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Book Reviews

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Book Reviews

Geoffrey M. Hodgson, *Economics and Utopia: why the learning economy is not the end of history* (London, Routledge, 1998), pp. 368, $100.00 hardcover, ISBN 0-415-07506-8, $29.00 paperback, ISBN 0-415-19685-X.

The collapse of Soviet-style socialism has been seized upon as an indication that there is only one viable way to organize society. The free market triumphalism that has followed in the wake of the Eastern bloc's demise has overwhelmed political cultures throughout the world, encouraging resignation to (if not open embrace of) the 'inevitability' of the 'natural order' of laissez-faire. In *Economics and Utopia*, Geoff Hodgson delivers an important riposte to this fatalism, and provides the basis for a re-invigoration of popular debate about the economy.

The book functions on at least three different (but interrelated) levels. First, it serves as a critique of recent thought on comparative economic systems and their futures, in light of the collapse of the command economies. Second, it provides a critique of economic theory, simultaneously breathing new life into the evolutionary economics of 'old' institutionalists such as Thorstein Veblen. Finally, it acts as a vehicle for reclaiming the valuable prescriptive elements of utopianism from the detritus of past utopian thinking.

Part I challenges the idea that we have witnessed the 'end of history' and the triumph of a single social system. Hodgson argues that socialism (defined here as synonymous with central planning) was a failed utopia, but that the market-individualist vision of neoliberalism is also a failed utopia, one that is infeasible in principle and that, therefore, does not—indeed, cannot—exist in practice. Ultimately, both socialism and market-individualism are identified as suffering the same flaws. First, they make unwarranted assumptions about the availability of information. The ubiquity of tacit knowledge and fundamental uncertainty render both complete planning and complete contracting impossible. Second, and related to this first point, both utopias share an emaciated conception of learning as 'information gathering'. They resist the notion of *transformational learning*, according to which learning is an evolutionary social process that affects the internal structure (the habits, preferences, aspirations, etc) of the learner. Socialists resisted this process because of their fear of counter-revolution, while market-individualists resist it because of their desire to portray the individual as given and prior in relation to society. Based on this critique, Hodgson dismisses as a fatal conceit the market-individualist claim that 'the individual always knows best'. Contesting this claim is frequently regarded as a sign of paternalistic arrogance and is associated with a statist love of 'big government' for its own sake. But Hodgson argues that it is, on the contrary, a

This review has benefited from extensive discussion with Shelby Brown, Carol Clark, Ed McKenna, Michael Sacks, Diane Zannoni and Manijeh Zavareei. It is dedicated to the memory of Manijeh Zavareei, whose warmth, wit and intellect will be sadly missed.

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sign of humility, and that it is the market-individualist position itself that is arrogant. This is because both information problems and the process of learning are trivialized when it is asserted that the individual is always best placed to determine what best serves his or her interests.

Finally, both socialist and market-individualist utopias are identified as monistic visions, that posit the necessity of just one structural form (either plan or market, respectively) for the functioning of society. They are therefore blind to the structural impurities that characterize successful social systems. According to this view, socialism failed because it attempted to supplant all other social structures with the central plan. Meanwhile, capitalism has survived precisely because it has never approximated the utopian vision of market-individualists, who propose that all other social structures can and should be supplanted by the market. Hodgson's impurity principle suggests that while all social systems are characterized by a dominant structural form (in the case of capitalism, for example, the market), other structural forms exist and persist (such as religious organizations, families and the command structure of the firm). Moreover, these 'impurities' are hypothesized as being necessary for a social system to endure. They provide structural variety, lending a system sufficient flexibility to outlast the unforeseen circumstances that are an inevitable upshot of the process of social evolution.

While Part I addressed the deficiencies of recent thought on comparative systems, Part II critiques contemporary economic theory. On the basis of three criteria for theory evaluation, Hodgson rejects Marxism, Austrian economics and neoclassical economics as inadequate for understanding the workings and prospects of contemporary capitalist economies. The first criterion is the degree of universality embodied in a theory's assumptions. Each of the aforementioned approaches is found guilty of building social theories too much on the basis of ahistorical, universal 'truths', such as axioms about individual motivation and behaviour, or abstract categories of analysis (for example, the 'mode of production'). The second criterion is the place of non-market social relations in a theory. Marxism, Austrian economics and neoclassicism are criticized for subsuming all social relations under the rubric of relations of commodity production and exchange. The final criterion concerns the relationship between the individual and society (or between structure and action). Judged on this criterion, the failings of Austrian and neoclassical theory are putatively different from those of Marxism. Ultimately, however, a common failing emerges once again—each theory posits that either the individual or the structure of society is prior to, and independent of, the other, so that there is a unique starting point for social analysis. In Austrian and neoclassical theory, the individual appears as a prefabricated atom, whose intrinsic structure is given independently of social interaction (in processes of production or exchange, for example). In Marxism, meanwhile, individual agents are subjugated to class relations, possessing no meaningful autonomy from the social structure inherent in the mode of production.

Only 'old' institutional economics (OIE) of the Veblenian variety emerges with any credibility from this exercise in comparative theory evaluation. First, the OIE better reconciles trans-historical and specific concepts, emphasizing both the former (the principle of evolution) and the latter (historically specific institutions) in its economic analysis. The concept of the institution also forges a link between structure and action, since institutions are both constituted by, and constituents of, individual agency. In other words, structure and action influence one another, neither being prior to the other in any meaningful sense. Finally, because variety is central to the principle of evolution, the OIE avoids structural monism—including an emphasis on commercial relations as the epitome of all social relations. Its structural pluralism also makes the OIE consistent with Hodgson's impurity principle.

Having established the OIE as the only suitable framework for economic analysis, Hodgson moves on in Part III to provide an institutionalist analysis of the possible fates of contemporary capitalism. Although the social blueprints and teleology of past utopian thinking are rejected, a role for utopianism remains in this exercise, in the form of envisioning *what might be*. To this end, Hodgson identifies 'scenario planning' as critical to the method of the modern utopian thinker. Scenario planning involves the identification of imagined future realities rooted in (specifically, shown to be possible evolutionary outgrowths of) current economic realities. Foremost amongst the latter, for Hodgson, is the notion that both production processes and output itself are becoming more complex and more skill intensive, resulting in a 'knowledge intensive' economy. This, he argues, poses problems for the capitalist employment relationship, the essence of which is identified as *command*—the authority of the employer to direct the labour of the employee. Hodgson suggests that it will become ever more difficult to monitor and 'boss' a collection of workers whose increasingly specialized skills may be only partly understood by their closest colleagues, much less by administrators and managers. The result is that self-direction and trust must play ever-larger roles in the social relations of production, and this involves, by definition, the withering of the capitalist employment relation. It is important to note that Hodgson does not deny the possibility of a future that involves de-skilling (in the style of Braverman, 1974) rather than the re-skilling envisaged here. Nor does he rule out the influence of reactionary forces reasserting the 'right to rule' of the property owner, or the possibility of different ownership structures accompanying changes in the employment relation. Instead, a variety of possible futures rooted in contemporary economic realities are identified. However, the central thesis is that capitalist growth is a transformational process, and that the current fruits of this process may be providing the foundations for an alternative socioeconomic system—i.e. a non-capitalist future.

Of course, just how non-capitalist this future looks depends on what one identifies as the quintessential feature of capitalism, as Hodgson himself admits. Does the withering of command and authority in the employment relationship constitute a meaningfully non-capitalist future if residual earners who are not engaged in production continue to exist? The answer to this question is far from trivial, not least because the purpose of utopianism—even of the scenario planning variety advocated by Hodgson—is the identification of *preferred* possible future realities. The extent to which knowledge-intensive production undermines authority and command is also open to question, as illustrated by the current plight of academics. The latter are frequently so highly specialized that even colleagues in their own disciplines do not fully understand what they do. And yet the trend in academia is away from the collegial model of faculty self-governance, and towards a corporate model of governance by a cadre of professional administrators. Self-governance (and with it, trust) is diminishing, and managerial control increasing, in a production process (the creation and dissemination of knowledge itself) that epitomizes skill intensivity and worker specialization. Perhaps it could be argued that this trend represents the last throes of reactionary forces seeking to reassert managerial control, and that the self-governing academy will eventually reassert itself as specialization continues apace. However, without having criteria with which to differentiate long-term trends from short-term counter-trends, it is difficult to know how to distinguish this possibility from wishful thinking.

An undoubted strength of this volume is its reclamation of the notion of a utopia—a socioeconomic reality that is currently non-existent but thought to be desirable—from the rubble of recent dystopias that have given any sort of idealization a bad name. As Hodgson reminds us, rejecting prescription can result in a debilitating fatalism that decries any debate about socioeconomic outcomes as unwise and undesirable. Properly construed, utopianism can help reassert the principle that society exercises choice over the form that it takes. Moreover, even anti-utopian fatalism is not free of prescriptive content, given that it implicitly asserts the superiority of the *status quo*. So in this way, utopianism can be defended as a simple preference for rendering explicit the prescriptive content of socioeconomic thought. This thinking dovetails with that of Milberg & Heilbroner (1995); a utopia as defined above would constitute part of the pre-analytic vision that these authors celebrate and encourage contemporary economists to spend more time developing and articulating. In sum—and without being trite—society needs its dreamers, although it should be obvious that Hodgson's particular brand of utopianism is far more methodical, structured and nuanced than idle imagining.

It is somewhat disappointing, however, that the latter stages of the book do not discuss policies that might help bring about the author's preferred non-capitalist realities. Given that these realities are preferred but also far from inevitable, the question as to how they might be actively nurtured is left begging. It is also surprising that an institutionalist should place so much emphasis on skill-biased technical change when assessing recent trends in unemployment and income inequality (Chapter 11). Although this theme complements the author's focus on the dynamics of knowledge-intensive economies, one might have expected more discussion of recent changes in institutions and the balance of power in capitalist economies (on which, see Cornwall & Cornwall, 2000; Osterman, 1999; and Palley, 1998). Finally, issues remain with the institutionalist theoretical framework within which Hodgson's analysis is couched. For example, much is made of the ubiquity of tacit knowledge in causing information problems. But it is not clear why (if at all) tacit knowledge is not codifiable in principle, and therefore whether or not it need remain a source of information problems. Meanwhile, the status of the impurity principle—and, in particular, the notion that structural impurities are a necessary condition for the survival of social systems—as either an axiom or else a testable hypothesis (that requires both theoretical development and empirical verification) remains somewhat unclear. It should be noted, however, that Hodgson is explicit about the incompleteness of the OIE as a vehicle for economic theorizing, and it is certainly unfair to expect him to have filled all the gaps in one sweep.

Even these lacunae ultimately serve to illustrate the formidable scope of this book, and its success in engaging the reader on a variety of different levels. Above all, *Economics and Utopia* serves as a timely reminder that, as evolving social constructs, economies are continually reproduced or transformed as a result of discretionary choices and deliberate actions. It is impossible to separate what is, from our individual and collective senses of what should be—hence the importance of continued economic discussion and debate, and the grave danger of a fatalistic political culture of complacency and resignation.

Mark Setterfield
Trinity College

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Ross Zucker, *Democratic Distributive Justice* (Cambridge, Cambridge University Press, 2001), pp. 336, $69.95 hardcover, ISBN 0-521-79033-6.

Today's prevalent debate on the distribution of income and wealth pits a libertarian viewpoint against various versions of egalitarian liberalism. Ross Zucker's book represents an unconventional attempt to shift the theoretical focus further to the left. The welfare state, Zucker argues, merely addresses the problem of *poverty* by guaranteeing a certain subsistence level of income, whilst the strong liberal commitment of current theories of justice makes for growing *inequalities* to pass unscathed. He finds an explanation in the widespread neglect of intersubjective aspects of the person, which would in fact call for a more egalitarian distribution.

His argument consists of three main parts. First, he traces the current theoretical tolerance of inequalities to an omnipresent individualism that permeates the history of liberal theory. Second, and running counter to this tradition, he develops a notion of 'dueness' for contributing to the creation of economic value, as well as a partly communal conception of economic activity; these concepts are used to show that people have a 'redistributory property right' to an equal share of a certain portion of national income. Finally, to facilitate the implementation of justice, these revised property rights should be incorporated in our concept of democracy—hence the title of the book—in the form of substantive rights.

Zucker identifies individualism as the implicit 'logical substructure linking all traditional liberal property theories' (p. 81). His comprehensive survey of liberal thought starts off with Locke's premises of labor as an independent activity and of the separate nature of men's purposes, moving on to Kant's assumption of autonomous persons. These individualistic concepts act as a barrier to any intersubjectively founded entitlements to property. Even Hegel's social theoretical position, one of the first acknowledgements of the formative influence of society on the individual, cannot—according to Zucker's analysis—be cashed out in terms of concrete, economically relevant equalities between persons that would call for some equalization of resources.

Perhaps the most insightful comments in this first part are on John Rawls' theory of justice. Although Zucker acknowledges the role of social theory in Rawls, he claims that individualistic features prevail. In particular, he implicitly accuses Rawls of limiting his attention to the supply side determinants of distribution: 'But market rewards to productive agents depend not only on training and education but also on the demand for what is being produced' (p. 79). This is a legitimate challenge, but does Zucker deliver appropriate remedies?

Two substantive claims of distributive justice based on what Zucker calls 'justice as dueness' as well as the 'ethics of economic community' form the centerpiece of the book. The first picks up on the challenge against Rawls. Zucker here suggests that consumption contributes just as much to economic value as production does; and since this contribution is partly determined by a 'systemic' social influence on the consumer, a certain portion of national income should be equally distributed.

Let us start with the second part of this hypothesis. With respect to production, Zucker acknowledges that the importance of social contributions to individual capacities has already been highlighted. Yet, advocates of such a view, such as Ian Shapiro, have generally denied the practical possibility of delineating the impact of individual versus social factors, and hence lack a tool for analyzing the *extent* to which there are so-called 'indirect entitlements'. Focusing on consumption instead, Zucker transforms the neoclassical postulate of self-determined preferences into a concept of 'socially self-determined needs' (cf., for instance, p. 86). Given that we view consumption as a contribution to economic value, a certain portion of this contribution should be attributed to society. Claiming to address Shapiro's practical worry above, Zucker suggests that due to the infinitude of wants characteristic of all members of a capitalist society—which is similar to the neoclassical postulate of insatiability—we can assume this mutual influence on preferences to be equally strong; this 'systemic' influence justifies a certain distributive equality. However, Zucker himself acknowledges that this crucial assumption of an *equally strong* mutual influence on individual preferences in turn depends on equal income and wealth to start with. This unrealistic premise renders the central result of this section not only highly speculative, but also less interesting.

More fundamentally, Zucker fails to realize that any market, by definition, rewards both sides of the exchange; the producer receives the money and the consumer receives the good she wants. You don't have to be a neoclassicist to feel that any additional reward for the consumer amounts to double-counting the economic value created.

In sum, although most people would agree that social determination should not be understood as a threat to free will but as a constitutive influence on the individual, the conclusions Zucker tries to draw from this statement with respect to consumer preferences are the least convincing of his book. The superficial attempt to predict the macroeconomic consequences of a more egalitarian distributive scheme on growth, which concludes this section, does not help to reverse this impression.

Let us turn to Zucker's second proposal to redraw the lines of distributive justice. 'Total social income,' he argues, 'should be distributed unequally to the extent that it is created by distinct individual actions, and it should be distributed equally to the extent that it is created by the joint activity of people shaped by social conditions' (p. 4). This is the point where Zucker's opening historical analysis of liberal theory has its most direct impact. From Kant onwards, theorists acknowledge that a common activity motivated by a common end justifies an equal distribution of the ensuing benefits. However, their individualistic approach leads them to believe that such a common purpose does not exist in an economic context. In neoclassical economics, which follows this tradition by endorsing Adam Smith's metaphor of an invisible hand coordinating individual interests, 'the individual is not remunerated in her capacity as a cooperative agent, but only as an individual agent' (p. 162). The paradigm here is one of competition rather than community.

Notably, Zucker is eager to delineate his project not only from views held on the right of the political spectrum, but equally from communitarian thought. The latter would both deny the existence of a dimension of community in an economic context and restrict its reach of identification to subgroups of society. In contrast, Zucker explicitly speaks of an *economic* community and he even requires a *system-wide* notion in order to deduce any egalitarian distributive consequences.

He identifies two different types of economic community. First, employing a social theory of capital circulation, which has its origins in Karl Marx's work and has been further developed by David Levine, Zucker proposes to interpret the relationship between consumers and producers as at least partly communal. Both the producers' serving of consumer needs and the consumers' contribution to completing the capital cycle through their expenditures serve a common goal, namely 'preserving and expanding capital' (p. 185) or, put differently, ensuring the functioning of the capitalist system. Importantly, Zucker characterizes this process as self-organizing, in the sense that it does not presuppose the participants' intentional pursuit of the common goal.

The straight neoclassical reply to Zucker would state that the participation of consumers and producers in the capitalist system is contingent on the benefits they stand to gain in the form of consumer and producer surpluses. Yet, since this answer would amount to endorsing precisely the paradigm Zucker is questioning, here are two—hopefully more neutrally phrased—objections to his case.

First, Zucker expands his definition of capital to comprise ‘all elements of the circuit of commodities and money together’ (p. 172). If both money and virtually everything you can buy with it are included in the notion of capital, it becomes almost trivially true that both consumers and producers pursue its expansion.

Second, Zucker’s move away from the classical assumption that community presupposes ‘consciously intending the common interest’ (p. 208) seems problematic. Granted, a common goal may indeed be passively promoted. Yet, do we really want to *reward* people for an unintended consequence of their action? Where do you draw the line between passively serving a common as opposed to another individual’s goal? These conceptual difficulties may lead one to agree with Zucker that there are more types of economic community than usually thought, but that not all of them should serve as the basis for entitlements.

Whereas the first of these considerations weakens Zucker’s case for speaking of an ‘economic community’ between consumers and producers as such, the second concentrates on a criterion we may want a common activity to meet for it to have distributive implications.

Zucker’s suggests a second type of economic community to hold between capital and labor. However, the book remains ambiguous on whether the common goal of these two groups still consists of ‘preserving and expanding capital’ or simply in production. The latter would represent a much more precisely formulated common goal, which could meet objection one above. Besides, the coordination required by a productive process indicates an active pursuit of the common goal; in this case, distributive consequences seem warranted.

Why does Zucker dither between this seemingly more promising route of postulating production as the common goal and sticking to the problematic choice of ‘preserving and expanding capital’? One explanation could be the sub-systemic character of the common goal of production, which is usually restricted to one or, at most, a group of products. Under these conditions, Zucker could not maintain his assumption of a system-wide notion of community, which would undermine his justification for a (partially) egalitarian distribution.

In any case, Zucker does not provide a measure for the extent of community of either type—between consumers and producers or between capital and labor. Without such a measure, his proposal will not be practicable.

At this point, one may ask why the book is entitled ‘*Democratic* Distributive Justice’ (my italics). With the analysis of liberal thought as well as Zucker’s substantive claims about distributive justice taking up almost nine tenths of the book, his views on the relation between justice and democracy receive less attention than they deserve.

Zucker champions a form of so-called substantive democracy over the currently prevailing procedural model. In a distributive context, this would mean identifying a set of economically relevant equalities between people, which subsequently serve as the basis of individual *rights* to a certain equal share of national income: 'The rationale for substantive democracy is that it is necessary for the realization of justice. Though morally supreme, justice has no authority without democracy' (p. 273). The substantive approach proposed by Zucker finds strong support in the fact that a procedural democracy based on majority voting fails to resolve distributive conflicts.

The link to the rest of the book is obvious. Zucker suggests that the concrete formulation of the substantive rights should follow the theory of justice he developed on the basis of the notions of 'justice as dueness' and an 'ethics of economic community'. Challenged by the criticism that assessing the attainment of economic rights may prove very difficult in practice, Zucker rightly points out that this difficulty applies to rights in general.

Democratic Distributive Justice is a very ambitious book. It starts from the innovative and very promising proposition that current theories of justice do not pay sufficient tribute to intersubjective aspects of the person, and hence are more tolerant of inequalities than they should be. The fact that this reviewer does not agree with Zucker's proposed remedies of this situation should not distract from the relevance and significance of the problem itself. And without ventures onto new territory like Ross Zucker's, it is hard to see how progress in political philosophy could be made. For anyone sharing his intuitions about the shortcomings of current theories of justice, his strong historical analysis of liberal thought as well as the systematic contextualization of his ideas in existing literature will prove illuminating.

Peter Dietsch
London School of Economics

Riccardo Bellofiore & Piero Ferri (Eds), *Financial Keynesianism and Market Instability. The economic legacy of Hyman Minsky*, Volume I (Cheltenham, Edward Elgar, 2001), pp. 222, £59.95, ISBN 1-84064-358-7.

Riccardo Bellofiore & Piero Ferri (Eds), *Financial Fragility and Investment in the Capitalist Economy. The economic legacy of Hyman Minsky*, Volume II (Cheltenham, Edward Elgar, 2001), pp. 210, £59.95 hardcover, ISBN 1-84064-359-5.

If there were any justice in the world of economics, Hyman Minsky (1919–96) would have received a Nobel Prize for his financial instability hypothesis, an analysis of great originality and importance, and which is also, I believe, fundamentally correct. Most of the contributors to these two volumes would concur with this judgement, and some would probably go as far as Steve Keen, for whom 'Minsky was the most significant economist of the last forty years—perhaps as significant in our time as Keynes was in his.' Accordingly, Keen hopes 'to see economics undergo a Minskian revolution, as it once underwent a Keynesian one' (Vol. I, p. 106). The December 1998 conference in Bergamo, where the papers in these two volumes originated, may eventually come to be regarded as the start of this revolution.

There are 19 contributions, preceded by a 30-page introduction by the editors, all but three pages of which is common to both volumes; the authors in the books come from Australia, Canada, Germany, Great Britain, Italy and the United States. Apart from the extremely brief (two-page) opener by Richard Day, the chapters can be grouped under four broad headings. (My classification differs somewhat from that adopted by the editors.) The first is the intellectual background to Minsky's economics, discussed by Victoria Chick, Duncan Foley, Anna Maria Variato and Steve Keen (in Volume I) and by Geoff Harcourt (in Volume II). The second is the financial instability hypothesis itself, which occupies most of the editors' introduction and is also the principal theme of the contributions by Ester Fano, Dimitri Papadimitriou & Randall Wray, and Perry Mehrling (Volume I) and by Alessandro Vercelli, and Marc Lavoie & Mario Seccareccia (Volume II). Third, Minsky's analysis is extended to the international economy by Philip Arestis, Malcolm Sawyer, and Jan Kregel (all in Volume I). Finally, formal models in a Minskyian mode are set out by Piero Ferri; Steven Fazzari, Piero Ferri & Edward Greenberg; Richard Arena & Alain Raybaut; Carl Chiarella, Peter Flaschel & Willi Semmler; and Domenico Delli Gatti & Mauro Gallegati (all in Volume II). The Chiarella, Flaschel & Semmler paper, at 52 pages, is by some way the longest of all.

Chick was a student of Minsky's at Berkeley in the 1950s. In 'Cassandra as optimist' she writes admiringly of his ability, like Keynes, to embrace opposites. For Minsky, capitalism was a very risky business, as demonstrated in 1929; but it was also basically benign. From the outset he stressed the importance of uncertainty and expectations in *The General Theory*, and was 'perhaps the earliest of the retrievers of Keynes's *Treatise on Probability*' (Vol. I, p. 36). Although influenced by Henry Simons, Chick suggests, Minsky transcended his Chicago master by endorsing discretionary monetary policy rather than rigid rules. Therein lay the grounds for his optimism. Harcourt met Minsky much later, but is (presumably) the only contributor to owe him his life—Minsky diagnosed Harcourt's diabetes in 1992 and ensured speedy and effective treatment. Harcourt's exposition of the Minskyian vision is consistent with Chick's: since investment decisions have to be taken on the basis of uncertain expected future cash flows, and expectations generally turn out to be wrong, investors' 'animal spirits' are alternatively buoyed and dampened by their experiences. Minsky's is thus 'a story of an endogenous cycle whereby one ultimately unsustainable situation transforms itself into another equally unsustainable situation, and so on, all of which is the result of the intermingling of real and financial factors' (Vol. II, p. 71). Harcourt links Minsky to Keynes and to Hicks, while Keen argues that 'Minsky's true "micro-foundations" lie not in the neoclassical realm, but in a revolutionary interpretation of Marx' (Vol. I, p. 107), shorn of the labour theory of value but emphasising the dialectic between use value and exchange value. Would that Minsky were able to comment on this chapter! Variato asks some interesting questions in her 'Hyman Minsky: what kind of (post-) Keynesian?', but does not really come up with convincing answers. Foley's dissection of 'Hyman Minsky and the dilemmas of contemporary economic method' is more incisive. 'Given Minsky's strong quantitative training and the nature of his early work in economics,' Foley observes, 'his refusal, often remarked upon, to develop a rigorous mathematical model to express his ideas about financial instability is a sharp reminder of the limits of our current methods' (Vol. I, p. 47). As Minsky himself had recognised, each act of investment is 'a unique existential crisis' (Vol. I, p. 50), which renders formal modelling distinctly hazardous. In addition, since we have no reason to suppose that the future will be like the past, statistical inference cannot be relied upon either. For Foley, like Minsky (and Keen?), 'real knowledge' instead 'resides in qualitative, dialectical insights' (Vol. I, p. 59).

Bellofiore & Ferri, Papadimitriou & Wray, and Vercelli all set out Minsky's financial instability hypothesis with its distinction between 'hedge', 'speculative' and 'Ponzi' positions and its prediction of persistent cyclical instability. There is, inevitably, some repetition and duplication here. Fano briefly outlines Minsky's reading of the Great Depression, which, she suggests, was greatly influenced by Joseph Schumpeter. In one of the most interesting papers in the two volumes, Mehrling uses the Long Term Capital Management fiasco to illustrate the differences between the financial instability hypothesis and what he terms 'modern finance', that is to say, the ideas of Robert Merton, Myron Scholes and their associates. Pursuing the principles of modern finance, 'LTCM's theory of security valuation abstracted from liquidity, with the consequence that illiquid securities tended to look underpriced relative to liquid securities' (Vol. I, pp. 153–154). It collapsed when it proved incapable of refinancing its positions, and this, as Mehrling observes, is 'exactly the kind of problem that Minsky's theory places at the centre of discussion' (Vol. I, p. 153). After LTCM the entire focus of monetary macroeconomics will have to change: 'No more Keynesians versus monetarists; the real debate is between Minsky (and the central bankers) on the one side and modern finance on the other' (Vol. I, p. 157). A less favourable assessment of Minsky's analysis is provided by Lavoie & Seccareccia, who present empirical evidence on the cyclical behaviour of corporate indebtedness for Canada and other G–7 countries from 1971 to 1995. They set out the underlying problem very clearly. Minsky predicts a sharp rise in corporate financial obligations in the upswing of the cycle as speculative, and then Ponzi finance comes to predominate over more cautious hedge positions. In order to generate increasing financial fragility, however, these obligations must grow faster than corporate resources. Everything thus depends on the rate of increase in aggregate profits, which (in a closed economy) is determined by corporate investment expenditure and by the size of the budget deficit. It is therefore entirely possible for the financial system to become more robust in a boom, rather than more fragile. In practice, Lavoie & Seccareccia conclude from their empirical research, 'there is no strong evidence in support of the financial fragility hypothesis' (Vol. II, p. 87).

What of those less-developed economies where financial crises actually did occur? Both Arestis and Kregel address themselves to this question. Arestis contrasts Minsky with the neoclassical 'financial liberalizationists', arguing that the financial instability hypothesis provides a far superior explanation of the banking sector crises in Latin America, Africa, Israel, Eastern Europe and Spain (between 1997 and 1996) and in South East Asia (in 1997–98). Like Kregel, he extends Minsky's theory to the foreign sector, since a key element in the crises under discussion was the vulnerability of banks with large dollar-denominated liabilities at a time of rapid currency depreciation. Kregel's analysis of the 'debt-deflation' crisis in South East Asia leads him to the alarming conclusion that IMF pressure for further liberalisation of capital flows 'is precisely the scenario which was the prelude to the global crisis of the 1930s' (Vol. I, p. 211). Since 1998, though, the Asian economies—with the predictable exception of Indonesia—have recovered rapidly, without seriously threatening the global financial system. I would have appreciated a postscript by Kregel explaining how this has been achieved. Sawyer also touches on the Asian crisis, reiterating his support for the Tobin tax, for financial regulation to be enforced by new global institutions, and for an international lender of last resort operating without the deflationary bias that characterises the IMF and the World Bank. His principal interest, however, is the European single currency. Minsky, Sawyer maintains, would have been severely critical both of the deficit-reduction requirements imposed on member countries by the Maastricht Treaty and of the principles governing the behaviour of the European Central Bank, which do not permit it to concern itself with anything other than inflation; employment, growth and the stability of the financial system are thus outside its remit.

In the final set of papers, Ferri's model of 'ceilings, floors, growth and the NAIRU' is more a tribute to Minsky than a formalisation of his ideas. Fazzari, Ferri & Greenberg, however, write a Minskyian investment function (with a distinctly New Keynesian flavour) and then, as if responding to Lavoie & Seccareccia, bring in aggregate demand to endogenise corporate cash flow. Their simulations generate cyclical fluctuations in investment and output. Similar conclusions are reached, analytically, by Arena & Raybaut, while Chiarella, Flaschel & Semmler again resort to simulation to model the macroeconomics of debt deflation. These two papers are fiercely technical, as is the concluding chapter by Delli Gatti & Gallegati, whose model of financial fragility departs from its New Keynesian roots by allowing for the existence of heterogeneous agents, and contains some beautiful graphics.

There is, then, a very great deal of meat in these two volumes. But there are also some weaknesses. Apart from the papers by Arestis, Sawyer and Kregel, there is very little on Minsky's views on economic policy, and even there the discussion is restricted to macroeconomic management and the regulation of financial institutions. However, there was a great deal more to Minsky the policy analyst than this (see especially Minsky, 1986). Only Lavoie & Seccareccia make any attempt at empirical testing of the financial instability hypothesis. Although, as Foley indicates, there may be good Minskyian reasons for the reticence of the others, some qualitative investigation by economic and financial historians could very well have proved enlightening. Minsky's own political views are entirely neglected, and even the treatment of the intellectual background to his economics is somewhat restricted. I know from personal experience that one cannot mention his two-price theory of investment to a neoclassical audience without someone asking about its relation to Tobin's *q*. Yet none of the contributors makes a serious attempt to connect Minsky to

Tobin's monetary economics. There is a similar neglect of the New Keynesians, whose relationship to Minsky is touched on by Variato but not developed at any length; this is especially surprising since at least two of the contributors (Fazzari, Delli Gatti) have clear New Keynesian affinities. Most striking of all is the failure to link Minsky to the other central figure in Post Keynesian monetary theory, Paul Davidson. It is a matter of public record that Minsky was no great fan of Davidson (Minsky, 1974), and there is anecdotal evidence that the two men subsequently agreed not to criticise each other's work in public, apparently to avoid giving comfort to the neoclassical enemy. The practical effects of this pact appear to merit some analysis—not to mention the ethics.

I also have two complaints for the editors (and publisher). It really is unacceptable that two expensive volumes such as these should nowhere contain either a biographical sketch of Minsky or a bibliography of his published work. Newcomers to Minskyian economics will learn little or nothing here of his career, his institutional affiliations, his bank directorships and his connection with the Levy Institute—not even the dates of his birth and death. (However, the relevant information can be found in Mehrling 1998, together with an almost complete list of Minsky's publications.) And why, apart from the obvious commercial reasons, are there *two* volumes, when a single 400-page book, eliminating the duplicated introduction and references, would surely have been technically feasible?

J. E. King
La Trobe University

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Truman F. Bewley, *Why Wages Don't Fall During a Recession* (Cambridge, MA, Harvard University Press, 1999), pp. 524, $57.95 hardcover, ISBN 0-674-95241-3.

One of the most intriguing questions of modern economic theory is why prices often fail to adjust to changes in demand or supply. The failure of wages to adjust to changes in demand or supply has reinforced the belief that unemployment emerges *because* of wage rigidity. A number of theories have been developed to explain this phenomenon, but empirical research has not yet established which of these explanations should be accepted. It is in this light that one should read Truman Bewley's recent book. Using interviews covering a wide range of issues—an empirical method not commonly employed by economists—Bewley provides in-depth insight into how employers and union leaders think about wages.

The book presents the results of more than 300 interviews with corporate leaders, union leaders, unemployment counselors and business consultants during the recession of 1991–93. After an introductory discussion on survey methods and interview techniques, Bewley begins to unfold his results. Seeking an answer to the question of why wages fail to clear the labor market in recessions, he unveils the wage policies of firms and labor unions in a large number of categories, including: productivity and morale, the presence or absence of unions at the workplace, layoffs and recruitment of workers, and the state of unemployment. Each category is explored with two aims: to enlarge our understanding of why wages do not fall in a recession, and to permit a comparison of existing labor market theory with actual market practice. As to the latter purpose, the book's penultimate chapter is devoted to a refutation of a large number of theories, while the final chapter presents Bewley's positive contribution to labor market theory.

The book's theoretical contribution, which is based on the presented empirical evidence, is interesting, but the principal contributions lie in the large volume of evidence obtained through Bewley's application of the interview method. He clearly appreciates the method's strengths and weaknesses, and he gives the reader a good insight into his own experiences from using it. He interprets his empirical results with relentless attention to the scope and limits set by the interview method, even stressing that this method is not a substitute for traditional quantitative methods, but is instead a supplement to our arsenal of empirical methods.

Interview methods still have not been fully integrated into the established arsenal of economic methods. Few of us ever use this method, and few graduate students are trained to use it. This is regrettable, since, as Bewley demonstrates, it allows us to get at the *why* behind economic activities and phenomena. Bewley's application of the interview method aims at a comprehensive establishment of facts for theory development, as in the stylized facts approach. While traditional statistical methods can detect *patterns* of economic activities, they are limited in their capacity to reveal the *motives* behind them. Interview methods can supply knowledge that traditional quantitative methods miss. At the same time, interview methods are faced with the problem of interpretation of answers: the same question (e.g. 'Why does your firm lay off employees and not cut wages in a sales slump?') can educe many different kinds of answers. In order to draw useful inferences the investigator has to impose a generalizing pattern upon these answers; in the process some details are inevitably obscured. Bewley repeatedly reminds his reader of this problem. He presents answers to questions both by quoting interviewees—displaying heterogeneity in word-by-word answers—and by generalizing evidence into categories.

A central concept of the book is, for obvious reasons, the definition of wage rigidity. In the end, Bewley adopts the following two definitions, which work well with the evidence:

> *Real wages* are downwardly rigid if employers feel obliged to increase pay by at least the rate of inflation in the cost of living. *Nominal wages* are rigid if there is resistance to cutting nominal pay but not to increasing pay by less than the rate of inflation in the cost of living. (p. 208; emphasis added)

An unavoidable problem with this definition of real wage rigidity is that the denominator is perceived differently by employers and employees. The definition implies that what matters is the living standard of workers, not the employers' real cost of labor. But Bewley's interview respondents were employers, who might be expected to focus on the latter rather than the former. However, Bewley's evidence shows that employers are concerned with the real wages of their employees, and that they fully understand their workers' negative reaction to proposals to cut real wages.

The implications of Bewley's evidence for theory are discussed in the final two chapters. Bewley confronts theory with his survey results to evaluate the validity of a large number of theories. In almost every case, the empirical evidence refutes existing theories. Two general patterns emerge that have important implications for theory: unemployment does not correlate with wage rigidity because workers voluntarily leave a job when confronted with lower money or real wage offers; and money wage rigidity is *desired* by both workers and employers.

The second of these results suggests that workers want predictable remuneration and, in return, they are willing to put in extra effort when employers ask for it. Evidence indicates that firms can enjoy higher labor productivity under a rigid money wage regime than when wages are flexible.

Aside from the major theoretical implications of Bewley's evidence, there are some smaller-scale, yet characteristic and illustrative, effects of his findings. One example of why wage cuts are not attractive alternatives to layoffs is that demand for labor is price insensitive in the short run, partly because labor costs are a small part of what firms charge their buyers. Bewley adds that product demand also is short-term price insensitive, so that even if wage cuts were chosen as a strategy to enable price cuts and thereby boost sales, the product price cuts would have little impact on sales.

It is sometimes proposed that workers insist upon wages that are too high relative to their skills, and therefore price themselves out of jobs. Bewley refutes this proposition:

> The rejection of overqualified workers stood out during the interviews, because it came up often and contradicted sharply the idea that excessive pay demands cause increases in unemployment during recessions. The label 'overqualified' usually applied to unemployed job applicants who had made considerably more money [roughly 30% or more] on their previous job than they would on the new one. (p. 281)

One interesting result is that the case for downward adjustment of wages is stronger in sectors with shorter employment contracts; as new hires come in it is easier to adjust entry wages downward. But even here there is a limit to the flexibility of wages: in many workplaces, such as supermarkets or restaurants, there is a core of old-time employees who know the wage structure and who will regard employer efforts to bid down wages of new employees too much as a threat to their own position. It is perhaps more accurate to say that wherever wages are flexible their flexibility is limited to a bracket.

The evidence brought forward in Bewley's book unintentionally portrays existing labor market theory as primitive and detached from real-world labor markets. This field of theory has emerged largely to explain why unemployment persists when core theory says it should decline as the economy drifts toward (a new) general equilibrium. Here are some examples of how Bewley's observations call standard economic theory into question.

Real business cycle theory. The idea that workers select the optimal amount of work and leisure in response to the real wage does not survive the small changes observed in the real world, unless one adds an assumption of work–leisure indifference. But the latter assumption is also inconsistent with reality

> since a common complaint of advisers of the unemployed was that their clients were desperate for work and miserable being jobless, and it is hard to believe that many employed workers would have failed to anticipate the difficult situation they would find themselves in after losing their jobs. (p. 400)

Voluntary exit theories. In these theories, real wages are kept rigid by workers' bargaining power and voluntary exits from the labor market. A weakness of this group of theories is that the low level of unionization in the United States contradicts the premise that collective bargaining is pervasive. Another weak spot is that employers are themselves reluctant to cut pay because of fear of damaging employee morale; Bewley observes that corporate management was almost always 'the first in line of resistance' to pay cuts.

The Lindbeck–Snower Insider–Outsider model contribution belongs to this class of theories. It suggests that outsiders (unorganized unemployed) bargain for their wages individually with employers, and that insiders (unionized employees) damage productivity by refusing to cooperate with replacement workers. So, the theory posits, employers keep money wages unchanged during recessions in order to preserve employee morale and workplace efficiency. Thus, sticky wages cause unemployment. There is no evidence whatsoever to support this argument: 'The main problem with the [Insider-Outsider] theories is that they assume conflict where there is none. There is usually no conflict between insiders and outsiders over pay cutting, because pay reduction is not thought of as saving jobs' (p. 404).

Market malfunction theories. The *market misperception model* claims that workers misinterpret their relative wage and therefore quit their jobs when they see wages fall below a reservation wage. The misperception approach implicitly claims that workers are indifferent between temporary and permanent jobs, but Bewley's evidence shows that the only time workers can accept a wage fall is when it will *help them preserve a permanent job*.

Firm behavior as a cause of wage rigidity. This is an *implicit contract approach* in which informal wage agreements between employers and employees prevent money wage flexibility. Employers implicitly agree to insure workers against a decline in incomes, by offering them a stable income instead of a possibly higher but also fluctuating income. Bewley finds the theory attractive, but finds that 'managers do not think in a way consistent with [the approach]' (p. 411).

Efficiency wage theory evoked interest among interviewed managers, who judged it to be harsh and incompatible with good management. One element of the theory was considered particularly repelling: performance bonds were dismissed either because they made the workplace 'like a prison', because it was illegal or generally improper, or because it created an employment guarantee that would, in effect, be a nightmare to firms.

On the basis of his results Bewley develops a morale-based theory of wage rigidity. He starts from observations of morale and wage rigidity, captured in an illustrative comment by an interviewee: 'A pay cut is like a criticism or an insult. Pay is so closely associated with self-worth that a cut is taken personally' (p. 175). Managers also expressed great concern about the impact of pay cuts on the standard of living of their employees. When employees had to adjust to a tighter cash flow they would lose focus on work and become antagonized because of the stress caused by the pay cut. Staff reduction is less offensive a method for a company to meet lost revenues than pay cuts—and it allows employers to exercise some control over who leaves and who stays.

Bewley suggests that predictable remuneration improves worker effort by boosting morale and therefore gives employers more output per work hour. His theory deserves detailed presentation, partly because it accords with recent contributions in price theory, according to which production and employment are *positively* affected by sticky wages in the same fashion as consumption benefits from sticky prices. Unlike mainstream price theory, where prices are flexible until flexibility is impeded, contributions by Okun (1982), Hicks (1989) and Larson (2000) claim that prices are rigid because there is demand for this rigidity on both sides of markets. Bewley's results speak a similar language: just as consumption and production can suffer from flexible prices, labor productivity is hurt by flexible wages as worker morale and loyalty are eroded.

Every economist, orthodox or heterodox, interested in the functioning of labor markets should read this book. It would make a useful supplemental text to a graduate course in macroeconomics: after a presentation of how mainstream macroeconomic theory and labor market analysis explain persistent unemployment, Bewley's evidence might serve as a powerful stimulus to students to explore alternative approaches.

The book can also be read as part of an education in empirical research methods. Its careful use of the interview method is, in itself, a worthy contribution to economics. This method, as we have noted, is not as widespread as traditional quantitative methods; but Bewley shows that it ought to be more widely used.

Sven R. Larson
Roskilde University

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Ralph E. Gomory & William J. Baumol, *Global Trade and Conflicting National Interests*, (Cambridge, MA, MIT Press, 2000), pp. 199, $29.95 hardcover, ISBN 0-262-07209-2.

The purpose of this book is to demonstrate that (1) under certain reasonable assumptions, free international trade will produce only one of many possible stable equilibrium outcomes; (2) that the equilibrium produced may not be the most efficient possible; and (3) that, even with free trade, what benefits one trading partner may not, and in certain circumstances will not, benefit the other trading partner. The explicitly stated implication is that government interference in trade is warranted. In an excellent penultimate chapter, the authors acknowledge previous work, dating back to Alexander Hamilton, in which similar analyses progressed toward their own, and they specify how their book has significantly, if marginally, added to those analyses by making the case in a different and more general way.

The argument of the book is tight and unremitting. It cannot easily be summarized. Nonetheless, the pages read quite easily—at times, deceptively easily. Frequent references to rigorous mathematical elaboration of the argument imply that that is possible and has, in fact, been carried out; but there is not a single equation in the text or the notes. The book publishes the Robbins Lectures that Baumol, working from ideas that he and Gomory developed together, delivered at the London School of Economics in 1994, and is constrained by the limitations of that forum. The whole presentation is, as the saying has it, 'intuitive'. Happily, the argument is made twice: first in a less sophisticated form for the non-specialist, and then, in the last five eighths of the book, in a form suited to the specialist, who is presumed to be more familiar with technical terms and more willing to exercise an ability to work through complex, lengthy rationalizations.

The two distinctively operative building blocks in the argument are the assertions that, in the presence of economies of scale and technological progress, there are market failures, and that the advance of one trading partner will, under some not unusual circumstances, damage another. That is, (1) the advantage of size or first-in marketing of a product prevents market entry of would-be more productive (efficient) processes; and (2) as a more rapidly advancing country produces a larger share of total world output, the shares of its less rapidly advancing trading partners fall, offsetting for the less rapidly advancing countries any new advantage from specialization as the more rapidly advancing country advances. These arguments, the authors acknowledge, are not new. The contributions of the book are its novel and more general statement of the case, an engaging diagrammatic presentation that was new to this reviewer, and a very readable presentation.

There are, of course, some internal limitations of the presentation.

This is a book in the established tradition of international trade theory. It sets out conditions in which the conclusions drawn are perfectly logical. These conditions do approximate what I consider to be 'real world' conditions, and I do not question the conclusions that it frequently benefits a more advanced country to keep its less advanced trading partner from advancing, and that a world of unrestrained market forces is not necessarily the best of all possible worlds. The policy conclusions that Gomory & Baumol draw from this, however, are not at all novel, and are given only a passing mention. The government failures that might be involved in any attempted rectification (if some principle of justice were asserted that would warrant the use of the term 'rectification') are similarly given only a glance.

Although the term 'global' appears in the title of the book, there is no historical and descriptive analysis of past and current surges of globalization. This book is not one of the flood of books on globalization that have recently appeared to explain the current state of affairs in individual nations and in the world as a whole. Certainly some of those currently protesting globalization have in mind the possibility of economic suppression of less developed countries, and they are not often in favour of what some call 'free trade'; but much of the related literature is institutional. That is, it is concerned that political and social rights gained over the past century could be lost in the reformation of the nation state to fit emerging, allegedly undemocratic, transnational institutions.

Perhaps more disturbing to the historian is the authors' failure to define clearly what is meant by the 'size' of a country. Is it GDP? Is it population? Is it GDP per capita? This matter is directly related to what is meant by the degree to which a country is 'advanced'. One would think that in a closely-knit argument of the sort offered in this book a clear definition of this matter would be essential; but it is missing. I also have trouble with the definition of economies of scale, because it seems to me that the authors shift from decreasing marginal returns, to decreasing or increasing returns to scale, to increasing and decreasing cost industries, and lastly to the effects on unit costs of trajectories in technological progress, whenever such shifting is convenient for the argument. This is not to say that Gomory & Baumol are unaware of the importance of the distinctions involved. Nor have they neglected to lay out the assumptions upon which a model logically can be constructed to yield the conclusions that are in fact drawn. What I *am* suggesting, however, is that if all of this was laid out in rigorous detail it might become evident that the whole argument is contrived—contrived, I repeat, to reach conclusions with which I generally agree on more historical grounds.

There is a further point of puzzlement that is perhaps personal to the reviewer. Despite a chapter on predecessors, an excellent list of references, many useful notes, and a good index, I can find no reference to John Rae, whose *New Principles of Political Economy* (published in 1834) is the classic in the field of literature to which Gomory & Baumol contribute. I am not puzzled that they are unaware of Rae's contribution, because I do not believe that they are; I am puzzled by their not referring to it.

Despite these criticisms, I find the book to be a useful contribution to the literature and a good read. I recommend it to anyone interested in the theory of international trade.

Robin Neill
University of Prince Edward Island and *Carleton University*

Neil De Marchi & Craufurd Goodwin (Eds), *Economic Engagement with Art* (Durham NC, Duke University Press, 1999), pp. 506, $22.95 paperback, ISBN 0-8223-2489-X.

For those looking for a book that proves interdisciplinary studies can be challenging, exciting and satisfying, this collection of 17 essays on what economists have said about the arts and what artists have said about political economics is a welcome effort. But for those involved in the field of cultural economics, this book is a reminder of the need actively to promote, even shamelessly advertise, your own academic field lest your findings are ignored by other academics.

Recently, I participated in a well-attended workshop on interdisciplinary teaching. The cheerful presenter started her talk on ways to add *interdisciplinarity* to the undergraduate classroom by showing us her favorite coffee mug. She described it, making sure she read all the labels inscribed on the mug, 'made in Korea', 'microwavable', and so on. She then asked us to design an interdisciplinary undergraduate course based on her mug. To my surprise, a lively discussion ensued in which a good number of new interdisciplinary undergraduate courses were drafted. One entitled 'Connections between International Trade and Caffeine Addiction 101' was without a doubt my favorite. Many college professors are understandably dissatisfied and frustrated with the boundaries their fields impose on the way they explain their surroundings to students and are looking for more holistic approaches to teaching. However, there are not many rigorous academic articles or challenging textbooks that discuss relevant issues under an interdisciplinary lens. Frustrated, professors either attend interdisciplinary workshops or spend long hours trying to organize a semester's worth of interdisciplinary issues. Some simply give up and go back to their old constraining ways of teaching.

The publication of *Economic Engagements with Art* gives these college professors an intriguing option. By implicitly posing the question 'What do we know about the production, consumption and distribution of the arts?', this collection of essays provides an intellectual history of the political, economic, sociological, aesthetic and psychological aspects of the arts, as analyzed by some of the most influential thinkers from the Scholastics to the New Classicals. This book, consisting of six essays on 'arts and economic theory', five essays on 'art and economic policy', and six essays on 'the business of art', proves what we have always known: scholars interested in studying the arts will be forced to break the boundaries imposed by their fields and set their sights on a wider horizon.

For instance, William Barber's essay 'International Commerce in the Fine Arts and American Political Economy, 1789–1913' is an engaging exploration of the political, economic and aesthetic aspects of the imposition of tariffs on works of art. Picture it: instead of using a coffee mug, the interdisciplinary professor could discuss the history of tariffs in the United States, analyze the costs and benefits of these policies, and explore the possibility that periods of lower tariffs on paintings and sculptures may not only have provided a jolt to the art world in the United States but may also have helped enhance its quality.

For those interested in the sociology and history of the theory of price, Toon Van Houdt's 'The economics of art in early modern times: some humanist and scholastic approaches' and Michael White's 'Obscure objects of desire? Nineteenth-century British economists and the price(s) of rare art' examine changes in economic theory from the Middle Ages to the 19th century by analyzing the values and attitudes of the economists who attempted to explain the seemingly irrational prices of 'luxury goods' and 'rare art'. Even those essays in the collection with a strong economic streak make a concerted effort to provide an interdisciplinary framework to their analysis. For instance, Balisciano & Medema's 'Positive science, normative man: Lionel Robbins and the political economy of art' offers a powerful picture of a conflicted Lionel Robbins. It would be enlightening for any undergraduate to learn that Robbins, a fierce proponent of free trade and opposed to state and state-sponsored monopolies, also supported whole-heartedly the imposition of tariffs on artistic goods in the name of defending national culture and the need for high excellence in culture.

So, did Robbins's 'heart occasionally override his head' when he wrote on the economics of the arts (p. 282)? Balisciano & Medema astutely argue that we should not confuse Robbins's 'economics of the arts' with his 'political economy of the arts'—we should not confuse the positive with the normative man. The argument is convincing; but the reader is nevertheless left with the bleak feeling that economics has very little to say about the arts. This is not the only essay in the collection that leaves us with that feeling. The very introduction of the book starts with an ominous statement: 'there is a gap between what the theory of price can explain and the sense that value is created by art' (p. 11). Later: 'art is acknowledged to possess and convey a civilizing value. On the other hand, no place for it has been found in economic analysis' (p. 28). Actually, even a casual reading of David Throsby's 'The production and consumption of the arts: a view of cultural economics' (1994) or Mark Blaug's 'Where are we now in cultural economics' (2001) would reveal the significant contribution economists have made to the understanding of taste for the arts, the market structure for artistic resources, the impact of technology on the market for artistic goods, and even the positive and normative aspects of public provision of the arts—an area of inquiry that owes a great deal to the conflicted Lionel Robbins. In fact, the growth of the field of cultural economics since the publication in 1966 of Baumol & Bowen's *Performing Arts: the Economic Dilemma* has been so significant that it has allowed, as Throsby puts it, economists to stop apologizing for presuming that economics might have anything useful to say about art.

What could have prevented the authors of this collection from including this vast literature in their essays? I do not know the answer, but I have an idea. A careful look at the articles published in the *Journal of Cultural Policy*, *Journal of Cultural Studies* and, especially, the *Journal of Cultural Economics* would leave us with the impression that before Bowen & Baumol's empirical study on the performing arts, no serious discussion had taken place about the interaction among the arts and economics. The overwhelming dominance of Bowen & Baumol's approach has sent a myriad of cultural economists in search of secondary 'cultural' data that can be analyzed using the latest econometric techniques. Moreover, Van Houdt is correct when he points out that 'it is striking how little effort has been made to date to examine the works of later humanists and the theologians in order to present … what they have to say about the economic aspects of the world of art.' This emphasis on searching for the next 'best data set' and relegating the intellectual history of this field to a footnote has severely hindered the development and adoption of interdisciplinary methods by cultural economists.

One last comment: the reader must be warned that even though each essay in the book is worth reading, the collection as a whole lacks cohesion. Annable Wharton's insightful essay on 'Economy, architecture, and politics: colonialist and Cold War hotels' explores the political, aesthetic and economic dimensions of the expansion of the Hilton hotel chain during the Cold War. While this work follows the tradition of great studies on the role of public space in economic development, and is a valuable contribution to the field of spatial studies, it is difficult to see how it fits with the other essays on the price of paintings and government support for the visual arts. Guido Guerzoni's essay on the intriguing question of why so many works of art were produced in Italy during the Renaissance digresses into an exploration of the origins and development of the terms *liberalitas* and *magnificentia*, and the answer to his original question gets pushed to the last section of the long essay.

In an effort to remedy this lack of cohesion, one of the editors of the book tries to link all the articles together by assessing what the contributors to this collection failed to accomplish: '[they] did not … construct a framework for understanding taste.' Instead, 'what most of [the authors] do is reinforce by example the impression that economists have long found art to be troublesome' (p. 11). Perhaps a more productive way of tying these articles together would have been to ask all the contributors to follow a consistent methodology, to place these issues in a larger intellectual framework, or to compare the methodology and findings of the early economists of the arts with the modern ones.

Javier Stanziola
Florida Gulf Coast University

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Massimo De Angelis, *Keynesianism, Social Conflict and Political Economy* (Basingstoke, Macmillan, 2000), pp. 240, $65.00 hardcover, ISBN 0-312-23146-6.

This book sets out to inject the conception of social conflict into economic theory. It builds upon an Italian tradition running from Antonio Gramsci's (1971) famous essay on Fordism to Antonio Negri's (1968) analysis of Keynesianism. The heart of this book begins with an insightful survey of the mood in postwar England, including Keynes's response to the situation.

Conflict was very much on the mind of John Maynard Keynes and his contemporaries in postwar England. Following close on the heels of the Bolshevik Revolution, with unemployment in double digits and her competitive position deteriorating, England's prospects at the time were not particularly promising.

In the early 1920s, the policy question that preoccupied Keynes was the problem of getting prices right. In his view, wages were too rigid. The price of gold was too high. Both inflation and deflation threatened to topple the existing social relations. Markets, for Keynes, might have solved these problems in the past, but in the 1920s he believed that 'trade unions are strong enough to interfere with the free play of the forces of supply and demand, and public opinion … supports the trade unions' (Keynes, 1925, p. 305). De Angelis shows that Keynes often repeated this sentiment, lending some support to those who have regarded sticky wages as a central element of later Keynesian theory.

For De Angelis, the Keynesian Revolution was a matter of changing the focus from the individual agent to the economic system as a whole. In Keynesian theory, wages were no longer determined by individual bargains or even by agreements between labor and capital within a single factory. Instead, the resolve of the working class as a whole was instrumental in resisting the downward pressure of wages. The problem of sticky wages was not universal. Keynes suggested that 'in a highly authoritarian society, where sudden, substantial, all-round changes could be decreed … a flexible wage policy could function with success. One can imagine it in operation in Italy, Germany or Russia, but not in France, the United States or Great Britain' (Keynes 1936, p. 269).

De Angelis reminds his readers that conflict is also at work in Keynes's well-known observation about how falling wages can undermine effective demand. For Keynes:

> The most unfavourable contingency is that in which money-wages are slowly sagging downwards and each reduction in wages serves to diminish confidence in the prospective maintenance of wages. When we enter on a period of weakening effective demand, a sudden large reduction of money-wages to a level so low that no one believes in its indefinite continuance would be the event most favourable to a strengthening of effective demand. (Keynes, 1936, p. 265)

A few pages later, Keynes acknowledged that a wage reduction need not diminish employment if it brings wages down to a definitive bottom. At that point, investors will no longer fear a further erosion of effective demand. Consequently, a reduction of wages can stimulate investment.

The next two chapters shift to the struggle between labor and capital in the United States in the early 20th century and then during the New Deal and the Second World War periods respectively. The first of these chapters emphasizes conditions in the Ford Motor Company.

Next, De Angelis describes how the success of wartime planning led economics to evolve from a cacophony of theories to a Keynesian orthodoxy during the Second World War. He relates the continuation of wartime Keynesian management techniques into the postwar period to the fear of social unrest during the downturn that demobilization was expected to unleash.

The downturn never occurred. During the Golden Age, large unions and large corporations reached labor agreements with national ramifications. This phenomenon helped to reinforce the collective nature of both labor and capital. At first, conflict was muted.

Interestingly, in his survey of postwar economics De Angelis does not reject IS–LM economics out of hand. Instead, he regards it as accurately reflecting the prevailing perception of stable relations between the classes at the time. In such an environment, Keynesian management schemes could guide the economy to an equilibrium without disrupting the balance of social relations.

Two later chapters deconstruct the multiplier and the Phillips curve to reveal how conflict lurks within. With regard to the multiplier, De Angelis modifies the conventional derivation. Instead of using the marginal propensity to consume as the driving variable in his multiplier, he focuses on a variable distribution of income between labor and capital. This derivation allows him to demonstrate how the traditional multiplier assumes stable class relations. I intend to incorporate this analysis into my next macroeconomic principles class.

De Angelis's Phillips curve analysis is more explicitly concerned with political forces; he shows how an upsurge in class conflict allowed this seemingly stable construct to disintegrate. According to Robert Leeson (1997), this aspect of the Phillips curve was present from the moment that Solow and Samuelson first introduced it into policy debates in the United States in 1959 in an attempt to respond to critics of Keynesian policy who were concerned about the looming problem of inflation.

De Angelis concludes by raising questions about the current disintegration of the neo-liberal arrangement that followed Keynesian demand management. The neo-liberal market society, he argues, is currently at an impasse. Rather than offer some programmatic recommendation, he turns to Keynes's famous essay, 'Economic possibilities for our grandchildren' (1930), which called for more a humane social arrangement than a Darwinian market system.

Keynes, however, also pointed in another direction. Recall that he believed an authoritarian government could overcome some of the contradictions that markets face, such as the damage that declining wages exert upon effective demand. Remember also that he suggested that 'the theory of output as a whole, which [he outlined in *The General Theory*], is much more easily adapted to the conditions of a totalitarian state, than is the theory of the production and distribution of a given output produced under conditions of free competition and a large measure of laissez-faire' (Keynes 1936, p. xxvi; see also Schefold, 1980).

Michael Perelman
California State University, Chico

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Karen Dawisha & Bruce Parrot (Eds), *Conflict, Cleavage, and Change in Central Asia and the Caucasus* (Cambridge & New York, Cambridge University Press, 1997), pp. 423, $74.95, ISBN: 0-521-59246-1.

The international environment during the Cold War was more perilous, but less challenging from an intellectual perspective, than the one United States policy-makers currently face. Thus, during the cold war, the single idea of containing communism drove US policy. However, despite the complex nature of the post-Cold War landscape, the United States again has chosen to organize its foreign policy (or, more precisely, its foreign policy *rhetoric*) around a simple unifying principle—the promotion of democracy.

The West's commitment to democracy promotion, led by Washington, in this 'third wave' of global democratization has generated a surge of literature on democratic transitions, examining concepts such as 'civil society', 'political culture' and 'democracy consolidation'. One of four books convened for the Project on Democratization and Political Participation in Post-Communist Societies, *Conflict, Cleavage, and Change in Central Asia and the Caucasus* contributes to this growing body of research. Edited by Karen Dawisha and Bruce Parrot, leading analysts of democratic transformations in the former-communist states, this volume brings together an impressive collection of well-researched essays on the plight of democracy in the newly independent former-Soviet states of Armenia, Azerbaijan, Georgia, Kazakhstan, Kyrgystan, Tajikistan, Turkmenistan and Uzbekistan. While aimed at providing an analytical framework in which to examine the factors that strengthen as well as undermine democratic progress, the book highlights two important questions for US policymakers: first, how significantly can US policy affect the evolution of democracy in these countries; and, second, how relevant to US interests is the endeavor?

These questions take on greater significance in light of the region's vast and relatively untapped hydrocarbon reserves. By some estimates, the oil reserves of the Caspian Basin are the second largest in the world, surpassed only by those of the Middle East. Any region outside of the volatile Middle East holding substantial oil reserves is of obvious geopolitical interest to the world's largest consumer of hydrocarbons. Of course oil is not uniquely an American passion. Since the collapse of the Soviet empire, the Caspian region has received enormous attention from Western governments and corporations, as well as regional powers such as Turkey, Iran and even China. And Moscow's obsession with the region has only intensified amid the 'independence' of the Caspian states.

With this geopolitical rivalry as a backdrop, several recurring themes animate the collection. First of all, each of the contributors, with the exception of the editors, questions the utility of searching for 'political participation', 'civil society', and the like in a region where these concepts are literally entirely foreign. In one of the best chapters in the volume, Nora Dudwick goes so far as to question whether it is useful to search for democracy at all in Armenia. Since Armenia is one of the few states in the study with a clearly defined national identity and previous experience with statehood, and would be rated relatively high on any democracy scale for the region, this observation does not bode well for the promoters of democracy. Even Kyrgyzstan, which was initially praised as an 'oasis of democracy' in the former-Soviet south is regressing away from, rather than progressing towards, democracy.

Another major theme running throughout the essays is the notion that personalities prevail over institutions in the politics of these republics. In Kazakhstan, for example, President Nursultan Nazarbaev's international prestige and popularity in Moscow have helped solidify his popular mandate at home. Perhaps the best example of this cult of personality is in Turkmenistan, where the President, Saparmurad Niyazov, is engaged in 'mythologizing about the melding of "Turkmenbashy" and his people.' Although the cult of personality has allowed these states to avert initial predictions of immense internal instability, the stability that exists is a precarious one. As Michael Ochs points out, there is an inherent instability to any regime in which power and authority are vested in one individual, as opposed to established institutions.

Moreover, the enforced stability that prevails in this region has been at the expense of democracy. In an analogy with Maslow's hierarchy for nation states, domestic peace and security in this region have priority over such far-away notions as 'self-rule', for rulers and subjects alike. Perhaps the most significant difference between post-communist democratic transformations in the republics of Central Asia and the Caspian and their Eastern European counterparts, is the Hobbesian character of the former. Ruling elites in Central Asia contend, perhaps justifiably, that the risks to the population associated with an immediate and complete transformation to democracy simply are unacceptable. The wealth of polling data in this volume confirms the view of Central Asia's rulers that the people of this region are more concerned with stability than democracy.

Another important difference between the Central Asian/Caspian republics and other post-communist states, is Russia's reluctance to let these states go. Thus, as the new republics endeavor to build independent democratic states they are also engaged in a struggle against Russia's bid for hegemony in the region. It is in this context that stability takes on added meaning for Central Asians. Any examination of Russian policy in the region illustrates clearly that democracy promotion is not a high priority for Russia. In fact, there is some indication that Russia perceives democracy in these states as contrary to her interests. This helps explain why Russia has been so active in the region—not helping to build institutions, but supporting coups, fanning smoldering ethnic tensions, and arming potential rivals.

Of the many uncertainties in this region there seems to be one given: democracy will not consolidate in Russia's 'near abroad' until the former-Soviet republics of Central Asia and the Transcaucasus are free from Russia's grasp. Since true sovereignty is a prerequisite to long-term democratic development in these states, and since this seems to be one of the few areas in which US policy can actually affect democratic development, US leaders can take solace in the fact that they have formulated a foreign policy in which democratic ideals and American interests converge.

Shawn E. Cantley
Brandeis School of Law, University of Louisville

Ray Marshall (Ed.), *Back to Shared Prosperity: the growing inequality of wealth and income in America* (Armonk, NY, M. E. Sharpe, 2000), pp. 423, $29.95 paperback, ISBN 0-7656-0425-6.

The book under review consists of 38 timely essays that were written for the Restoring Broadly Shared Prosperity Project initiated by David Hamburg, past president of the Carnegie Corporation. The essays focus on two primary questions: To what extent are major social and political problems in the United States caused by basic income and unemployment trends? And, is it possible once again for the American people to experience the shared prosperity that dominated the United States before the early 1970s?

To explore these questions, the volume is broken down into three main sections. The first section looks at shared prosperity in the United States by examining wealth and income distribution, crime and mental health rates, economic growth and productivity, and economic opportunity for different social and economic groups in the 1980s and 1990s. The second section looks at the major forces that have influenced the economic and social structures in the United States since the early 1970s. The third, and longest, section of the book covers public policies that the authors believe need to be addressed to 'restore shared prosperity.' The topics discussed in this section are wide and varied, and include national and international economic policy, health care, education, labor market policies, and corporate governance.

Although recent Census Bureau data show income gains in the 1990s for most working Americans, particularly among non-white households, the essays in the first section of the book show that these gains have not been able to offset the continued decline of shared prosperity that the United States has experienced since the early 1970s. The first part of this volume provides strong empirical evidence that the majority of American households have experienced lower or stagnant real wage rates and diminishing economic opportunity and mobility over the last two decades. However, the nation's wealthiest households did not share in this economic stagnation; quite the opposite, in fact.

Edward N. Wolff's excellent essay on household wealth distribution describes how the1980s and 1990s witnessed an amazing concentration of wealth in the hands of the very rich. This wealth gap has been accompanied by State and Federal cutbacks in programs that have provided a safety net for those in need during economic downturns. With less support available from these programs, individuals must rely more on their wealth accumulation to cushion themselves and their families when economic bad times come. Even though the average American household is working more hours, Wolff notes, the share of wealth held by the bottom 80% of households in the US has decreased over the last 20 years, while the wealth of the top 20%, particularly the top 1%, has increased significantly. Besides sharpening the economic divide between different economic groups in the United States, this increase in inequality has also exacerbated the level of economic insecurity of the middle class.

The second section of the book looks at some of the major social and economic forces that have affected shared prosperity in America since the 1970s. The section focuses on three major forces: technology, globalization and demographics. The story presented here runs as follows: the United States experienced a Golden Era of economic prosperity from the end of the Second World War to the early 1970s. This was created by a 'mass production' economy that relied on natural resources, high fixed capital costs and a large working force that needed only basic literacy and math skills to be successful. However, by the end of the 1960s, there were signs that the mass production economy was weakening and being replaced by a more competitive global economy that rewarded 'knowledge-intensive work'. With the basic structure of the economy changing, chiefly due to globalization and technological progress, the rules of the game for economic prosperity were also changing. Education and human capital became more important, and command over natural resources and fixed capital less important, to economic success at both the corporate and individual levels. Demographics have also played an important role in the structural changes that occurred in the United States since the 1970s. The US is experiencing a growing older population that will require more resources for health care, social security and pension plans. In addition, with non-white individuals expected to become the majority population by the year 2050, public and private investment will be needed to meet the educational and training needs of these individuals, and efforts to guarantee equal opportunity for these groups will become increasingly important. Such major economic shifts will require new thinking about public policies, if we are interested in restoring shared prosperity to the United States.

This is the focus of the third section of the book. What becomes clear from these essays is that market forces by themselves will not be able to restore the shared prosperity America experienced in the past. The new economy will need a strong working relationship between the private–public sectors, akin to the one that prevailed in the Golden Era. The authors in this section of the book agree that the United States has a choice in this new economy: it can provide high-value-added jobs or low-wage jobs. Unfortunately, as these essays show, many of the governmental, managerial and financial arrangements that exist in the United States today support the old hierarchical and authoritarian mass production economy, which has a bias towards low-wage jobs instead of high-value-added jobs. For example, high-wage jobs in the new global economy require highly skilled labor and a business environment that values quality and flexibility and is willing to make long-term investments in its employees. But as Appelbaum & Berg point out in their contribution to this collection, financial markets are forcing companies to focus on short-term gains such as cost-cutting as a way of increasing shareholder value, often at the expense of investment in workers, to increase long-term productivity.

All the essays in this section question such arrangements and provide insightful and provocative ideas for achieving shared prosperity in an ever-changing global economy. The authors come up with new proposals for retirement and healthcare for the aging population; they address the need for new standards in our schools to meet the educational challenges facing our young people. They recognize that work rules need to be changed because the traditional top-down management approach will no longer work in the new economy; and they point out that the macroeconomic assumptions under which the Federal Reserve and Congress have been operating need to be re-examined. Overall, these essays provide us with a helpful blueprint to follow as we try to achieve the shared prosperity among all economic and social groups that has been missing in the United States for far too long.

Richard P. F. Holt
Southern Oregon University