example the impact of AIDS on global demographic trends (chapter 1 by Brynn Epstein), on developing economies and government finance (chapters 2 and 7 by Markus Haacker), and on the public health sector (chapter 10 by Mead Over). Chapter 5 by Robert Greener is particularly helpful in demonstrating how AIDS can impact different parts of the income distribution differently (and in the case of Botswana, result in increased poverty but an unchanged Gini coefficient). Nancy Birdsall and Amar Hamoudi's exposition of the different "channels" whereby AIDS affects the accumulation and utilization of human capital (chapter 4) provides a similarly helpful analytical frame. This, coupled with the book's accessible approach to modelling the impact of AIDS, should have endeared it to my students.

So why did they find the book so unsatisfying? Perhaps it is because they live in South Africa where one fifth of adults are HIV-positive and over a third of the labor force is unemployed. Under such conditions, it is far from evident that the economic impact of AIDS will be as uniformly bleak as the message contained in Haacker's book. Indeed, two out of three macroeconomic models of South Africa predict that per capita income will rise relative to the "no-AIDS" scenario. In a widely circulated recent paper, Alwyn Young (University of Chicago) argues that AIDS will boost wage and productivity growth—and that a rosier economic future will be the "gift of the dying" to future generations. He concludes that AIDS is a humanitarian disaster, but not an economic disaster. Having been exposed to such arguments, my students were disappointed that Haacker's book did not engage seriously with them.

To be fair to Haacker, his book does touch on some of these alternative modelling exercises—but only to critique them for not taking into account the negative impact of AIDS on institutional efficiency, on productivity, and especially on human capital accumulation. The main message of the book is that AIDS will have a catastrophic impact on development largely via its impact on education and skills development. The chapters by Haacker, Birdsall and Hamoudi, and Bell, Devarajan, and Gersbach make strong arguments that investment in human capital will fall because the returns to education are negatively affected by AIDS-related declines in life expectancy. Bell et al. (chapter 3) take the logic one step further by developing a model in which the early death of parents reduces human capital accumulation to an even greater extent by undermining the transfer of skills across generations—thereby exacerbating the sharp downward spiral in economic growth and per capita income.

My students simply did not buy this story. They pointed out that whilst there is a danger that human capital accumulation will be affected negatively by AIDS, there is as yet no evidence of this happening on anywhere near the scale required to fuel Bell et al.'s apocalyptic vision. For example, a recent review article of empirical studies by Paul Bennell (2005) concludes that AIDS has yet to have any measurable impact on the schooling of orphans and directly affected children in East and Southern Africa. Likewise, South African studies routinely show that families continue to prioritize the education of their children—even in the face of the terrible hardship posed by AIDS.

Some people working in the emotionally charged AIDS arena are uncomfortable with such research results on the grounds that that they could be used to derail the flow of funding for AIDS-related interventions. Kinghorn and Kelly (2005) make precisely this point in their reply to Bennell, arguing that the prepublication circulation of his paper resulted in "at least one international agency . . . (deciding) . . . not to proceed immediately with an education delivery service." They concluded bitterly that: "There may be debate amongst academics, but this is not an academic debate. The life situation and lifetime opportunities of millions of children . . . are being dealt with here" (p. 498).

Kinghorn and Kelly are articulating the dominant (but largely unstated) ethic amongst AIDS researchers that we have a moral duty to highlight the horrors of AIDS in order to encourage governments and donors to allocate resources to AIDS prevention and treatment. My students were convinced that Haacker and his cocontributors were operating within this framework. For them, the book's clear and accessible summaries within each chapter were suggestive of a decision to target policymakers as well as academics. They thus concluded that it had a clear activist agenda, and that this in turn was the reason for its failure to explore seriously alternative scenarios of the macroeconomic impact of AIDS.

Whether this is true or not is, of course, irrelevant. What is more interesting is the question of whether the academic analysis is correct and informs appropriate policies. For example, if the impact of AIDS on education is not as immediate nor as widespread as feared, then there may be a good case for diverting resources elsewhere (e.g., to AIDS clinics or even to general poverty alleviation). Similarly, if the aggregate economic impact of AIDS is not as dire as predicted by Haacker et al. and if it impacts different groups/classes and generations differently, then constructing apocalyptic models of macroeconomic collapse may be neither appropriate nor strategic ways of encouraging government to be more proactive about addressing AIDS. The political elite may instead believe a Young-type model of rising per capita income and calculate

privately that they will be better off funding their health needs through private medical insurance rather than paying the additional taxes required to expand health services to the many unemployed living with AIDS. I have suggested elsewhere that this kind of self-interested class-based calculus may be one of the reasons for the South African government's tardy and still reluctant approach to rolling out antiretroviral treatment through the public health sector (Nattrass 2004). If this is the case, then policymakers are unlikely to be moved by the kinds of arguments articulated by Haacker et al. Academic research would probably be more effective if, instead of trying to panic government into action by articulating contestable stories of economic collapse, it focused on exposing the complex political-economy of AIDS policy—thereby adding intellectual support to efforts by civil society movements (such as the Treatment Campaign in South Africa) to pressure government into changing its position.

But this is a book published by the International Monetary Fund after all. Perhaps the real constraint facing the authors was the requirement that their policy implications be supportive rather than undermining of the state. Nevertheless, the book is diminished significantly by the absence of any political economic analysis. Having witnessed the dramatic and ongoing conflict between the South African government and the Treatment Action Campaign, it is not really surprising that my students were disappointed with what is in other respects a very good book.

Referneces

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J Labor and Demographic Economics

Perspectives on the Economics of Aging. Edited by David A. Wise. NBER Conference Report series. Chicago and London: University of Chicago Press, 2004. Pp. ix, 539. $90.00. ISBN 0–226–90305–2. *JEL 2004–1349*

David Wise has compiled an excellent selection of papers for this ninth edited NBER volume on the economics of aging. The book offers a diverse array of topics ranging from the impact of 401(k) plans on saving to a comparison of housing wealth in the United States and United Kingdom to the effect of income on nutrition in Russia. New in this volume is an expanded view of the economics of aging, one that looks beyond the United States to other countries. Missing is a chapter on retirement behavior, which would have nicely rounded out this survey of current knowledge.

The book opens with a chapter by Jim Poterba, Steven Venti, and David Wise on the impact of 401(k) plans on retirement saving. They show that not only is the average contribution per 401(k) participant about double that of defined benefits (DB) plans, but the time pattern of pension accruals generates incentives that cause DB holders to retire earlier than 401(k) participants. Together these effects imply that defined contribution (DC) plan participants retire with greater retirement wealth than DB participants. Quite interestingly, they argue that crowd-out of DB contributions by 401(k) contributions is relatively small (at most 11 percent), owing to the fact that the rise in 401(k) coverage has been driven by employees without a DB plan being offered 401(k) plans, rather than by DB participants losing DB coverage.

The second chapter, by Jamies Choi, David Laibson, Brigitte Madrian, and Andrew Metrick, examines the effect of firm automatic enrollment policies on employee saving. They find that while automatic enrollment boosts 401(k) participation rates, which in turn increases average wealth accumulation, this effect is roughly offset by more employees clustering in relatively conservative default investment vehicles at low default saving rates. Nevertheless, automatic enrollment does succeed in increasing wealth accumulation in the lower tail of the wealth distribution, simply because automatic enrollment generates plan participation by many employees who would not have otherwise participated.

Next, the book turns to a detailed look at how older households use housing equity by Steven Venti and David Wise. On average, households maintain home ownership as long as they can, resorting to discontinuation or downsizing of ownership only after major shocks such as the death of a spouse or entry into a nursing home. In fact, when older individuals sell their homes, they are more likely to purchase another home rather than discontinue ownership, and at that, a home of greater value. In his comment on the chapter, Jon Skinner suggests older households may be purchasing smaller homes that cost more, perhaps as they move to be near adult children who may live in higher cost suburban areas.

Staying with the theme of housing equity, James Banks, Richard Blundell, and Jim Smith next examine an interesting puzzle: why do younger households in the United Kingdom hold so much more of their wealth in housing equity, rather than in the stock market as is done by U.S. households? They argue that while institutional differences play some role, the dominant explanation is that high volatility in U.K. housing prices leads younger U.K. households to invest in housing as a way to insure against future price uncertainty when they later desire to move into larger homes. In light of the findings by Venti and Wise that older U.S. households tend not to consume housing equity, it would be interesting to see how this pattern differs for U.K. households. Since U.K. households have a lower share of their wealth portfolios in liquid assets like stocks, one might expect much greater reductions in housing equity as elderly U.K. households age.

The book next turns to a series of chapters examining the relationship between socioeconomic status and health. First, Angus Deaton and Christina Paxson extend earlier work on the effect of income growth on mortality decline in the United States and United Kingdom. Although individual level data suggests that income is protective against mortality, at the cohort-level income growth shows no systematic relationship with mortality decline in the United Kingdom, and a relatively weak relationship in the United States. They conclude that mortality decline is likely driven by something other than income, and point to technological change as a possible explanation for future study. Next, Anne Case takes our focus back to the individual level, this time in South Africa. She compares the self-reported health status of black and colored adults living with pensioners with the health of those who do not live with pensioners. She finds that, in households that pool income, income strongly affects the self-reported health of all household members, perhaps operating through the specific channels of improving nutritional status, living conditions, and reducing the stress of day-to-day life. In a novel exploration of the micro-relationship between SES and health, Robert Jensen examines the nutrition channel in a sample of elderly Russians, finding positive associations between income and nutrient intake, and nutrient intake and physical functioning. The book also includes an expanded version of the 2003 *Journal of Econometrics* paper by Peter Adams, Michael Hurd, Daniel McFadden, Angela Merrill, and Tiago Ribeiro on testing for causal links between SES and health.

The book closes with two chapters on mortality. The first, by David Cutler and Ellen Meara, examines mortality decline during the twentieth century. Even though mortality rates declined at a nearly constant rate throughout the century, gains in life expectancy did not accrue uniformly over the age distribution; rather they shifted from younger ages to older ages. A well-known explanation is that, in the early part of the century, gains in life expectancy are thought to have been driven by public health interventions to eradicate infectious diseases, which largely benefited infants and children; by the latter part of the century gains were driven by advances in medical care (in particular treatment of cardiovascular disease), which largely benefited the elderly. Finally, Victor Fuchs, Mark McClellan, and Jonathan Skinner discuss geographic variation in mortality and healthcare utilization in the United States. They note a particularly interesting puzzle in Florida—unusually high healthcare utilization coupled with unusually low mortality rates—which they leave for future work.

When all is said and done the question comes to mind: after *Issues, Topics, Studies, Advances, Inquiries, Frontiers, Themes,* and now *Perspectives on the Economics of Aging*, what could be left? A quick thesaurus search reveals we may yet welcome *Subjects, Viewpoints, Explorations, Examinations,* and perhaps *Progress in the Economics of Aging.* Much to look forward to!

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Putting Children First: How Low-Wage Working Mothers Manage Child Care. By Ajay Chaudry. New York: Russell Sage Foundation, 2004. Pp. xxiv, 341. $39.95. ISBN 0–87154–171–8.
JEL 2004–1339

This is a book about the "other side" of the effort to increase work effort among single mothers: Who is minding the kids? It is an attempt to increase comprehension of the need for child care for young children of various ages and of the difficulties that parents face in seeking child care, and to understand the ways in which the public sector is helping in one large urban area, New York City.

Anyone studying labor force participation would do well to understand that the need for child care in single-parent families with young children is an important and frequently costly constraint (as indeed it is in two-earner families). Subsidies for child care have, moreover, become a far larger and growing component of public policies designed to increase work by low-income single mothers. Understanding the problems faced by those seeking child care may help us design better policies to encourage work and provide care for young children.

As Chaudry notes, the labor force participation of women with children under age six increased rapidly over the three decades beginning in 1960. Among married mothers, the rate increased from 18.6 percent to 62.8 percent, and among single mothers it increased from 40.5 to 73.2 percent. Even among mothers with children under one year of age, more than half were working: as of 2003 53 percent of married women and 56 percent of single mothers were in the labor force. [1] The number of children in child care increased significantly, reaching approximately 10.1 million for children five and under by 1997.

Over roughly the same period, the proportion of welfare funds going to child care also dramatically increased. Four separate sources of funds were combined into the Child Care and Development Fund (CCDF), which gives states a great deal of leeway in determining their allocation and programs.[2] CCDF provided $4.8 billion to states, territories, and tribes in fiscal year 2004.[3] These funds are available to those with young children in need of child care. Within certain federal constraints, states can set their own eligibility policies. [4]

The heart of the book is the struggle to find child care by a group of forty-two lower-income single mothers in New York City. The group is a sample of convenience. Respondents lived in four New York City communities with high levels of child poverty and were recruited through referrals from community-based organizations, through direct contacts that the author/investigator made while volunteering in these neighborhoods, and through additional suggestions made by the original set of respondents. [5] Some of these women had an older child, but for the

[1] CPS 2004; ftp://ftp.bls.gov/pub/news.release/History/famee.04202004.news, accessed on 8/18/2005.

[2] The Child Care and Development Fund authorized under the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 provides child care assistance to families who are receiving TANF aid, are transitioning from public assistance, and other low-income working families. Federal law requires that recipients of subsidies under this program must have income below 85 percent of state median income.

[3] http://www.acf.hhs.gov/programs/ccb/geninfo/index.htm accessed on 8/24/05.

[4] The CCDF provides funds to states, which must then meet federal requirements for program content and state financial contributions. Funds for CCDF are based on three streams. First, states receive a share of the discretionary funds appropriated to the CCDF through the annual Congressional budget process. States also qualify for mandatory funds that represent the funding that states received from previously existing federal child care programs in a base period. Finally, states can receive additional matching funds if they meet maintenance of effort requirements and if they commit additional state funds to draw down the matching funds. States may also transfer up to 30 percent of their TANF block grant to the CCDF program.

[5] The communities are Highwall Valley, heavily African American, Pier Points, primarily Latino, Centerville, also primarily African American, and Mary's Harbor, primarily Latino. Both areas that are largely Latino have a high proportion of immigrants.

majority this was the first child. All mothers at the time of the first interview had a child between the ages of two and three, were New Yorkers, were working at the time, and had less than fourteen years of formal schooling.[6] The author repeatedly interviewed these women about their work and child care experiences as the children went from infants through toddler to preschool age (four).

Through these interviews we are made aware of the myriad problems faced by a single mother with a young child who is attempting to be a full labor force participant. The long list of daunting problems include finding and paying for care, (repeatedly) applying for subsidies, insuring that subsidies are paid and/or paying for care, making it to and from the day care provider and work on time, and, often, dealing with breakdowns in care occasioned by the loss of a provider or their own family circumstances, such as a child's illness. The women in this study struggled hard to find providers who offered adequate or better than adequate care, who would be available for the frequent odd hours of work undertaken by many single mothers, or who would take a preschool or younger child.

The specifics of the women's stories differ substantially. Some of these women had little education and unskilled jobs from which they were readily fired if they were late or missed work because of problems with day care. Others were obtaining more education and went on to semiskilled jobs. Still others had some college education. Some worked within the welfare system, others outside it. All, without exception, had child care problems.

Most made some use of family (kin) and other informal arrangements when the child was very young and to fill in when other care arrangements broke down or there was a need for off-hours care. But these arrangements were themselves unreliable; the relative or friend might be late, have made other plans, be ill or limited in ability to care for a young child, or move away. Small group or family care was another common arrangement. It proved hard for the parent to know very much about the quality of such care, and all too frequently it might be low-quality.[7] The sudden disruption or end of one of these informal arrangements was one of the most difficult challenges facing these mothers. How were they to maintain a job and at the same time find new child care arrangements?

As the children became older, most parents sought center-based care, including Early Head Start and Head Start. These were the most desired arrangements in most cases, because they provided some developmental stimulation and tended to be far better funded. They too presented challenges, however. Center hours tend to be set and there is little leeway for extra hours or odd hours of care. Centers are, moreover, the most expensive form of care and many of these mothers were either ineligible for a subsidy (or eligible for only a small subsidy) or had trouble establishing their eligibility.[8] Nor are there nearly enough slots for all young children whose parents seek to enroll them in center-based care.

When their children were infants, many of these women would have preferred to stay at home to take care of them. Many were pushed into the work force to obtain the income they and their families needed to survive. These mothers tended to have little information regarding child care; it appeared to be hard to find and expensive. Kin care, the most common form, on average lasted less than half a year and was quite unstable. The cost of more formal care (including family care) might be nearly equal to the mother's take-home pay (the EITC might provide the

[6] Fifty women were contacted; of these, forty-nine agreed to meet with the investigator, but seven were not eligible for the study. All forty-two in the final sample remained in the study until their focal child reached the age of four.

[7] These arrangements all too often had little structure and children spent much time watching TV; in some cases, though, providers offered a warm, caring, and stable environment. To secure the latter kind of care, most arrangements were made well in advance of the need for child care.

[8] Localities have their own systems for providing provide child care subsidies. According to Chaudry, New York's is one of the most complicated and created extra difficulties in both establishing and maintaining eligibility. Those mothers who had worked within the system seemed to have connections that enabled them to access services; others had far more difficulty in gaining subsidies even though they were eligible.

extra margin). Even though the majority of these women were eligible for subsidies, few received them for a variety of reasons: the time required to establish (and maintain) eligibility, delays in the start of payments, complications arising from two different sources of subsidies with different eligibility rules, time limits on one source of subsidy, and the lack of available care among qualified providers.[9]

Not all of these mothers were former welfare recipients, but all were, by design, low income. The volume gives a great many statistics on their use of care, but because this is a convenience sample, we really do not know how representative the women are of low-income working mothers in general, and we cannot generalize from them even to the low-income population with children between birth and age four. But their patterns of care give a sense of the difficulties single-parent families face, and they provide a richer set of factors to consider in modeling and testing responses of single-parent families to welfare reform and child care subsidies.

In reading this volume, one may suspect that the problems described may not be specific to the low-income single mother. Clearly, low-income and single parents face many more obstacles, on average, than the more advantaged. Yet a college graduate or a single mother with a graduate degree who has a lucrative job may have similar problems in finding and maintaining high-quality care for a very young child. How do parents, married or single, set about finding and judging high or good quality care? How likely is it that one parent in a couple can postpone reentry into the work force to stay at home with an infant or toddler? Finally, and perhaps most important for the future, what is best for the infant, toddler, and young child, regardless of the family's income? Is consistency of care important? Is being with other infants and toddlers a benefit for the child? Is center-based care good for a child under two and a half years of age?

As a Ph.D. economist, I faced many of these problems: finding good-quality care that was available for flexible (and sometimes long) hours, learning whether that care was safe, warm, and caring. When my children's wonderful care provider broke her leg, I had suddenly to find a new provider; when that provider proved unsatisfactory, it was all to do again. As my children grew older, I had to find a high-quality preschool and make sure to get the children there on time. Through all of this I recognized and had to come to grips with the high cost of care for the parent, set against the very low earnings of the provider.

Times have improved since my children were young. There are tax subsidies for care, there are some licensing requirements for preschool, there is more publicly provided preschool than there was and, in some states, there is universally available preschool. In the United States, the child development fund and the use of the tax code to subsidize child care are consistent with recognition of the value of investment in young children and their parents. But are these separate funds and the current set of policies really the optimal ones from the perspective of society's future? Would the universal provision of preschool for children aged three and four improve opportunities for children and their working parents? Would greater use of certification of providers improve the flow of information and thus the market for child care? Other countries, such as France, have decided to pay extra benefits for young children, viewing this as an investment in the country's future.

This volume provides rather compelling evidence that the market for child care in the United States is far from perfect. One reason for this is lack of information. In part because the market is made up of many small providers, it is difficult for parents to compare quality, cost, and availability of care, and they are often unsure how to evaluate the information they do acquire. Working parents, especially those in low-wage work, need child care that is easy to get to and does not involve long daily trips with small children; in general, they confine their search to relatively small geographic areas and are constrained in terms of the cost of care.

The current child care subsidies, although much larger than ever before, have not gone far enough to create enough slots for children in care that is appropriate for their age, and the transaction cost of acquiring such subsidies is too high, especially in light of the limited availability of qualified child care. Supply has not responded sufficiently to the demand for care. Absenteeism

[9] According to Chaudry, bureaucratic problems and time to qualify were the primary difficulties in obtaining subsidies.

is greater among parents and parents' productivity suffers from the lack of quality care. All of which is to say that the benefits of high-quality care accrue to society generally, as well as to parents and children: lower costs for elementary and later schooling, as children enter school better prepared; and increased productivity and lower need for social services, as working parents (and their employers) face fewer absences and disruptions occasioned by child care problems.

For the economist, several issues worthy of research derive from this volume.

Does the requirement that a mother works when a child is less than a year old pass a benefit–cost test? What are the long-term earnings gains for the mother of the young child? What are the consequences for the child's school performance, future earnings, the probability of delinquent behavior? What is the cost for other children in the shared classroom of the presence of a child raised by multiple providers, some of them incompetent or even uncaring?

How can the public sector increase the supply of adequate child care for very young children? Are there training programs that could be encouraged? Would raising the payment, or perhaps raising it if the provider stayed with an infant for a specified period of time, improve the supply of care for infants, as well as its quality and stability? Would raising the voucher payment for toddlers improve the availability of adequate care for this age group? Can payments for odd hours be increased (as is done for nurses and other hospital workers) to increase the availability of care for young children whose parents work nonstandard hours?

Can the public sector find meaningful ways to measure the quality of care? Is licensing of providers the only way to improve quality and provide information on quality, or is there some lower level of accreditation that might succeed? Are there indicators that parents can use to assure at least some minimum level of care? Are these standards the same regardless of age, sex of the child, or hours spent in child care?

What can the public sector do to improve information in the child care market, so that parents can in fact find care for their children? Can the internet be used to improve information flow and, if so, how can access to the Web be improved for those with such limited resources of money and time as single working women with very young children?

What can the public sector do to administer programs more efficiently? Why, for example, are there delays in payments after an individual has qualified for a voucher? What can be done to reduce delays or at minimum, provide some type of guarantee so that providers do not refuse to provide care?

Many perceive that the 1996 welfare reform legislation was successful in increasing labor force participation. Indications are that it may be less successful for the well-being of the next generation, the children born to these women. If child care policies cannot both maintain work effort among mothers and improve children's well-being, perhaps it is time for economists to spend more time in evaluating child care policies and their implications for the quality and productivity of the next generation. Read the first few chapters of this book to get a real sense of the enormity of the problem. In fact just read the introduction; you will get a good sense of the problem, enough to be informed.

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Unequal Chances: Family Background and Economic Success. Edited by Samuel Bowles, Herbert Gintis, and Melissa Osborne Groves. New York: Russell Sage Foundation; Princeton and Oxford: Princeton University Press, 2005. Pp. vi, 304. $35.00. ISBN 0–691–11930–9.
JEL 2005–0657

The degree of intergenerational mobility in a society is a central determinant of equality of economic opportunity. Consequently, it has been a topic of active empirical research among all fields of social sciences ever since the 1960s, when the first micro data sets became available. This volume, edited by Samuel Bowles, Herbert Gintis, and Melissa Osborne Groves, presents recent and more definitive evidence on this important question. Much of this progress stems from the recent availability of better and more complete data, which have allowed researchers to make better measurements of intergenerational correlations. In addition, the recent data have also allowed researchers to focus on mechanisms of intergenerational transmission (for example, the transmission of personality attributes) that were ignored—because of data limitations—in the first-generation studies. Reflecting the heterogeneous collection of mechanisms that account for

intergenerational mobility, the chapters contained in the volume span a wide range of topics.

Chapter 1 sheds some new light on attributes of parents that are transmitted across generations. The chapter extends the "traditional" analysis of intergenerational mobility to behavioral characteristics, such as psychological wellbeing, drug-taking, arrests, etc. The authors examine the data through the lens of various models of the intergenerational transmission process. The objective is to determine if traditional measures of family background can account for the correlation in behaviors of parents and children (both with measurements at adolescence). The authors find that family background and parenting only explains a small fraction of these intergenerational correlations.

A long-standing challenge to studies of intergenerational mobility is the appropriate measurement of lifetime economic status. Because of data limitations, previous studies have used short-term averages of earnings as a proxy for lifetime economic status. This can lead to poor estimates of the degree of intergenerational transmission since year-to-year fluctuations in earnings mostly reflect the transitory components of earnings as opposed to the permanent component. Chapter 2 presents estimates derived from a new data set that overcome this problem. The data set, which is constructed by linking the Survey of Income and Program Participation to the Social Security Administration's Summary Earnings Records, allows the author to construct long-term earnings histories and mitigate the life-cycle bias that also plagued previous studies. The results suggest that previous estimates greatly understated the persistence of earnings across generations. Using the newly assembled data set, the author finds that the intergenerational elasticity of income (the coefficient on children's earnings in a regression of parent's earnings on children's earning) is closer to 0.6 as opposed to the 0.4 found in multiple previous studies. The results also point to a high degree of persistence at the top and bottom of the earnings distribution. These findings have important implications for the policies aiming at reducing long-term inequality. Moreover, the paper provides a useful framework for future studies of generational transmission.

Chapter 3 addresses the important question of how intergenerational transmission of economic status has changed over the last four decades. The authors investigate how the correlation between parental characteristics (such as education, occupation, income, etc.) and adult's family income has changed over time. To this end, the authors have combined data from the Occupational Change in a Generation Survey, the General Social Survey, and the Panel Study of Income Dynamics, thereby spanning the 1961–99 period. In addition to this focus on long-term changes in the degree of intergenerational transmission, the paper also has two marked points of departure with the previous literature. First, it studies the relationship between parental characteristics and adult family income instead of labor market earnings. The distinction is important since economic status is only partially determined by labor market earnings. In particular, the authors assess the impact of family background on the economic success of all individuals, not only those who are working. Second, the authors summarize the changes in intergenerational transmission by reporting parent–child multiple correlation coefficients (the square of which is the R^2 coefficient in regressions of family income on parental characteristics). While comparing multiple correlation coefficients over time is made difficult by differences in the surveys used, the authors reach several interesting conclusions. Most importantly, they find that intergenerational transmission (as measured by the multiple correlation coefficient) remained stable between the 1970s and the 1990s for men, but actually declined for women.

Chapter 4 offers new insights in the long-standing question of the role nature versus nurture in generating earnings variation. The authors make use of an extraordinarily rich data set on nine different sibling types living in Sweden. The authors contrast earnings correlations across sibling type under different assumptions for the siblings correlation in the genetic and environmental components of their earnings. Since the data set has information on siblings who presumably share a large fraction of genetic and environmental components (e.g., monozygotic twins) and on siblings who only share environmental backgrounds (e.g., adoptive siblings), the authors can relax and evaluate the credibility of the assumptions typically made in the literature. The chapter has several important conclusions. First, the authors find that assumptions pertaining to the siblings correlation of genetic and environmental factors have

direct implication for the nature versus nurture debate. Assuming that identical twins share the exact same environment tends to overstate the share earnings inequality attributable to nurture as opposed to nature. Interestingly, the importance of the genetic component of earnings variation is found to be different for males and females. For males, at least 20 percent of earnings inequality is attributable to genetic components, while the same figure is only 10 percent for females.

Chapter 5 looks at the role of race in earnings mobility in the United States. Using data from the Panel Study of Income Dynamics, the author shows that the degree to which intergenerational mobility varies greatly by race. The results indicate that the intergenerational elasticities are comparable across race (though slightly smaller for blacks). The author then argues that the elasticities do not adequately represent the extent of mobility at the extremes of the income distribution. Comparing the rate of intergenerational mobility of whites and blacks at the bottom and top decile of the income distribution, the author finds that blacks have a much lower rate of upward mobility than whites. He also finds that black/white differences in parental educational attainment cannot explain the differential intergenerational mobility rates across race.

Chapter 6 presents a meta-analysis of correlations between parents and child's personality trait or attitude. The "sample" consists of 1,279 correlations reported in previously published papers. In order to contrast the role of genetic and environmental factors to the transmission of personality traits and attitude, the author compares average correlations across different parent–child pair types. The tabulations reported reveal that there is a stronger degree of intergenerational transmission for attitudes than personality traits. Moreover, the intergenerational transmission of both attitudes and personality traits is mostly attributable to the shared genetic components (rather than the environmental component) of family background.

Chapter 7 continues in the same vein, but instead provides a micro data analysis of the role of personality in the intergenerational transmission of earnings. The author uses data on matched father–son pairs from the mature and young male cohorts of the National Longitudinal Surveys. The statistical models link son's and father's permanent income, while also controlling for human capital variables, IQ, and the Rotter score, a commonly used indicator of personality. The results show that a substantial share of the intergenerational transmission of earnings is attributable to the transmission of personality traits: 11 percent of the father–son correlation in earnings is explained by the transmission of personality.

The last two chapters address the question of intergenerational mobility from a more normative perspective. In chapter 9, the author investigates the question of how much intergenerational transmission of income is morally desirable. The key point raised by the author is that the complete absence of intergenerational transmission (for example zero correlation in parents and child income) may not be a morally desirable. The argument is that the complete separation of parents and children's income may compromise important aspects of family life. This point is illustrated in chapter 8, which focuses on three aspects of intergenerational transmission in China: cultural transmission of preferences toward sons, transmission of marriage form and living arrangements (virilocal versus uxorilocal marriages), and transfers from children to parents. The evidence suggests that both altruistic and self-interest motives from the parents creates a link in economic statuses across generations.

In summary, this book provides a significant benchmark on the state of current research on the transmission of economic status across generations. Importantly, it highlights both what we know and what we do not. Clearly more research needs to be done on this important question. For example, even with the availability of better data, a large share of intergenerational mobility still remains unexplained. Moreover, the recent research on the transmission of health across generations was not incorporated in the present volume. I expect that even more research will be done on these important questions in the coming years, and that Bowles, Gintis, and Osborne Groves may have to assemble a new volume sooner rather than later! Until then, this volume will be extremely useful to researchers interested in social mobility.

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K Law and Economics

The Costs of Crime and Justice. By Mark A. Cohen. London and New York: Routledge,

2005. Pp. xiv, 121. $115.00, cloth. ISBN 0–415–70072–8, cloth; 0–415–70073–6, pbk.
JEL 2005–0667

This slender volume by Mark Cohen addresses an important topic for economics and public policy more generally—the social costs of crime and crime control. Almost forty years ago, Gary Becker (1968) argued in a seminal paper that the public should invest in crime control up to the point where marginal benefits equal marginal costs, an idea that holds obvious attraction for most economists—but apparently not enough to motivate many economists to work on estimating these costs themselves. One result is that many policy debates combine rigorous estimates for how different public policies impact crime with a fair amount of hand-waving about benefits and costs. For example, in a seminal paper Steve Levitt (1996) provides what are arguably the first convincing estimates for the effects of imprisonment on crime. Yet drawing inferences from these results about whether the incarceration rate in the United States is too high or too low—perhaps the most important criminal justice debate of our time—is complicated by how little we know about the social costs per crime. We know even less about the social costs of imprisonment beyond what the government spends per prison bed.

Cohen seems to have written this book to summarize for a broad audience of economists, criminologists, and policymakers what is known about the costs of crime, most of which comes from Cohen's own research in this area over the past twenty years. The book provides useful one-stop shopping for readers interested in learning more about this literature. However Cohen's effort to be comprehensive, together with some specific choices about the book's structure and presentation, may leave some readers confused about some of the key conceptual and empirical issues.

The main reason that economists and policymakers should care about estimating the costs of crime is to conduct benefit–cost analyses of crime-control interventions, as Philip J. Cook and I argued several years ago in our own book about the costs of gun violence (Cook and Ludwig 2000). For this purpose, the most relevant definition of the costs of crime is the public's willingness to pay for a reduction in crime in the future. Unfortunately, Cohen's conceptual discussion in chapter 2 mixes together items that would be relevant for an ex post definition of the costs of crime (like lost earnings or medical expenses suffered by an identifiable crime victim, relevant for jury decisions about after-the-fact compensation) with the ex ante definition that is relevant for benefit–cost analysis.

The organization of the empirical chapters of the book is likely to contribute to this confusion. Chapter 3 is on Victim Costs, with the first part of the chapter devoted to a summary of Cohen's previous estimates for "tangible costs" such as lost productivity, medical and mental health care costs, emergency responder costs, victim services, and property loss. The second part of the chapter focuses on "intangible costs," including a summary of results from the literature on the value per statistical life and Cohen's own work drawing from jury awards to value pain and suffering costs associated with nonfatal crime injuries. Chapter 4 is entitled "Third-party and Society Costs" and reviews estimates for tangible costs to third parties, avoidance behavior, the costs of crime control, and offender costs. But many of the distinctions drawn in these chapters, such as those between tangible and intangible costs, or between costs to victims versus third parties or society as a whole, will be a distraction for readers trying to develop benefit–cost estimates from an ex ante perspective.

The empirical part of the book also suffers from what journalists call "burying the lead," in the sense that the most interesting development in the cost-of-crime literature—the use of contingent valuation (CV) methods—is left to the very end of chapter 4. The CV approach, widely used in environmental economics and recently employed to estimate the costs of crime by Cook and Ludwig (2000) and then Cohen et al. (2004), asks survey respondents to value reductions in crime within the context of questions about hypothetical interventions. In principle, CV responses may capture the willingness-to-pay value that is relevant for benefit–cost analysis, an important advantage over other measurement techniques. Yet whether CV is capable of generating meaningful estimates for these values in practice is controversial. What is the current scientific consensus within the environmental economics literature about the utility of CV, and the conditions under which this method can yield useful results? What special issues arise in applying CV to understanding the costs of crime and justice?

How much faith can policymakers place on existing CV estimates for the costs of crime, and what should the priorities be for future research along these lines? These and other key questions do not receive much discussion in the 1.5 pages devoted in Cohen's book to the new CV literature on the costs of crime.

Economists, criminologists, and policymakers alike should be grateful to Mark Cohen for having done so much over the years to carry research on the costs of crime forward. This book provides a nice overview of Cohen's "greatest hits" even if we might wish the book had put somewhat greater emphasis on the newer conceptual and empirical developments on this topic.

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The Great Divestiture: Evaluating the Welfare Impact of the British Privatizations, 1979–1997. By Massimo Florio. Cambridge and London: MIT Press, 2004. Pp. xvi, 424. $45.00. ISBN 0–262–06240–2. *JEL 2005–0243*

In this well written and highly readable book, Massimo Florio presents a comprehensive social welfare analysis of the British privatization program under the Conservative governments of Margaret Thatcher and John Major, and reaches a generally underwhelming conclusion regarding the program's utility and benevolence. He examines the impact of this program on specific segments of society—firms, shareholders, employees, consumers, and taxpayers—and finds that the Great Divestiture benefited shareholders and certain employees (especially managers), had surprisingly small effects on firms and most employees, and generally harmed taxpayers and most consumers. Florio also presents an overview of the evolution of U.K. political and economic thought regarding the role of public enterprise in British society, which helps place the privatization program in a solid historical context, and he discusses why and how individual enterprises were originally nationalized. Additionally, he surveys the actual operating and financial performance of the nationalized industries prior to divestiture, over both a short and long term horizon, and concludes that their overall performance was substantially better than generally believed. Florio also assesses the macroeconomic impact of the U.K. privatization program, presents a case study of British Telecom, and concludes with a general assessment of the program's impact—which includes his negative take on the optimality of "a state without ownership." Florio concludes that the best overall estimate of the net welfare gains from Britain's enormous, two-decade-long privatization program is a mere £57 billion, or about £50 per citizen per year, and that accounting for the program's negative impact on the distribution of income and wealth might well negate these net benefits entirely.

There is much to admire in this important book. Florio presents the first comprehensive analysis of the single most important national privatization program to date (though, in time, China's program will certainly become history's largest and most important). He presents a defensible framework for assessing the overall impact of a national divestiture program that accounts for potentially negative effects on consumers and taxpayers, and he stresses the importance of designing the proper regulatory regime for privatized utilities—especially those which remain effective monopolies. Florio repeatedly points to the absence of any productivity shock resulting strictly from ownership change as evidence that the U.K. privatization program failed to revolutionize the British economy. Though the program may have helped arrest Britain's long economic decline relative to other developed countries, it did not reverse this trend. Additionally, several of Florio's chapters are excellent and highly informative for a reader inexpert in public finance and welfare economic analysis. Finally, Florio presents a treasure trove of data in the book's tables, figures, and appendices.

Weighing against these positive features of Florio's book are several weaknesses. Some of these are fairly minor. For example, I was very surprised that his historical discussion devoted so little space to describing the initial Labour nationalization program of 1946–47, and he mentioned the Beveridge Report only once, in passing. I also take issue with his claim that most Conservatives had accepted, even supported, Labour's nationalization program and the efficacy of state enterprise during the years preceding 1979; my take on postwar British history and the events leading to the Thatcher Revolution would put greater stress on endemic class conflict and ideological polarization.

Other weaknesses in Florio's approach are much more serious. The most important weakness is a selective coverage of the theoretical and (especially) empirical literature on privatization and the relative merits of state versus private ownership. Florio's coverage of British studies is indeed comprehensive, but when he ventures into making international comparisons he somehow finds a way to cite nearly the population of studies that criticize privatization, yet cites barely a fraction of the far larger number of studies showing that privatization improves firm performance, enhances macroeconomic stability, and either improves consumer access to vital services or reduces costs (or improves quality), or both. Studies critical of privatization are taken at face value, while the handful of supporting studies that are cited are typically subjected to a "yes, but" dissection. It is also astounding to see a professional economist assert today that we cannot definitively conclude that private ownership is superior to public ownership in almost all circumstances; Florio is only able to defend this thesis by ignoring a string of theoretical and empirical articles, published over the past decade in the very top journals, supporting the greater efficiency of private ownership. More generally, Florio seems unaware of the enormous recent corporate governance literature (especially the Law and Finance literature) that stresses the importance of ownership and legal protections for investors.

A second major weakness of Florio's book is his negative analysis of the financial impact of privatization. In my opinion, he gets several things wrong. First, he frequently criticizes the underpricing of privatization IPOs, and seems shocked at the £17 billion total he calculates this cost the Treasury. Yet, deliberate underpricing of privatization offerings—coupled with share allocation policies favoring small, domestic investors (especially employees)—has been a pervasive feature of national programs around the world (China's average first day return exceeds 200 percent), and is motivated by sound political and economic reasoning. This logic is spelled out in several A-list journal articles, yet Florio cites only one of these, in passing. He also calculates the "cost" to the state of selling off state enterprises in terms of a decline in public sector net worth, yet it is unclear how (if at all) he accounts for the market value and investments of these privatized firms. The fourteen largest independent privatized companies have a market value of over £257 billion today, and privatization has had an enormous impact on the growth of capital markets (especially stock markets) in almost all countries where major share issue privatization programs have been launched—including Britain. In fact, it can be argued that privatization particularly helped Britain because it promoted the financial sector, which is an unusually large and productive fraction of the U.K. economy, and helped solidify London's preeminence in global finance.

Florio also displays a real antipathy toward the institutionalization of stock ownership that has occurred in Britain coincident with the Great Divestiture. In particular, he compares pension fund ownership and governance unflatteringly to entrepreneurial capitalism and concentrated ownership, and believes pension funds are more concerned with "satisficing" than with demanding maximum corporate performance. This viewpoint is hard to fathom; institutional investors have come to dominate stock market ownership and (especially) trading activity the world over, and most financial economists consider these large, sophisticated, demanding investors to be superior monitors of managerial performance. Florio also seems unaware that recent academic research has firmly associated concentrated share ownership with a poorly functioning national corporate legal system, whereas the corporate governance system that has evolved in Britain over the past two decades is now generally considered to be better than that of any other large country. The countries of continental Europe that are struggling to move from a government run, pay-as-you-go pension

system to a funded system of privately managed pensions can only gaze wistfully at Britain's large pension funds and tradition of stock market investing.

A final critique can be levied at Florio's conclusion that most, if not all, of the improvements in the performance of privatized companies that has occurred since 1979 have resulted from technological changes and/or enlightened regulation, rather than from ownership changes per se. He does not satisfactorily address whether these changes could have been achieved without ownership change, or at least without the spur of ultimate privatization. He persuasively shows that at least some of the value-maximizing investment programs would have been adopted under continued state ownership, but it seems to me unlikely that Britain's traditionally fractious labor unions could have been tamed, or that the power of entrenched producers and service providers could have been broken without privatization. Few economists would today dispute Florio's assertion that regulation and promotion of competition are vital to achieving an efficient and distributionally fair economy, and he makes a valuable contribution by showing just how slowly the British regulatory system evolved towards this standard. Nonetheless, recent research also clearly shows that private ownership has a separate and significant positive effect on the efficiency of network industries.

On balance, how convincing is Florio's book? Before opening his text, I had already read several hundred privatization and ownership studies, and had authored a dozen or so myself, so I began reading with well defined opinions about privatization's effectiveness. Florio's book did not fundamentally alter those opinions. On the other hand, someone already predisposed to view privatization critically will find his or her views fully supported by this text. The average professional economist—with only a general interest in privatization—who reads this book will probably conclude that Britain's privatization program was, on balance, a worthwhile undertaking, but one that could and should have been implemented with greater concern for consumers and for society's most vulnerable. This is a valuable lesson for everyone involved in implementing or assessing privatization policy to learn.

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Strong Medicine: Creating Incentives for Pharmaceutical Research on Neglected Diseases. By Michael Kremer and Rachel Glennerster. Princeton and Oxford: Princeton University Press, 2004. Pp. xiv, 153. $24.95. ISBN 0–691–12113–3. *JEL 2005–0248*

Strong Medicine by Michael Kremer and Rachael Glennerster makes the case for "pull" programs to stimulate research on diseases that are important in many poor countries. The subtitle of the book is "Creating Incentives for Pharmaceutical Research on Neglected Diseases."

The book begins by identifying diseases endemic in low- and middle-income countries. These include diarrheal diseases, malaria, tuberculosis, measles, HIV/AIDS, pertussis, tetanus, syphilis, schistosomiasis, and Chagas disease. Three of these diseases—malaria, tuberculosis, and HIV/AIDS—are singled out for further discussion.

The central question for these diseases is why are the large multinational pharmaceutical firms with large R&D budgets not investing in developing treatment regimes for these diseases? The book notes that of 1,233 drugs licensed worldwide between 1975 and 1997, only thirteen were for tropical diseases. Of these, five came from veterinary research, two were modifications of existing drugs, and two were produced for the U.S. military. The slow response of the large multinational pharmaceutical companies to the HIV/AIDS crisis in Sub-Saharan Africa is now well known and documented.[1] The industry should normally have been expected to provide antiretroviral cocktails in low-income countries at low prices. But prices were not lowered until pharmaceutical firms in India and Brazil begain to offer lower priced drugs.

Chapter 4 of *Strong Medicine* discusses market and government failures and chapter 5 discusses the role of push programs. The world pharmaceutical market is dominated by rich countries with the U.S. market accounting for 39 percent of world sales. It is not difficult to see why the large pharmaceutical firms focus on the rich country market. And it is not difficult to see why drugs are

[1] The large multinational pharmaceutical firms include Merck, Hoffman-LaRoche, Pfizer, Eli Lilly, Bristol-Myers-Squibb, and American Home Products.

patented and subject to prescription by doctors. It is also not difficult to understand why the pharmaceutical firms invest so heavily in advertising (even though many advertised drugs are prescribed by doctors). Apparently, the Food and Drug Administration does not pressure firms to offer more "over the counter" drugs.

Chapter 5 on push programs notes that USAID did initiate an effort to develop a malaria vaccine in the 1980s. That effort was unsuccessful. The chapter also discusses R&D tax credits for work on HIV, tuberculosis, and malaria and concludes that it is difficult to achieve gains with traditional push programs.

The "pull" programs advocated in *Strong Medicine* require the following conditions:

1. Scientific potential
2. Multiple R&D program response
3. Minimum Product Safety, Efficacy and Usability
4. An Independent Adjudication Committee
5. A Market-Test Requirement with Copayments
6. Exit Clauses

The authors propose that a price of $15 per person for the first 200 million people immunized with an effective malaria (or tuberculosis) vaccine would be an effective pull program. But would it?

Consider the first requirement. Is there adequate scientific potential for a malaria vaccine? The authors argue that scientists think so. Consider the second requirement. If only one of the major multinational pharmaceutical companies developed a program, the chances of achieving the objective would be lower than if many companies initiated programs. The authors note that many small start-up firms have entered the drug development field in recent years as biotechnology firms.[2] Indeed, given the abysmal record of the large firms on HIV/AIDS pricing in Africa, it would appear likely that smaller biotechnology firms hold the best hope for achieving an effective malaria vaccine.

The role of the Independent Adjudication Committee and the copayment requirement are essential if corruption is to be avoided. *Strong Medicine* also discusses patent buyouts (a topic on which Michael Kremer has written extensively) but in practice, governments not only do not engage in patent buyouts, they also do not implement compulsory licensing provisions in existing patent laws.

Strong Medicine discusses the cost effectiveness of pull programs in terms of Disability Adjusted Life Years (DALY) saved. Health interventions in the United States are considered cost effective at $50,000 to $100,000 per DALY saved. The World Bank regards $100 per DALY saved to be cost effective in the poorest countries. The cluster of vaccines against polio, diphtheria, tetanus, and pertussis costs $20 per DALY saved in low-income developing countries and $40 per DALY saved in the middle-income developing countries. *Strong Medicine* suggests that a malaria vaccine could achieve costs of $21 to $26 per DALY saved.

Strong Medicine thus constructs a good case for pull programs, and perhaps with the entry of many new biotechnology firms in the pharmaceutical industry the preconditions for success are good. The record of the major pharmaceutical firms in R&D strategy and new drug development is not promising. The new biotechnology entrants do offer some hope, although they too are seeking to develop drugs for rich countries.

The message of *Strong Medicine* is important. The poorest people in the world do not have adequate R&D investment for illnesses endemic in tropical countries. *Strong Medicine* makes a contribution by drawing attention to the issue. Existing multinational pharmaceutical firms have been the problem, not the solution. But we are seeing more third-world pharmaceutical firms as well as many new biotechnology firms. The pull programs advocated in *Strong Medicine* might actually succeed.

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N Economic History

The History of Foreign Investment in the United States, 1914–1945. By Mira Wilkins. Harvard Studies in Business History, vol. 43. Cambridge and London: Harvard University Press, 2004. Pp. xxvi, 980. $95.00. ISBN 0–674–01308–5.
JEL 2004–1421

[2] These new entrants include Genentech, Chiron, Amagen, Biogen, Genzyme, Immunex, Genetics Institute, ImmunoGen, and Millenium Pharmaceuticals.

In the sequel to her 1989 *History of Foreign Investment in the United States up to 1914*, Mira Wilkins (Professor of Economics at Florida International University) examines the causes and consequences of foreign investment in the United States between the First and Second World Wars. Wilkins relies on exhaustive archival research and a narrative approach to tell this story. At times she provides so much detail (980 pages and 3,065 footnotes!) that it is difficult to separate the trees from the forest. Nevertheless, her breadth of knowledge and ability to tie various concordant developments together provides the reader with a panoramic view of inward foreign investment during this tumultuous period.

Wilkins makes three important claims. The first, which will go unchallenged by most economic historians, is that foreign investment was fundamentally influenced by the larger economic, financial, technological, social, and political changes taking place between the two wars. Indeed, the book's primary strength is to explain how these forces, particularly security concerns and nationalism, influenced foreign investment.

Prior to the First World War, foreign investment was driven largely by technological advantage with political factors playing a minor role. Several American industries were dominated by foreign multinationals. The "high-tech" rayon industry was led by the British-owned American Viscose Company, which transferred technical skills and experience across the Atlantic, so that rayon became "among the most spectacular growth industries of the 1920s" (p. 237). British firms also played major roles in radio (Marconi Wireless Telegraph), processed foods (Morrell and Lipton), soap (Lever Bothers), natural textiles (J. & P. Coats), and oil (Shell). With superior technology, German firms (e.g., Bayer and I.G. Farben) dominated the pharmaceutical and chemical industries. In contrast, American firms "towered over their European counterparts" in autos, steel, and electrical equipment, because they "were too technologically superior for the Europeans to make any headway" (p. 262–63).

The First World War dramatically altered the pattern of foreign investment as political forces took center stage. The Alien Property Custodian was created in 1917 to confiscate enemy assets and distribute them to Americans. For example, the Bayer affiliate, including the Bayer name and logo, were expropriated and sold to an American company. Fearing that foreign control of the radio industry would compromise national security, the U.S. government purchased Marconi's American affiliate and created the Radio Corporation of American (RCA) in 1919. The government also outlawed transportation of merchandise along U.S. coasts by foreign-owned ships, prevented foreign companies from using public lands if similar privileges were not granted to U.S. companies operating abroad—giving the State Department leverage to open doors for American businesses—and reduced American dependence on foreign trade credit by expanding the powers of American banks to provide dollar acceptances.

Despite these political impediments and macroeconomic forces that were transforming the United States into a net creditor, the tentacles of foreign multinationals grew longer during the 1920s as they sought to benefit from the size and affluence of the American market, vault tariff walls erected after the war, and use technology licensing agreements as negotiating tools to form cartels and divide world markets with potential competitors.

As the Great Depression deepened and geopolitical tensions grew, the backlash against foreign firms strengthened. Francis Garvan, the Alien Property Custodian during World War One, warned that I. G. Farben would stymie American economic progress by "hampering and obstructing scientific research" and use "everything, from theft of ideas to the use of every known device for the throttling of competition, price-cutting, bribery, tying-contracts, rebates, subsides . . . " (p. 296). Others denounced multinationals "for their part in the growth of Hitler's power" (p. 458) and argued that they would drag the United States into war just like the international bankers did twenty years earlier.

Wilkins rejects both charges. Citing research from the 1980s, she argues that German superiority in synthetic rubber technology stimulated U.S. research and development in the 1930s and that these efforts "served as a foundation for the U.S. success in wartime . . . Indeed, it seems likely that the interchange of patents and the impact of foreign multinational enterprise proved positive rather than negative" (p. 542). Rather than dragging countries into war, Wilkins contends that company records reveal "a pride in enterprise accomplishments" and a deep

belief that "Technology has to carry on—war or no war . . . " (p. 470).

This sanguine view was not widely held during the late 1930s. Thus antitrust policy became more aggressive under Roosevelt and was used to redistribute intellectual property. If American firms had acquired patents through illegal cartel agreements, the government, acting in the public interest, was entitled to break these agreements and distribute the technology to Americans. For example, Standard Oil of New Jersey was forced to license royalty-free to other American firms all patents in rubber and other critical products that it had obtained from I. G. Farben. By 1946, the United States government had vested 47,000 patents. Wilkins argues that the diffusion of technological knowledge embedded in these patents "represented some of the most consequential policy measures linked with foreign assets in the United States; they set the broader precedent for compulsory licensing of patents when in the public interest" (p. 529).

Wilkins's second major claim is that the available statistics grossly underestimate the impact of foreign investment on American economic development. Indeed, the first "official" balance-of-payments was not made until 1922 and mandatory reporting requirements did not exist throughout the 1920s. Moreover, many investments recorded as portfolio were actually direct in nature. For example, despite the fact that British-owned Nobel Industries only held a four percent stake in General Motors in 1920, this investment "was not arm's length; it was closely associated with Nobel Industries' American strategies; the investor did get board representation; the large size of the interest could mean possible influence (if not control)" (p. 154).

During the 1930s, underestimation of foreign involvement in the American economy resulted more from deliberate obfuscation than imprecise accounting. To avoid capital controls imposed by the Nazis (punishable by death beginning in 1936), "much of the German 'investment' had grown through technological rather than financial exports" (p. 380) by the late 1930s, making it less visible to government statisticians. Also, fear of expropriation by the American government caused German firms to use "cloaking devices" to hide their interests. For instance, in 1939, I. G. Farben transferred ownership of its American joint ventures with Standard Oil of New Jersey to SONJ and, in return, received all rights to their joint ventures outside of the United States. However, a document discovered after the war, which authorized I. G. Farben to cancel the agreement when it was safe to do so, revealed that the deal was a sham. Although I. G. Farben "technically" had zero American direct investments by 1941, Wilkins concludes that its "U.S. interests were many; and as a multinational enterprise it continued to be an important influence in America's chemical industry" (p. 468).

Overall, Wilkins makes a convincing case (through the sheer number of anecdotes that she offers as evidence) that foreign investment in the United States was much more influential than the statistics suggest.

Wilkins's final claim is that the interconnections between foreign direct and portfolio investment (FDI and FPI) are so important that modern scholars who study them independently miss an essential part of the story. Of particular significance is what Wilkins refers to as *cosmopolitan finance*, i.e., foreign companies establishing American subsidiaries for the purpose of dispatching capital abroad. There are numerous examples from the 1920s. Swedish financier Ivar Krueger created the International Match Company in New York (inward FDI) largely to tap American financial markets (outward FPI) for the purpose of erecting match monopolies in Europe. Foreign banks created the International Acceptance Bank in New York (inward FDI), which financed European trade (outward FPI). Wilkins argues that cosmopolitan finance was largely new to the 1920s, vast in scale and driven by American capital abundance, its unregulated financial markets, and lax tax collection on nonresidents.

The discussion of cosmopolitan finance helps us understand the emergence of New York City as a major financial center in the 1920s. Unfortunately, it is the least developed of Wilkins's arguments and raises more questions than it answers. For instance, what role did the broader institutional and financial development taking place during the interwar period (i.e., the creation and maturation of the Federal Reserve, emergence of the dollar acceptance, deepening of the American bond and equity markets, etc.) play in this process? Wilkins tells us a great deal about the complexity of these "conduits" for capital flows, but does not explain the fundamental informational problems these arrangements

address or the imprimatur role played by the foreign affiliates.

This criticism aside, this is an excellent book about a fascinating period. Without reflecting on this history it is easy to surmise that the United States has always been a technological leader, creating positive spillovers for the rest of the world. Wilkins reminds us that the United States benefited greatly from European technology transfer during the first half of the twentieth century and that aggregate statistics underestimate this impact because, often, the devil is in the details.

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O Economic Development, Technological Change, and Growth

Scarcity, Conflicts, and Cooperation: Essays in the Political and Institutional Economics of Development. By Pranab Bardhan. Cambridge and London: MIT Press, 2005. Pp. xi, 306. $25.00, paper. ISBN 0–262–02573–6, cloth; 0–262–52429–5, pbk. *JEL 2005–1070*

The title of this volume, as well as the introductory materials in chapter 1, promise to deliver a focus on the place of institutions in development economies. Even more enticing is the commitment to take the subject beyond the customary near exclusive focus on property rights. However, even if we accept Bardhan's broad definition of institutions as "rules of structured social interaction," this is not exactly what is presented here. Instead, we are offered collected essays from the pen of Professor Bardhan, but not all necessarily dealing centrally with the subject at hand. But no matter; the various chapters represent excellent surveys of the literature on various important subjects, more or less closely related, by a master craftsman. Twin promises are kept, for few models and little originality, but much challenging material is presented in an easily digestible and extremely lucid fashion.

Chapter 1 focuses on the encompassing institutional dimensions underlying property rights, from decentralization and accountability to the trade-offs between transactions and production costs along the North–Greif axis. Institutions ideally can serve to weaken this trade-off, especially as an economic system becomes more complex. Financial markets provide a good example of this, with new institutional intermediaries softening the conflict between savers' trust and investors' competence. Property rights have an admittedly different meaning for the rural poor and the urban rich but when the state is inept or corrupt it should be remembered that NGOs and decentralized governments can also be captured by local elites.

Bardhan endorses the need to search for clever instruments to overcome the institutional endogeneity problem, the difficulty illustrated by the deployment of the mortality rate of European colonial settlers. He also expresses a strong preference for comparative historical case studies over cross-country regressions but concedes too much, in my view, by concluding that "for a long time to come both methods will have to be utilized, with their limitations noted."

Chapter 2 asks the highly pertinent question why dysfunctional institutions survive and responds with the aid of an extensive and very useful literature review. Much emphasis is placed on the impact of initial inequality, especially when potential gainers from a particular policy change, such as liberalization, are diffuse, e.g., exporters, while losers, e.g., import competitors, are concentrated.

Chapters 3 to 5, dealing with power, the commitment problem, and democracy, while interesting in their own right, fueled my suspicion that the volume comes closer to a collection of essays than a coherent treatise on the role of institutions in development. Yet, once again, subtle literature reviews await the reader. In particular, chapter 5 provides a healthy antidote to prevailing simplistic views on the positive relationship between democracy and poverty reduction, while ignoring the important differences between procedural and participatory forms of democracy.

Chapter 6, on the other hand, returns to the theme of the decentralization of governance, concluding that local government's information advantage is usually trumped by its exposure to capture by vested interests. While agreeing that real devolution is exceedingly rare and even delegation is often overly romanticized, I believe neither the theory review or the case studies presented here are conclusive. The "gold-fish bowl" effect at the local level versus the "big black box" at the center are given inadequate attention.

Chapter 7 deals with the closely related topic of relative capture and governance at central and local levels. Inevitably, in a collection of essays, repetitions and overlaps become unavoidable. For example, Al Stepan's useful distinction between "coming-together" and "holding-together" federalisms is cited here for the second time.

Chapter 8 deals with corruption and explores both its positive and negative efficiency-related responses to exogenous control systems. The relatively greater insulation of East Asian bureaucracies from rent-seeking private interest groups is acknowledged. But I would place at least some of the blame for the increase in corruption in post-socialist systems at the door of a conversion from nonmonetary to monetary forms of corruption.

The last few chapters tackle subjects several steps removed from the main theme of the volume. Chapter 9 addresses internal ethnic conflicts generated by some combination of greed or grievance and exacerbated by IMF/World Bank pressures for macroeconomic austerity. It is not clear, in nations thus under siege from below as well as above, what the mediating institutions creating the necessary trust among groups really look like. Bardhan refers to the avoidance of the politization of the police and of "first past the post" electoral rules, but we also know that the temporary accommodation to minorities often becomes permanent. One could have wished for some discussion of the currently enhanced willingness of outsiders to get involved in internal ethnic conflicts.

Chapter 10, on collective action, asks when cooperation is sustainable and when it becomes a victim of "prisoner's dilemma." In addition to reviewing much of the literature, Bardhan makes an exception here by presenting his own model which emphasizes the impact of inequality, side payments, and punishments on the chances for cooperation to be stable. Chapter 11 presents an empirical demonstration of cooperation in the realm of the sharing of irrigation waters.

Finally, the only internationally oriented chapter, chapter 12, focuses on an ambitious agenda: Global Rules, Markets and the Poor. While we might accept Bardhan's concessions on the impact of cultural protectionism and short-term capital inflows on the poor, his assertion that growth is necessarily associated with increased inequality is by no means proven.

In summary, this volume delivers thorough, balanced, analytically well reasoned, and highly accessible reviews of the literature on a number of subjects, if some only loosely connected to its advertised central theme. Not only economists but also other social scientists interested in development will profit from it. The volume is strongly recommended both for the high quality of its scholarship and the high quality of its exposition.

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World Development Report 2005: A Better Investment Climate for Everyone. World Bank. Washington, D.C.: World Bank; Oxford and New York: Oxford University Press, 2004. Pp. xvi, 271. $26.00, paper. ISBN 0–8213–5724–7, cloth; 0–8213–5682–8, pbk. *JEL 2005–0350*

For over twenty-five years, the World Bank has produced its annual *World Development Report* (*WDR*) to provide its thoughts and recommendations on economic development, clearly a document of importance to people interested in the fate of poor countries. The content of the report has changed over the years. At first, it put most emphasis on taking stock of recent trends and prospects for the world economy. Then it added to the discussion of the world economy a discussion of an important topic of perennial concern that differed from year to year such as the role of agriculture or education or income distribution in the development process. Finally, the reports have dropped the discussion of the world economy to place sole focus on one special theme.

Each *WDR* is written by a specially constituted team and the drafts are vetted by many readers prior to being finalized. Ideally these reports would meld the best understanding from peer-reviewed research by universities, the Bank itself, other international organizations, and nonprofit think tanks. Teams also can make use of the special status and resources of the Bank to generate or assemble data that would otherwise not be available. The ultimate motive for the reports' messages is the Bank's goal of recommending beneficial policies to its member countries, policies that at the end of the day must involve specific actions.

The theme of the 2005 *WDR* is what the Bank terms the investment climate, defined in this *WDR* as the opportunities and incentives for firms to invest productively, create jobs, and

expand. The report aims to present a program for governments to promote a better investment climate evaluated from a broad social perspective rather than the firm owners' narrow interests. This concern is consistent with the principles of applied welfare economics and its attention to questions of economic efficiency and equity, and these principles certainly help to structure this *WDR*. The Bank's agenda for governments is to correct both errors of omission (market failures that governments have neglected) and errors of commission (bad policies that governments have pursued). As a broad framework, the approach is certainly sensible and it can support discussion of a wide range of topics.

Early on, the report discusses the value of establishing clear property rights and enforcing contracts and how to achieve these goals. Credit bureaus can help firms to know the reputations of their contractual counterparts. Registries for movable property can let firms know who owns what and if ownership rights are encumbered, and are an important step to allow the use of assets as collateral. Procedures for repossession and the efficient enforcement of contracts and resolution of disputes through arbitration or the courts can facilitate lending and other types of business transactions. The *WDR* identifies some specific arrangements and provides some information on what has been tried in different parts of the world. Nonetheless, there is limited detail and no answers to many practical questions that reformers face, such as: Should losers pay winners' court costs? Should commercial disputes be settled before juries or judges? For answers to this sort of question, it would have been useful to draw on the extensive literature on law and economics (e.g., Miceli 1997).

This *WDR* also discusses regulation, primarily as it deals with competition. There is discussion of barriers to entry, barriers to exit with bankruptcy laws the primary example, and the behavior of incumbent firms. Again, however, there is not much detail, for instance on the principles and modalities of bankruptcy laws or laws and agencies to control anticompetitive behavior. The *WDR* (p. 105) sees the removal of anticompetitive policies as "perhaps" more important than the implementation of procompetitive ones. This conclusion would seem difficult to justify and potentially dangerous, and the *WDR* team could have benefitted greatly from attention to the work of Laffont (2005) based on his 2001 public lectures, circulating in manuscript while the *WDR* was under way.

The discussion of taxation focuses on administration ranging from complexity in the tax code to corruption in the tax administration. In contrast to the academic literature, it gives essentially no attention to the issue of deadweight loss in the design of taxation. No doubt there are obstacles to implementing a full-blown optimal tax solution to minimize deadweight loss, for instance, its potentially highly differentiated rates. Nonetheless, attention to deadweight loss is ineluctable and there are many quite general results, such as the desirability of taxing final consumption over say trade that provide real guidance, but not in this *WDR*. For example, when a broad-based sales tax is converted into a trade tax by poor administration of the tax on sales of domestic production, it is the effect on deadweight loss that is material rather than the deviation from statutory intent in and of itself.

The *WDR* next turns to finance and infrastructure, with the emphasis on private provision where possible but with regulation to deal with natural monopolies. It recalls the ill experiences with government intervention in finance through state banks and development finance institutions and policies of directed finance. It then circles back to issues introduced in earlier chapters—credit bureaus, collateral registries, promotion of competition by a variety of financial institutions. On prudential regulation, it concludes: "banking systems seem to work better when market discipline is encouraged through market monitoring—not strong supervisors" (p. 123).

The treatment of infrastructure supply and pricing is similar, with almost all emphasis on increasing competition by removing government barriers to it with little concern for market power that may be exercised by private firms.

The *WDR* devotes one chapter to workers and labor markets. Some of the topics, such as the minimum wage and laws affecting worker dismissals seem close to the core theme of the report, others such as policies that foster economically beneficial education seem to drift further and further away.

Throughout, the *WDR* adduces support for its positions from Business Opinion Surveys that report the responses of firm managers and owners. In my view, the *WDR* gives these opinions too much weight. For example, on page 5 it

reports the ratings of 26,000 firms in fifty-three countries of the severity of constraints such as corruption, taxes, finance, courts, electricity, labor regulations, and transportation. Figure 2.7 reports the percent of firms identifying policy uncertainty as a concern. Figure 4.3 reports that “many firms do not believe the courts will uphold their property rights,” although the meaning of abstractions such as property rights is unclear and therefore does not help in devising and implementing specific reforms.

The obvious worry is that these opinions about business conditions are not a reliable guide to constraints firms face and priorities for reform. First, there is no reason to think that just because these people spend their time making profits they are very good at or motivated to reflect in an articulate way on their situation. It is hard enough to get people to answer accurately about variables that are objectively measurable in principle, such as the amount of their fixed investment last year. The opinions of a bunch of possibly grumpy people harassed or cajoled into filling out a survey is not a substitute for a formal analysis of the impact of objectively measured constraints. As the *WDR* itself remarks: “For as long as governments have levied taxes, those who pay them have complained” (p. 107)—but this observation does not make a strictly positive level of taxation any less inevitable or socially desirable. Second, there is every reason to expect that the answers to such questions are influenced by changes that have recently occurred, changes that might be under consideration or changes that business people think could plausibly be made, rather than a calculated benefit/cost analysis even one done from the exclusive perspective of the private firm. In short, what is on their minds easily might not be what really constrains firms, and this *WDR* provides no reassurance about these concerns. All in all then, owners of firms are not well-positioned to know which alternative configurations of, say, the commercial court system might improve their economic prospects and are therefore in no position to say that it is a lesser priority than a reliable electricity supply which they might more easily envision or tax payments which might simply enrage them. Third, the opinions of business people do not in any way speak to the social concerns that correctly were made central right at the beginning of the *WDR*. Presumably, the owner of a firm that is polluting thinks that any restriction of pollution is intrusive regardless of social considerations. In short, the business opinion survey is an unsound methodology and its use should be terminated. And finally, many of the variables that firm owners opine about deal with endogenous factors that are merely associated with other desirable development outcomes rather than having the causal significance that the *WDR* often attributes to them. There are analogous shortcomings in the *WDR*’s use of such indices as the “aggregate governance measure” in figure 1.1 or the “investment profile” in figure 1.10.

Everything taken together, then, this *WDR* is broad rather than deep, long on general exhortations to cut back on government activity and short on specific recommendations for the design of interventions to deal with market failures. A lot more could have been done if the report had focused more narrowly, for instance on the rules and institutions that promote property rights and the enforcement of contracts. Other *WDR*’s have had as their sole focus some of the other topics covered in this one, like infrastructure, and subsequent *WDR*’s could look at some of the topics that could only be given inadequate attention in this one, such as taxation or employee/employer relations. And finally, this report would have benefitted from a closer attention to published research that has already been thoroughly reviewed while avoiding the dissipation of its own resources on such ill-advised novelties as the results of opinion-based business surveys.

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The theme of this year’s *World Development Report* (*WDR*) is “Better Investment Climate for Everyone.” The *WDR* starts off by arguing that good investment climate, i.e., the local institutional, regulatory, and policy environment in which firms operate, stimulates economic growth by providing firms with the incentive to invest and improve productivity. This message echoes a common belief among economists. Recent

research has emphasized that aggregate incomes are higher in countries with secure property rights, institutions that can enforce contracts, macroeconomic stability, better governance, and open borders. Of course, a debate persists about the direction in which the causality runs (a nuance that the *WDR* fails to raise) and how exactly poor countries can attain these characteristics. My comments below focus on what the *WDR* has to say in the area of international trade policy discussed in various parts of chapters 1, 3, 5, and 10, and on some strengths and limitations of the Investment Climate Surveys, a new data source heavily relied upon in the current *WDR*.

On the trade policy and foreign investment front, the *WDR* offers standard policy prescriptions that governments in the developing countries should "take advantage of international openness" by continuing to lower policy-related barriers to exporting, reducing import tariff and non-tariff barriers to trade, and removing policy restrictions to inflows of foreign direct investment (p. 64). It also encourages developed countries to improve market access to exports from poor countries, emphasizing the importance of lowering tariff and non-tariff barriers on agricultural products and textile exports from poor countries. In addition, the *WDR* draws attention to potentially large (but often ignored) costs imposed on firms involved in international transactions through long delays in the time it takes for goods to clear customs and the corruption in custom administration. The report thus also calls for greater efficiency in custom administration.

The *WDR* justifies these recommendations by pointing to the standard benefits of freer trade for a country's welfare and growth. It begins by arguing that there are virtually no countries that have grown persistently without opening up markets. It then points to benefits of exporting for firms through greater exploitation of economies of scale, productivity gains, and spillovers associated with exporting. Here, the report probably gives a bit too much emphasis to the idea that exporting leads to greater firm productivity and spillovers, given that the main studies in this line of work show that this positive association is mainly driven by selection of more productive firms into the exporting market (see Bernard and Jensen 1999, Tybout et al. 1998). The *WDR* then reviews the empirical evidence that suggests that lower import tariffs are associated with greater competition in domestic markets, greater availability of goods (including inputs for firms), and greater productivity among import-competing firms. On the latter, the *WDR* highlights the importance of the reallocation of resources from less to more productive firms, and the interactions of lower barriers to international trade with low policy barriers to entry and exit (and expansion and contraction) of firms, for industry growth. Overall, this discussion provides a good overview of what has been learnt in the past decade about how trade policy and foreign direct investment affect firms' performance.

Unfortunately, when it comes to the discussion of how trade policy (and investment climate more broadly) affects the lives of the poor, the *WDR* mainly evokes the argument that a better investment climate promotes aggregate growth and that, in general, aggregate growth reduces poverty (see, for example, the discussion on page 31). When it points to more micro evidence or anecdotes on pages 32–34 and on pages 64–67, it emphasizes gains to the groups that benefited from the reforms through job creation (for example, 200,000 new jobs created in Cambodian garment sector following an export boom during the late 1990s), and lower prices of goods and services. Yet, it fails to discuss the distributional effects of trade reforms although a large body of research suggests that wage inequality between educated and less educated workers has grown in many developing countries subsequent their trade and FDI reforms (see Goldberg and Pavcnik 2004 for a recent survey). The *WDR* also fails to acknowledge that, despite the widely held belief that the trade reforms in developing countries will benefit the poor, the empirical evidence on this front is lacking. Fortunately, this state of affairs has started to change with the work by Porto (2002) that examines how trade reforms might affect the poor through consumption and labor income channel in the case of Argentina (which the *WDR* surprisingly fails to discuss), and a more recent collection of papers in the forthcoming NBER volume on Globalization and Poverty edited by Ann Harrison. These forthcoming studies point to surprisingly low mobility of individuals across sectors or regions in response to trade shocks as a potentially important (but not well understood) component of how trade policy might affect the poor. For example, a study by Topalova (2005), finds that although

poverty has declined in India overall following the 1991 trade reforms, poverty declines were smaller in areas specializing in industries with bigger import tariff cuts. In addition, the individuals living in Indian States with labor market regulation that favors workers over business owners experience smaller declines in poverty (or greater increases) in response to trade reform than individuals from Indian states where labor markets are less regulated. Topalova conjectures that these individuals fare relatively worse because immobility (potentially related to labor market regulation) precludes them from reallocating from sectors/regions that were hit hardest by declines in tariffs toward sectors/ regions that benefited from the reforms.

That said, the *WDR* is humble to acknowledge that much remains to be learnt about how various components of investment climate affect the firms and that this state of affairs in part reflect the lack of appropriate data. The World Bank should be applauded for trying to fill this data void through the newly available *Investment Climate Surveys* (ICSs). The ICSs are *cross-sectional* surveys conduced by the World Bank and national statistical agencies of over 30,000 firms in more than fifty countries. They collect information about firms' perceptions of investment climate ranging from physical infrastructure, rule of law, enforceability of contacts, taxation, and regulation, in addition to information on various characteristics and measures of performance of firms. The surveys aim to provide indicators of investment climate that are comparable across (and within) countries and are more detailed than the commonly available country-level indices of institution quality, security of property rights, etc. The *WDR* views the ICSs as a crucial tool for policymakers and for the Bank's own operations and technical support because the ICSs supply them with the information needed to assess the state of the investment climate in a country and identify the main constraints that are holding back firm growth. In fact, a significant amount of the findings (and a vast majority of the graphical materials) in the current report rely on preliminary research based on the ICSs.

While this detailed information is obviously useful for identifying the constraints that might be holding back existing firms, one should be cautious when using the ICS data to evaluate the implications of investment climate measures for firm performance. For example, on page 31, the *WDR* claims that ICSs "make it possible to see how broader packages of policy improvements can influence firm performance by use of counterfactual comparisons." The report then illustrates in Box 1.10 on page 31 this potential by an example: "if the investment climate for firms in Dhaka, Bangladesh, matched that of Shanghai, Dhaka would reduce its productivity gap by 40 percent, and wages could rise by 18 percent. For Calcutta, the effect is even larger: 80 percent of the productivity gap could be closed, and wages could rise by 38 percent." For a policymaker, it might be more informative to learn about the returns to a particular policy change rather than imprecisely defined "broad package of reforms." Also, the inference based on the cross-sectional data from the ICSs is potentially misleading. The firms' perception of the investment climate will in part depend on firms' performance. In addition, more successful firms might be selecting to operate in Shanghai (rather than in Dhaka or Calcutta) because the location offers a better business environment and more educated labor force (and not because a better business environment caused the firms to be successful).

One wonders whether a better alternative to understanding the implications of the investment climate reforms would be to focus on evaluating the consequences of implemented reforms in a country by linking measures of the particular policy reform across locations (or sectors) within a country to the existing databases of firm-level (or industry-level) performance measures. This approach has been increasingly used in studies ranging from the analysis of the consequences of trade reforms for firms' productivity and competition (see Tybout 2003 for a survey) to the implications of labor market regulation for growth (see Besley and Burgess 2004). The World Bank could serve an important role by encouraging statistical agencies to make micro-level surveys more accessible to researchers and policymakers and to merge them with the administrative data on the extent of various reforms. By linking the administrative data on a particular investment climate policy change to household surveys of location specific poverty measures, this approach might also provide a more direct mapping of how various policy reforms in the area of investment climate affect the poor as has recently been done by

Topalova (2005) for trade reform and by Duflo and Pande (2005) for the implications of large dam construction in India.

Overall, the *WDR* does a good job drawing the attention of policymakers and public at large to the importance of investment climate for firms' investment decisions and aggregate growth. Nonetheless, in the discussion of the trade policy and other aspects of globalization, the *WDR* would be better served if it provided a more balanced discussion of distributional consequences of the advocated reforms and emphasized the gaps in our understanding of how investment climate improvements affect the lives of the poor. Fortunately, these issues have recently received increased attention among researchers, and I am optimistic that in the next few years this line of research will improve our understanding how investment climate affects firms, growth, and ultimately poverty.

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The Decline of the Welfare State: Demography and Globalization. By Assaf Razin and Efraim Sadka. In cooperation with Chang Woon Nam. CESifo Book Series. Cambridge and London: MIT Press, 2005. Pp. viii, 133. $28.00. ISBN 0–262–18244–0. *JEL 2005–1452*

Broadly viewed, the modern welfare state is a device for redistributing income. Redistribution takes place from young workers to old retirees and from the rich to the poor. Typically these transfers are financed on a pay-as-you-go basis by labor income taxes, such as payroll taxes, although in principle other taxes such as capital income taxes can and are also used. Two key features of recent demographic and economic change are conspiring to doom the ability of the welfare state to finance itself. First, in the world's major industrialized countries, where the welfare state appears to be a fixture, aging populations and the immigration of low skill workers have increased the demands on the welfare state while simultaneously increasing the tax burden that must be borne by young workers and the rich if benefits to the elderly and the poor are not to be cut. For example, in the United States, the dependency ratio, the ratio of those aged 60 and over to those ages 15–59, is expected to rise from 27 percent to 47 percent over the next fifty years. The problem is even more severe in the European Union and Japan where the ratios are expected to rise from about 35 percent and 36 percent to 66 percent and 70 percent. Second, the world economy has become increasingly integrated in recent years. This phenomenon of globalization is by now well documented. Given the reliance of the welfare state on factor market taxes to fund its redistributive programs, the increasing integration of world capital markets and, although to a lesser extent, labor markets is most relevant. Globalization has also been accompanied by increasing international tax competition as countries seek to make themselves more attractive havens for investment. Together globalization and international

tax competition have combined to reduce the tax base of the welfare state, further increasing the difficulties of financing the redistribution of income that is at its heart.

These are the issues that are explored by Assaf Razin and Efraim Sadka (in cooperation with Chang Woon Nam) in their recent monograph *The Decline of the Welfare State: Demography and Globalization*. The book begins with an introductory chapter that motivates the issues and then moves quickly into presenting models and empirical evidence dealing with each in turn. Chapters 2 and 3 examine the implications of population aging and immigration for tax rates and benefit levels in a median voter political equilibrium. It is then argued in chapter 4 that balanced budget rules, such as the European Union's Stability and Growth Pact, are an impediment to many of the reforms that may be needed to maintain the welfare state. Chapters 5 and 6 explore the implications of aging for capital income tax rates and benefit levels in political equilibrium for the welfare state. Finally, chapters 7 and 8 explore how globalization and international tax competition hamper the ability of the welfare state to resort to using capital income taxes to fund benefits.

Throughout the book, Razin and Sadka use simple overlapping generation models to develop predictions concerning the effects of an aging population, immigration, and globalization on equilibrium tax rates and benefits levels. Tax rates and benefits are determined in a political equilibrium using the median voter model. In order to model the distribution of income, Razin and Sadka assume that individuals differ in terms of innate ability with the more able requiring less time to invest in productivity enhancing education. In addition to this time cost, there is also a "tuition" cost of acquiring an education that is not tax deductible. Empirical evidence relating to the model's predictions is provided following each variant of the model.

As already noted, the sharp rise in the dependency ratio that is expected in coming years will require the welfare state to increase tax rates on young workers and the rich in order to maintain benefits. The rising share of the elderly in the population along with the increasing numbers of naturalized immigrants would seem to tilt the political balance in favor of the welfare state by providing broader political support for increased taxes *and* benefits. However, Razin and Sadka argue that there are important indirect effects that work against this tendency. In fact, these effects may be strong enough that the welfare state may actually shrink rather than expand in response to aging and immigration. They go on to argue that because of the forces working to shrink the welfare state as the population ages, governments may be forced to rely more heavily on capital income taxation (as opposed to taxing labor income more heavily). Again, this result seems counterintuitive since old retirees get most of their income from capital. Finally, Razin and Sadka show that globalization and increasing international tax competition severely limit the scope for shifting the burden of financing the welfare state from labor to capital. Intuitively, globalization makes capital and labor more mobile making it easier for them to flee any increasing tax burden the welfare state might try to impose. Only stricter enforcement of international tax laws and an unprecedented degree of international tax coordination can salvage things. The conclusion seems to be that the welfare state's demise is eminent in the face of aging and globalization *unless* we see the rise of a sort of world welfare state.

Two key features of the model underlie almost all of the results. First, the interaction between labor taxes and the tuition costs of education results in a distorted labor choice and associated welfare cost. Second, with a growing population, the median voter, the voter that determines tax rates and benefits in the welfare state, is not an old retiree but a young worker. Whether the median voter turns out to be a high income educated worker or a low income unskilled worker, aging and immigration both give rise to forces that may cause the welfare state to shrink.

All of the models presented are crisp and clean and the intuition underlying what are really fairly complicated results is lucidly presented. In fact, reading the book one is struck by how such a simple model is able to yield so many insights into such a wide range of issues. However, the implications of virtually all of the models presented are ambiguous. One set of forces works to expand the welfare state as the population ages while another works to shrink it. There is no way in general to tell which will dominate. As a result of this ambiguity, the empirical evidence Razin and Sadka present cannot properly be viewed as a test

of the model but rather the evidence serves to illustrate which set of forces must dominate if the model is to be consistent with the data. One interesting exercise would have been to choose parameter values for the model so as to mimic the distribution of income (appropriately defined) and the levels of tax rates and benefits currently observed in various welfare states and then compute the actual response of tax rates and benefits to aging. In principle this should be a fairly straightforward exercise given that the models have been designed to be simple enough to yield analytical results.

The conclusion that the welfare state is doomed because globalization and international tax competition severely undermine the ability of the welfare state to substitute capital income taxation for labor income taxation as the population ages is striking. However, it may be overstated. When examining the implications of globalization for the welfare state, Razin and Sadka take world interest rates as given. The problem with this is not so much the implicit small country assumption underlying the analysis, but rather the assumption that other welfare states are not experiencing the same financing difficulties. Since aging is a demographic factor in many if not all welfare states, this is simply unrealistic. The international tax coordination needed to prop up the welfare state may not require the formal emergence of a world welfare state. The de facto emergence of a world welfare state, as each country working in its own best political interest simultaneously raises capital income tax rates in response to an aging population, may be enough.

Overall the book is interesting and provocative. Given the attention currently being given to social security problems and reforms throughout the world it is certainly timely. For those looking for a brief introduction to the issues surrounding the policy debate and formal economic models dealing with the issues, it is hard to imagine doing better. The book is published by MIT Press as part of the CESifo Book Series (Center for Economic Studies and Ifo Institute for Economic Research) originating in Munich. According to their website, the goal of the series is to "present an overview of current issues in various economic areas and thus contribute to the economic policy debate." The book serves this goal more than admirably.

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Beyond Oil: The View from Hubbert's Peak. By Kenneth S. Deffeyes. New York: Farrar, Straus and Giroux , Imprint: Hill and Wang, 2005. Pp. xv, 202. $24.00. ISBN 0–8090–2956–1.
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JEL 2005–1483

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JEL 2005–0833

The Party's Over: Oil, War and the Fate of Industrial Societies. By Richard Heinberg. Gabriola Island, Canada: New Society Publishers, 2003. Pp. xii, 274. $17.95, paper. ISBN 0–86571–482–7. *JEL 2005–1141*

The world has already used about one trillion barrels of the planet's finite supply of recoverable petroleum. The question is how much is left. Most industry and government sources estimate that more than two trillion barrels remain, which means some eighty years supply at the current rate of oil consumption. However, increasing numbers of oil analysts are suggesting that such estimates are excessively optimistic and that the planet has already used up almost half of its original petroleum endowment.

How do these "cassandras"—so-called to distinguish themselves from the "cornucopians"—reach their pessimistic estimates? Partly on anecdotal evidence: (1) discovery of new oil reserves on the planet peaked some forty years ago and oil consumption has been outrunning oil discovery for

twenty-five years; (2) all recent discoveries of oil have yielded small fields; (3) all the big oilfields in the world are showing technical signs of decline; and (4) everyone, including Saudi Arabia, is now drilling at capacity and still world output has stagnated in recent years.[1] Moreover, the world oil industry is behaving as if it knows the downturn in oil production is imminent: recent oil price increases are stimulating little new oil exploration; U.S. refineries are running near capacity but no new refineries have been built in the last thirty years; oil tankers are fully booked but are being retired faster than new ones are being built; and mergers and takeovers by oil giants are increasing as they seek to acquire existing reserves.

But their principal evidence comes from time-series analysis of past world output to determine when world output will peak and begin to decline. This peak is called Hubbert's Peak after M. King Hubbert, a Shell Oil geologist who applied a logistic growth curve to U.S. oil production data:

$$(1) \qquad (\mathrm{d}Q_t/\mathrm{d}t) = kQ_t(Q^* - Q_t)/Q^*,$$

where Q_t is cumulative oil production to time t, $\mathrm{d}Q_t/\mathrm{d}t$ is output rate at time t, Q^* is the original recoverable oil endowment of the United States, and k is a parameter. Dividing equation (1) by Q_t permitted him to fit a linear regression:

$$(2) \qquad (\mathrm{d}Q_t/\mathrm{d}t)/Q_t = k + (k/Q^*)Q_t,$$

which yielded estimates of k and Q^*. Extrapolation then permitted Hubbert to estimate the date at which Q_t would equal $Q^*/2$ and $\mathrm{d}Q_t/\mathrm{d}t$ would begin to decline. In 1956, he published this date—1970. Everyone laughed. He was right.[2]

Since then, many analysts (including Hubbert himself) have tried this same procedure on world oil production data, and have typically estimated Hubbert's Peak for world oil output to be somewhere between 2000 and 2015—i.e., world oil production will begin to decline very soon, if it has not already peaked. But the Hubbert's Peak procedure puts a heavy burden on a statistical form that is very weak in its conceptual foundations—applying a model developed to study the limits to population growth directly to the limits to resource depletion ignores the many factors that differentiate living beings from inanimate objects, and assuming that production begins to decline when half the world's recoverable reserves have been extracted ignores any influence of price on the rate of production. Moreover, the resulting parameter estimates are highly dependent on the data years used—adding or subtracting a decade of data to the regression changes the estimated date of the Hubbert's Peak by about a decade.

These five books seek to separate themselves from "cornucopian" estimates of oil reserves by stressing the different policy prescriptions that follow—imminent crisis in the cassandra case versus distant worry in the cornucopia case. In fact, however, the two approaches are not so far apart. Consider the cornucopian estimate of the U.S. Geological Survey (USGS) that more than two trillion recoverable barrels of oil remain. At the recent rise in world demand for oil (over 1983–2003) of 1.3 percent per annum, the Hubbert's Peak (when another half trillion barrels are used and half the world's oil is gone) will be reached in eighteen years. Indeed, according to the USGS, at this 1.3 percent growth rate (assuming we can find and extract the recoverable oil as fast as we need it), *all* of the earth's original endowment of three trillion barrels will have been extracted by the middle of this century. Considering that the average new car lasts about eighteen years (in the United States, less in Europe, more in poor countries), both approaches strongly urge action now to find, develop, and adopt new energy sources.

All these books see Micawberan outcomes around the Hubbert's Peak date—world oil output growing at 0.01 percent per annum, result happiness; world output falling by 0.01 percent per annum, result misery. Apparently, none of these authors can visualize a gradual reduction in unimportant auto trips and a gradually heightened urgency to commercialize and adopt alternative

[1] Even the discoveries of 1999 and 2000 in Kazakhstan and Iran are estimated to provide a total of only seven months' supply at current world consumption rates.

[2] Hubbert's original analysis followed a slightly different method. In an earlier book (*Hubbert's Peak: The Impending World Oil Shortage*, Princeton: Princeton University Press, 2001, p. 135), Deffeyes, who was Hubbert's colleague in the late 1950s, suggested that Hubbert reached his conclusion first and then searched for data and methods to support it. But fitting equation (2) to the U.S. data of 1930–55 does come close—it yields 1967 as the estimated date of Hubbert's Peak in the United States.

energy technologies. Following the logic of the logistic curve, not until nearly twenty years after the Hubbert's Peak date will the planet's annual oil production rate have fallen by as much as one-fourth of its peak rate. Nevertheless, Deffeyes promises "war, famine, . . . and death" (Deffeyes, p. 8).[3] Heinberg and Roberts foresee "wrenching" changes (Heinberg, p. 149; Roberts, p. 9). Goodstein sees "crisis" and "painful" (Goodstein, p. 123). And Kunstler believes that even "moderate" declines in oil output will "crush our economy" (Kunstler, p. 4).

Why do these authors insist that a gradual transition to a new energy regime is not possible? Partly their answer is political and psychological. Existing energy industries "have zero interest in seeing the emergence of competing technologies" (Roberts, p. 191). "Unfortunately, our present national and international leadership is reluctant even to acknowledge that there is a problem" (Goodstein, p. 123). "We can no longer conduct contests in which politicians try to outbid each other in promising higher growth rates" (Deffeyes, p. 182). "When we operate on the basis of false expectations, we live in a state of confusion, and our attempts to solve problems are unlikely to be effective" (Heinberg, p. 240). We just "cannot survive on less than the amount of oil generated at peak" (Kunstler, p. 25).

The long-term solution to our power source, should civilization survive the transition to it, is clear to all these authors. It will involve nuclear fusion and solar power, probably with the help of hydrogen and fuel-cells. But these are "still decades away from mass deployment" (Roberts, p. 190). And none of these authors consider things like hybrids, diesels, trains, buses, and price elasticity of demand to be adequate to bridge these decades. Furthermore, they cannot decide on any existing fuel that can be made sufficiently energy-efficient, pollution-free, and low-cost to provide an acceptable bridging fuel. Their views of the principal bridge candidates:

1. *Coal*. It is seen as an essential part of the transition, but even with our fairly abundant coal, "the end of the age of fossil fuel . . . will probably come in this century" (Goodstein, p. 34). Heinberg and Kunstler note that the best coal mines are already depleted and that mining coal will be at increasingly higher private cost—plus the multifarious external costs that accompany coal. Nevertheless, as Deffeyes sighs, "I hate to say it, but we likely will be forced to choose" coal (Deffeyes, p. 98).

2. *Natural Gas*. Goodstein, Deffeyes, and Kunstler agree that natural gas is only a temporary help since the Hubbert's Peak date of natural gas will "occur only a couple of decades after the one for oil" (Goodstein, p. 33). Heinberg and Kunstler go further, pointing out that the former SSRs and Middle East can pipe natural gas to Europe and the East Asia, but they can only expensively and dangerously transport it to the United States as liquid natural gas (LNG). Kunstler adds that siting LNG-using plants is made difficult by neighbor fears (though I have never seen panic among the sun-bathers on a fancy beach in Barbados when the weekly LNG tanker off-loads from a berth 100 meters offshore). Roberts, however, is more optimistic, suggesting that the world's natural gas could serve as the bridging fuel "for more than a half century" (Roberts, p. 167).

3. *Nuclear Fission*. Heinberg and Roberts maintain that "it would be a disastrous error to turn to nuclear power when energy shortages arise" (Heinberg, p. 139). They offer the usual reasons: the fuel is not abundant; uranium mining is wasteful, polluting, and dangerous; nuclear power is not safe. In short, the nuclear path is "our most expensive conventional energy source" (Heinberg, p. 136), and "its future is in doubt" (Roberts, p. 190). But the other authors reluctantly recognize the importance of nuclear power in the transition: "there may be no alternative" (Goodstein, p. 104); "we will have to use nuclear" (Kunstler, p. 140); and "eventually, it has to happen" (Deffeyes, p. 150).

4. *Wind, Tidal, and Solar Power*. Roberts thinks these could provide a maximum of 20 percent of our power because they are expensive and intermittent (Roberts, pp. 201ff). Goodstein agrees—"we won't be able to live on it" (Goodstein, p. 111). Heinberg is more optimistic; while wind power cannot help with the transport fuel problem and would require "huge investments" in turbines and transmission lines, it is still "most compelling" (Heinberg, pp. 141f). Deffeyes is also (albeit briefly) optimistic—"the

[3] The omitted part of this quote is the word "pestilence," but Deffeyes thinks "we might avoid the pestilence part" (ibid.).

Wyoming wind never seems to stop blowing" (Deffeyes, p. 183). And Kunstler would like to be optimistic, claiming that solar and wind power could buy us time if we committed to it, "but don't count on it happening" (Kunstler, p. 128).

5. *Ethanol*. Goodstein, Heinberg, and Deffeyes all state that ethanol is a net energy loser. Heinberg goes further: "If the entire U.S. automotive fleet were to run on pure ethanol, nearly all of the continental U.S. would be required in order to grow the feedstock" (Heinberg, p. 157). And Kunstler dismisses it with "forget biomass" (Kunstler, p. 138). Only Roberts supports it, hinting that "biofuels produced from marginal croplands could replace a fifth of U.S. transportation fuel" (Roberts, p. 79).

What's in these books for economists? Not much economics. All the authors (except Roberts) appear to share a suspicion of economic thinking. Comments like the following pop up regularly: "There is a school of thought, whose ideas are voiced mostly by economists, that says there is plenty of oil" (Heinberg, p. 102). Harold Hotelling is not mentioned once in any of these books, although in fairness, the standard Hotelling model offers little insight into the oil market, where government policies and monopolistic forces are so deeply and intricately involved, where no reliable estimates of proven, recoverable reserves are available, and where the existence—not to mention the cost—of potential energy alternatives is unknown. Two of the books trot out the Kenneth Boulding saw: "Anyone who believes exponential growth can go on forever in a finite world is either a madman or an economist" (Deffeyes, p. vii; Heinberg, p. 167).[4]

Instead, the books share an insight into the enormity of the scientific and political problem of switching from a fossil-fueled economy to something else. Since few readers of this review will want to read all five of these books, let me end by pointing out what the distinctive features of each are.

Deffeyes. This is the best analytical development and exposition of the Hubbert's Peak analysis, and it is a useful and interesting run through the possible short-run bridging fuels. It stresses what can be economically adopted quickly, as it must since Deffeyes's estimated date of Hubbert's Peak is 2005. The book reads very well—like a lecture from a fascinating lecturer—but the sidetracks are sometimes too long and irrelevant—e.g., "one of the reasons for focusing on [biological] diversity is sheer intellectual laziness" (p. 173) and "if you teach calculus to teenage girls, they go on to have far fewer babies" (p. 177). Overall, I liked the book, partly because I share many of his cantankeries, but it is not the one to read if you are reading only one of the five books and want to maximize learning-per-page.

Goodstein. This is really two books. The first fourth and last fourth of the book contain (in about fifty pages) the guts of the problem and the possible (and difficult) solutions. It is succinct and well-written—I liked it best of everything in all five books. Goodstein calls things clearly from the start, the book's first line being "the world will soon start to run out of conventionally produced, cheap oil" (p. 15). And he treads nicely between pessimism and optimism, the book's final line being "civilization as we know it will come to an end sometime in this century unless we can find a way to live without fossil fuels" (p. 123). In between these fourths is a mistake—energy for poets. It offers a fast history of how people have harnessed energy ever more efficiently and a brief explanation of the limits to the energy we can harness. It suffers from the problem that most "what-ology for poets" courses do—it is not deep enough to be fully understandable and it is too deep to be easily understandable. In short, to maximize learning-per-page, read the first and last fourths of this book.

Heinberg. A curious book. When directly discussing energy issues, it is well-informed and well-written, especially in the chapter on the possible alternatives to petroleum. But the hobby-horses he rides are legion. He especially dislikes economists, who to him are all just like Bjorn Lomborg (who isn't an economist by training). He is a fan of Henry George. He is not above a cheap shot, such as writing that Robert Solow said "the world can, in effect, get along without natural resources" (p. 3) when the full quote is: "If it is very easy to substitute other factors for natural resources, then there is in principle no 'problem.' The world can, in effect, get along without natural resources, so exhaustion is just an event, not a catastrophe." Two recurring themes are "the political influence of car and oil

[4] Whenever I see that quote, I wonder if Boulding had read Hotelling.

companies and the general corruption and inertia of the political process" (p. 148). While Heinberg thinks we can never fully replace oil, we may nevertheless be better people, if not better off, in "an energy-conserving society that is less mobile, more localized, and more materially modest . . . [that] may bring highly desirable lifestyle benefits for our descendants" (p. 165).

Kunstler. We have been living in a "fiesta of luxury," now we are "running on fumes," and we face "the Long Emergency . . . [when] nobody will get anything for nothing" (pp. 304, 185, 303). It is sprightly writing, but the message is dark. Our material standard of living will be much lower. But the end of oil will serendipitously rid us of agrobusiness and "monopolistic global corporate enterprises" (p. 256), rid us of a large number of family cars, rid us of much of suburbia, rid us of most of the interstate highway system, vastly increase reducing, reusing, and recycling (because new goods will be much more expensive), return us to masonry and wood house construction, revitalize railroads, and reduce the amount of schooling—which is a good thing since it is currently "little more than day care for virtual adults" (p. 271). (He is also pleased to forecast that the "red" states will fare much worse than the "blue" states.) If you want an alternative to the (very guarded) optimism of the other books, this sci-fi-like descent into global darkness is a good read.

Roberts. The welcome aspect of this book is that he knowledgably discusses external costs, carbon taxes, feebates, and cap-and-trade systems. The unfortunate aspect is that his economics is shaky. For example, he adds the low extraction cost of Middle East oil to its shipping costs and concludes that the total would be the price of OPEC oil "in a free market. OPEC members, however, base their oil price not on actual costs of production but on the very high level of revenue they need to keep their corrupt and inefficient governments in the black" (n. 21, pp. 347f). He periodically and muddily revisits the question of why oil prices aren't already higher if oil is already getting so scarce. His answers range from oil scarcity is "not yet apparent to the market" (p. 271), which is hard to believe considering that clear vision would be worth many billions of dollars, to OPEC restricts its production, forcing the world to overproduce high-cost oil, which keeps the current oil price too high, and "masks many changes in long-term supply" (p. 61), which is contradictory. Despite the often-maddening economic arguments, the book covers the history and issues very well, and it is engagingly written. It is the one I would read if I were reading only one of these five books.

All these books may exaggerate the imminence of Hubbert's Peak and the catastrophe that inevitably follows it. But they do correctly warn us that the Peak is nigh, if not already here, and that it is none too soon to get serious about preparing for it. The last sentence of the Roberts book sums up the message of them all: "the question facing us isn't whether our energy systems will change . . . but whether we can live with the outcome" (Roberts, p. 332).

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The Economics of Energy Efficiency: Barriers to Cost-Effective Investment. By Steve Sorrell et al. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2004. Pp. x, 349. $110.00. ISBN 1–84064–889–9. *JEL 2005–0443*

If there were $20 bills on the ground, someone already would have picked them up. That is, unless to that someone they are no more useful than foreign currency that for some reason cannot be readily exchanged. Then, one might just leave the bills on the ground and not bother to pick them up. After a while, one might not even notice them; they would seem to become invisible.

In *The Economics of Energy Efficiency*, Sorrell and coauthors argue it is not individuals but institutions that leave money on the ground. The authors investigate the alleged existence of unrealized opportunities for cost savings through improved energy efficiency—the metaphorical $20 bill that has spawned a familiar debate. If there exist unrealized, cost-effective opportunities to save energy, the dialogue goes, then competitive market pressure should ensure that the opportunities are realized. At least in the long run, such $20 bills cannot exist. Others reply with a litany of reasons why such opportunities are not realized, especially in the short run.

Sorrel et al. have put together an outstanding collection of five detailed case studies that advance this debate considerably. The case studies include higher education in Germany and the United Kingdom, brewing in the United Kingdom, and mechanical engineering in Ireland. In these four cases, energy use represents from

2–4 percent of the organization's costs. A fifth study involves the construction of buildings, which are responsible for 47 percent of final energy consumption in the United Kingdom. Their project is to learn whether unrealized opportunities to save money through energy efficiency are common and what institutional features explain their existence.

As a collection, the studies share the virtue of a common structure in the organization of each chapter, which makes comparison of the studies useful and easy. More importantly, the authors approach the question as scholars rather than as advocates. Preliminary evidence must have helped motivate the selection of these case studies and all the case studies show short run cost-saving opportunities and, therefore, presumed evidence of hidden costs. However, the conclusions and implications do not appear predetermined. Each study offers interesting insights as to why opportunities may continue to be unrealized, why they can be of an important magnitude, why they might involve an institutional failure of some sort that invites corrective action, or why they might not.

Sometimes the studies identify technological opportunities that cut across the industry; in other cases the opportunity for energy savings are process specific. Other times, to realize energy savings requires changes in organizational technology. For example, savings in one department may not be realized on the budget of that department. Investment in efficiency may come at the expense of the department's other operational priorities. Amidst competing priorities in the current year and absent global budgeting, cost reductions from energy efficiency measures might lead to a direct reduction in the department's budget in a subsequent year, without any reward for the cost incurred by that department.

Investments in efficiency may be diffuse across the physical plant or difficult to monitor, while administrative procedures that are intended to guard against fraud under just such circumstances introduce an expensive rigidity to the process. Or, it simply may be that efficiency investments require construction activities that are run by a different department, which has its own priorities, causing unfortunate delays.

Moreover, cost savings compete with priorities on the other side of the ledger, and the latter may be more urgent. The brewing firm faces pressure to increase revenues in the current quarter amidst overall declining industry sales. The pressure is so immense that cost savings to be realized over a two or three year time frame are not on the priority list. In higher education, the other side of the ledger corresponds to the prestige and rank of the institution and its faculty.

The book is organized with a thorough second chapter introducing the theoretical framework for the issues that are examined, a broad literature review as well as a justification for the case study methodology. This useful chapter has to appear where it does in the book, but it may be best if not read there; it is a bit lengthy if read straight through. Most readers will benefit from jumping into the case studies. Chapter 8 presents an excellent summary of the studies and an overarching evaluation of barriers to investments in energy efficiency: risk, imperfect information, hidden costs, access to capital, split incentives, and bounded rationality. Ultimately, the authors conclude, the cumulative effect of barriers is more important than any one barrier. Removal of one barrier may be only partially successful if other barriers remain. It is most interesting to return to chapter 2 after having read at least some other portion of the book, because then the theoretical framework takes on greater relevance.

The best case studies for the classroom debate are the two on higher education, although economics students and business students may perceive the issues differently. For the economics student, the conceptual test is whether unrealized opportunities—the so-called efficiency gap—are due to the existence market failures or to hidden costs. The former invite policy intervention, the latter do not. The business student is likely to be thinking about the structure of the organization. The presence of hidden costs may provide an opportunity for reform within the organization. In either case, the authors duly note, the argument over the size of the energy efficiency gap often reflects differing views on which costs can be avoided. The assignment for students is to distinguish the avoidable costs from the unavoidable ones. With this information in hand, the project of identifying remedies that would allow organizations to capture opportunities for energy efficiency may be less of an ideological debate, and more of a practical one.

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Resources for the Future

Price, Principle, and the Environment. By Mark Sagoff. Cambridge; New York and Melbourne: Cambridge University Press, 2004. Pp. x, 284. $70.00, cloth; $24.99, paper. ISBN 0–521–83723–5, cloth; 0–521–54596–X, pbk.
JEL 2005–1154

Wildlife biologist Robert Weeden once warned his brethren that embracing the monetization of environmental benefits amounts to "bedding with porcupines."[1] In *Price, Principle, and the Environment,* Mark Sagoff issues an emphatic wake-up call to those so reclined: "The attempt to elicit maximum WTP as a measure of benefit or utility appears to be a snare and a delusion of no use to environmental policy . . ." (p. 100).

In this upgraded collection of [mostly] previously published articles, Sagoff wields the rapier of his philosopher's logic to attack everything from tacky restaurants at Gettysburg National Park to the demand for toilet paper. The common theme is that economists should not put their camel's noses under the tent of the demand curve anywhere to the left of market equilibrium. Why not? Because (1) "what knowledgeable people have to say—not how much they are willing to pay—should inform policy" (p. 23); (2) preferences are hopelessly inscrutable; (3) marginal value in use bears no relation to market price; (4) the theory of market failure is "worthless" (p. 115) because the transactions costs of any remedies must exceed the benefits; (5) ecosystem services are too important to be priced, especially because they might end up priced quite low; and (6) arguments for environmental protection based on 'carrying capacity' can only serve as a "temporary defense" (p. 176). For these reasons, environmentalists should eschew valuation and fight the good fight for environmental protection on moral and aesthetic grounds.

Sagoff does a poor job of justifying propositions (1)–(4). For example, the sale of Girl Scout Cookies is deemed an "ordinary market transaction" (p. 64) and used to show that determining relevant opportunity sets and thus revealed preference is impossible. Clearly, such purchases are anything but "ordinary" and cannot be used to conclude much of anything. But even in this murky situation, I always buy Thin Mints and my partner always buys Samoas, and the scouts have no problem discerning *these* preferences from our behavior. But no matter; the cookie story, plus a few others, is sufficient for Sagoff to conclude that "The relation economists assert between preference satisfaction and concepts such as "welfare" or "well-being" is at best an empty, trivial, frivolous, fatuous, specious, and tautological one" (p. 78).

The book improves dramatically in the later chapters, as Sagoff argues propositions (5) and (6) with more data and less dogma. These chapters are stimulating contributions to current debates about ecosystem services. His debunking of the alleged savings from New York City's acquisition of land around its reservoirs is sobering. The chapter on carrying capacity is excellent, and should be required reading for all instructors, students, and cheerleaders of ecological economics. Similarly, a chapter on mechanism design extols the virtues of market-based instruments for pollution control.

The final chapter is a case study of the trials and tribulations of the Quincy Library Group, a self-organized effort of local stakeholders to resolve intractable forest management conflicts. Sagoff—and others—had high hopes for this "face to face collaboration to manage shared resources" (p. 201). Unfortunately, the outcome has been disappointing, with lawsuits, special legislation, and little progress toward implementation and environmental improvement. Those who have spent time on university governance committees are probably not surprised. Sagoff is strangely silent about the huge transactions costs that would be incurred by a wholesale embrace of local, face-to-face meetings. Too, what if one of these groups decided to undertake a contingent valuation study to aid its decision making?

Overall, Sagoff's rapier seems mostly to slice through an army of straw men. Did economists propose using cost–benefit analysis to design the Gettysburg visitor center? Or that every person consumes toilet paper until marginal benefit equals price? Nonetheless, *Price, Principle, and the Environment* is extremely well written and offers real benefits. Opportunity costs are another matter: Works such as the Diamond–Hausman critique of

[1] Weeden, Robert. 1987. "On Wooden Nickels, Trojan Horses, and Lonely Drummers." *Alaska Fish and Game*. May–June. Juneau: Alaska Department of Fish and Game.

contingent valuation,[2] or Weitzman's 1974 classic, "Prices vs. Quantities,"[3] belong higher on an economist's reading list. Environmentalists seeking noneconomic reasons to protect nature should first read Sagoff's earlier book *The Economy of the Earth.* Then again, they might also consider the advice of the author's intellectual hero, John Muir, who evidently knew a thing or two about preferences and trade-offs: "One day's exposure to mountains is better than cartloads of books."[4]

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[2] Hausman, Jerry. 1993. *Contingent Valuation: A Critical Assessment.* Amsterdam: North Holland.

[3] Weitzman, Martin. 1974. "Prices vs. Quantities." *Review of Economic Studies*, 41(4): 477–91.

[4] Quoted in Wolfe, L. 1938. *John of the Mountains*. Available at http://www.sierraclub.org/john_muir_exhibit/writings/mountain_thoughts.html.

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