



Review: The Unfashionable Elegance of Tsuru's Unorthodox Economics: An Essay on "Institutional Economics Revisited" and "Japan's Capitalism"

Author(s): K. (Vela) Velupillai

Reviewed work(s):

Institutional Economics Revisited (The Raffaele Mattioli Lectures, 20-22, May 1985) by S. Tsuru

Japan's Capitalism by S. Tsuru

Source: *Journal of Economic Issues*, Vol. 29, No. 4 (Dec., 1995), pp. 1213-1230

Published by: Association for Evolutionary Economics

Stable URL: <http://www.jstor.org/stable/4227032>

Accessed: 14/05/2010 02:57

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=ae>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



Association for Evolutionary Economics is collaborating with JSTOR to digitize, preserve and extend access to *Journal of Economic Issues*.

<http://www.jstor.org>

Review Articles

**The Unfashionable Elegance of Tsuru's Unorthodox Economics:
An Essay on *Institutional Economics Revisited*
and *Japan's Capitalism***

K. (Vela) Velupillai

An extended review of S. Tsuru, *Institutional Economics Revisited (The Raffaele Mattioli Lectures, 20-22, May 1985)*. Cambridge: Cambridge University Press, 1993a, and S. Tsuru, *Japan's Capitalism*. Cambridge: Cambridge University Press, 1993b.

My generation of undergraduate and graduate economics students were blissfully unaware of anything like *Institutional Economics*. We were schooled in gratuitous statements such as:

The theory of growth is not a theory of *economic history* . . . Where the theory is to be taken descriptively, it takes *the institutional setting* for granted and highly *idealises* it [Hahn 1971, vii; emphasis added].

If, in our education, Wesley Mitchell was mentioned, it was as a curious predecessor of monetary misperception theories of the business cycle¹ or as the father of the empirics of the NBER credo—"let the data speak for themselves"; if Myrdal was mentioned, it was during doctrine-historical exegeses on "Stockholm School" vs. Keynes; the very occasional references to Veblen may have been as a footnote to a

The author is Professor of Economics, Queen's University of Belfast, and a Senior Research Fellow in the research department of Statistics Norway, Oslo, Norway.

lecture on consumer theory and the empirical (in)validity of any supposition on *conspicuous consumption*.

The token concessions to radical demands were to books such as Paul Sweezy's *Theory of Capitalist Development* where we were, as a bonus, introduced to Shigeto Tsuru's incisive understanding of accounting aggregates. For the rest, we were brought up on the standard fare of Samuelson's classic *Foundations* and Patinkin's *Money, Interest and Prices* (MIP) and the methodologies codified in them: comparative statics, the "correspondence principle," etc. Some of us, however, ventured beyond the confines of such schemata—even as far as management schools and economic history departments. There we heard grumbles by Douglass North; iconoclastic brilliance by Herbert Simon; even within the citadel, we heard game-theoretic noises about institutionalism from Shubik and others. I have in mind, above all, Alchian and his non-Walrasian followers at UCLA where Marshack, too, was trying to introduce the new information paradigms to infuse institutional meat into the emaciated bodies of economic theories. Clower and Leijonhufvud were busy revamping macroeconomics toward similar evolutionary, institutional ends.

Then the new classicals took over! They introduced ad hoc shockeries to drive real business cycles, monetary misperceptions of signal processors, reincarnated Solow residuals, unit roots, cointegration, chaos, and OLGs. Frisch and Slutsky occupied the high road; the *Foundations* and MIP were the tools of the journeymen now. Macroeconomics became consciously, even conspicuously—to be Veblenian about it—mathematical, and the only institution considered was the perfectly competitive market, even though there was no theory of the market as an institution within the citadel.

The names that adorn Shigeto Tsuru's elegant books would make little or no sense to most graduate students of the leading schools of the West, including Tsuru's own Japan and even his own university. But no one should be excused for their ignorance after the appearance of these highly readable *Mattioli Lectures*, delivered in 1985.

So much for a macro theorist's apologetic preamble. To the applied economist and, in these days of *new growth theories*, also to the theorist, the miracle² economies of East Asia are grist to the mill. But the emerging applied work and the concomitant theoretical edifices are built on questionable facts, flimsy empirics, faulty accounting disciplines, and almost useless data. The stories that can be told of these miracle economies are many. None that seem to have been produced from within the traditional frameworks are as interesting as the word miracle indicates.

In contrast Tsuru, in his quietly confident and unorthodox way, has written a readable story of postwar Japanese economic performance [Tsuru 1993b]. It is not only readable, it is also believable, stimulating, and a work written in the great style of the pamphleteers and in the tradition of the political arithmeticians.

When a man works on experimental psychology for his first published paper, having previously been arrested for Marxist activities and being dismissed as a student from a prestigious university, and yet eventually becomes the president of an almost equally famous one in later life and, in between, occupies a vice minister-ship, becomes president of the *International Economic Association*, works as an economic advisor to the occupation authorities of his own country, and, during such appointments, continues academic research of the highest caliber, then there is not much wonder that he has become not only a prophet in his own country, but "also the world around": such is Tsuru's remarkable lifetime.³

It will, of course, be impossible for a somewhat narrow economist—albeit with good intentions—to summarize these two important books. At the risk of doing injustice to a great social scientist, I attempt this almost impossible task. I have, however, at least one (and, possibly, another half) qualification(s) that may excuse this attempt. I too claim a *direct descent* from Schumpeter via Richard Goodwin, my teacher, mentor, friend, and much else for more than 20 years. As for the possible "half," it was Myrdal's *Asian Drama* that deflected me from applied mathematics to economics.

*Institutionalism—The Raffaele Mattioli Lectures*⁴

[The great pragmatists] do not see it as their task to reduce rationality to a set of canons.

—Putnam 1990, 219.

Commons published *Institutional Economics* in 1934, in that amorphous period between Hayekian dominance and emerging Keynesian challenges when mathematical economics was also being codified for its modern versions by Wald and von Neumann. Vienna, Cambridge, and Stockholm held center stage. It is not surprising that *institutional economics* was almost stillborn.

Tsuru was trying his hand at experimental psychology in the early 1930s in Wisconsin (where, incidentally, Veblen was born), the results of which led to his first published academic paper on the subject of "The Meaning of Meaning." When Peirce set out to define *pragmatism*,⁵ it was as a method to determine *meaning* in philosophy. Are all these events—and those below—pure coincidences?

Thorstein Veblen, the acknowledged founder of institutionalism, was, in fact, a student of Peirce. Veblen, at one stage, was also Dewey's colleague. Wesley Mitchell was a student of both Veblen and Dewey. These are a few of the oddities of the personal origins of two genuinely American schools of thought: *pragmatism* and *institutionalism*.

The philosophical underpinnings of institutionalism have to be sought in pragmatism and its present offshoots—perhaps even in the philosophy of the later Wittgenstein. Those who criticize institutionalism for being an unformalized doctrine—and

most economists are guilty of this sin—have been barking up the wrong tree from the outset. Schumpeter, almost more than any other single person, is responsible for conveying this false impression. Tsuru's loyalty to Schumpeter is too strong, and his manners too gentle, for him to make this point.

Peirce and Veblen were kindred spirits—wild intellectuals who tried to eliminate pretension, metaphysics, and nonsense in intellectual discourse and institutional content. Tsuru suggests that Veblen did not want his ideas encapsulated in formalized structure. I disagree.

It is not that Veblen did not want his ideas encapsulated in formalized structures. It is, rather, that Veblen did not think there were sufficiently transparent and intuitively self-evident principles upon which to base the formalizations of his ideas. That is why I have “prefaced” this section with Hilary Putnam's important observation on the *methods* of the pragmatists. *That* is also why neoclassical economists find it difficult, if not impossible, to be sympathetic to institutionalists. I am sure that their reluctance is not due to ideological reasons. Surely, Mitchell, Myrdal, Commons, and Galbraith are not rabid radicals? The *sine qua non* of neoclassical economics is *its* closure, i.e., *preferences, endowments, and technology*. All else *must* be built up from this hard core, to be Lakatosian about it. Where, then, is there a place for institutions in this closure? Only in so far as it can be encapsulated in “endowments.” At the hands of imaginative theoreticians, it has been done. Indeed, even an interaction between technology and institutions can be modelled neoclassically and not only that, but even the interaction between ideology and institutions can be given some form of neoclassical content. Douglas North's *research program*, in the strict Lakatosian sense, is a repository of these ideas and efforts [e.g., Denzau and North 1994]. But North remains an outsider to the citadel in spite of the Nobel Memorial award—just as much as Tsuru and Myrdal are; and the latter, too, is a Nobel Laureate!

Institutionalists refuse any simple closure to characterize economics. They are, in this refusal, on solid mathematical, philosophical, and logical grounds—not to mention the psychological ones. This is what is not understood by the formalists of the neoclassical closure. If they had understood that Peirce tried to replace *truth* by *method*⁶ and that the pragmatists made “explicit the idea that methodology itself is something that evolves in the course of inquiry” [Putnam 1990, 219], then they *may* have found institutionalism congenial. But it is doubtful. North's imaginative work goes part of the way, but he, I would say, remains a *Lakatosian anarchist*; the institutionalists, and Veblen above all, are closer to Feyerabend in their *methodological anarchism*, if we have to use the labels that the philosophy of science has created for such discourses.⁷

Knowing these Peircian and pragmatist underpinnings of institutionalism as a mode of thought and discipline makes it easier to understand, sympathetically, Tsuru's attempt to characterize its defining elements as the following four:

- A. The emphasis on the *open-system* character of production and consumption, thus a broader view of the scope of economics;
- B. An interest in the *evolutionary* course along which the industrial economies are moving, with emphasis on the dynamic process of *technological change* and circular *cumulative causation*;
- C. Awareness of a growing need for guidance that can be supplied only through some form of overall social management or *planning*;
- D. Recognition that economics must become a *normative* science, positively formulating social goals and objectives [Tsuru 1993a, 101; emphasis in original].

How would an orthodox economist—that elusive creature—react to this characterization? Mercifully, we do not have to construct a strawman to shoot down; the strawman presents himself in the person of Professor Riccardo Rovelli who, in the comments on Tsuru's Lectures that are part of the volume, skillfully points out the achievements of orthodox economists on institutionalist issues [Tsuru 1993a, 155-9]. Admirable, though, Rovelli's knowledge of the achievements of orthodox theorists may be, his understanding of the *objectives* and *methods of analysis* employed by institutionalists, as characterized and discussed by Tsuru, seem to be quite comprehensively deficient. Rovelli's distorted understanding is best exemplified by one example *he* chooses to discuss.

Tsuru, in arguing persuasively for a revamping of accounting conventions for estimating national economic potential, pleads with us to go back to Fisher's classic definition of production. This will call forth a more widespread use of stock measures and a balance-sheet framework in national accounting rather than flows and income accounts. There is, then, a homely example that Tsuru harnesses to make not only the above points, but also the other *social* and *normative* concerns that have to be valued to measure economic performance and potential. Now, Rovelli takes this homely example in the following direction:

Regarding the environment, Professor Rovelli finds intriguing the statement in which Professor Tsuru poses the question of "whether we should expand our kitchen at the sacrifice of our garden within the limited area of our premises," since it appears that Professor Tsuru is actually paraphrasing L. Robbins's "relationship between ends and scarce means which have alternative uses." Professor Rovelli wonders whether Professor Tsuru is becoming neo-classical with this point but offers, in response to his own query, that Professor Tsuru would object to the allusion primarily because there is no market for the garden, which implies that we can disrupt it with zero private and high social costs. Although this observation is true, Professor Rovelli deliberates why this is so and he finds that the answer again lies with the orthodox

economists who propose the economic theory of property rights as well as the criteria for economic utilization of natural resources [Tsuru 1993a, 158].

This passage brings forward some philosophical and logical issues pertinent to institutionalism. First of all, however, it should be noted that the conflation of Robbins and Tsuru is complete nonsense. Robbins's definition is within the neoclassical closure in its hardest core. Tsuru *should* reply that the *methods* used to formalize and analyze the ends and the scarce means, and their analytical specification [Denzau and North 1994]—including an analysis of the *meanings* attached to all of the relevant entities—is the whole point of the difference. Rovelli does not realize that the neoclassical method is crudely positivistic;⁸ the institutionalist's methods would, at the least, call forth *modal* and *deontic* logics for analysis long before calculations are performed, let alone crude optimization. The neoclassical underpinnings are in classical, indeed *first-order*, logic. The institutionalist desideratum about economics becoming a *normative* subject is not simply an enumeration of norms for constrained optimization purposes, as in standard economic theory, but also a logical analysis of the norms. How are norms analyzed? Not by classical logic, but by the deft use of deontic (and modal) logic(s). It is not for nothing that Clarence Irving Lewis, who studied with William James and was particularly sympathetic to pragmatism, was one of the first to modernize modal logic.

Such concerns are the basis for the philosophical and logical underpinnings of institutionalism, and they are as far away as you can get from the crude positivism, and its underpinnings in classical logic, of neoclassical methodology.

On another point, in response to Rovelli's request for an explicit definition of open systems, Tsuru replies by quoting Myrdal:

. . . in reality there are no economic, sociological, psychological problems, but just problems and they are all mixed and composite [Tsuru 1993a, 159].

Formalism, in the strict mathematical sense of the term, requires the definition of a closure if the mathematics of the formalists are to be applied and the methodology of the positivists is to be invoked. This is the domain of neoclassical analysis. The institutionalists will have nothing to do with a closure required solely for the purpose of working within the framework of a rigid mathematics. There are other mathematical structures to work within, structures that are not so rigid.

I come, now, to the Marxian links. Tsuru has not abandoned his early fascination and convictions about Marxism (but whatever happened to *experimental psychology*? Perhaps it has been subsumed in the *pragmatism* of his belief in institutionalism!). That Marx had a theory of the interaction between evolution, technology, and institutions there is, of course, no doubt. Tsuru's abundant, judiciously selected quotes from various of Marx's writings make the case clearly—even too strongly. Surprising, though, that one of the earliest statements—if not *the* earli-

est—on these issues is not mentioned. I refer to the following paragraph in the 1859 Preface to *A Contribution to the Critique of Political Economy*:

In the social production of their life men enter into definite relations that are indispensable and independent of their will, namely *relations of production* appropriate to a given stage in the development of their *material forces of production*. The totality of these relations of production constitutes the economic structure of society, the real foundation, on which arises a legal and political superstructure . . . At a certain stage of their development, the *material productive forces of society come into conflict with the existing relations of production*, or—this merely expresses the same thing in legal terms—with the *property relations* within the framework of which they have operated hitherto. From forms of development of the productive forces these relations turn into their fetters. Then begins an era of social revolution [Marx 1859; emphasis added].

Shorn of the characteristically Marxian vitriol, there is nothing here that a modern theorist who emphasizes property rights and their incentive structure for the evolution of institutions will find unpalatable. But Tsuru does not emphasize this part of Marx. He, instead, looks at Marx as an institutionalist via a comparison with Keynes and Schumpeter; the comparison with the former in terms of an analysis of the foundations of the accounting aggregates that are fundamental for the respective analytical structures; the comparison with the latter in terms of their different approaches toward analyzing business cycles in capitalist societies. Is this comparison between Schumpeter and Marx in terms of their respective business cycle theories to bring into the fold, later, Mitchell too?

On the other hand, I was always taught by Richard Goodwin, that brilliant Schumpeterian descendent on business cycle theorizing *par excellence*, that Marx had *three* different theories of the business cycle, as, of course, Tsuru must know. These theories are the *distribution cycle* of Volume 1 of *Das Kapital*, the *reinvestment cycle* of Volume 2, and the *underconsumption cycle* of Volume 3. But Tsuru's point is that it is only in the structure of the *reinvestment cycle* that the aggregative accounting implications of the *real-value* dichotomy are most transparent. Thus, by choosing to emphasize the institutional basis on which Marx tries to resolve this endemic problem, he is able to show the deficiencies in Keynes and Schumpeter. There may also be a personal reason for this approach. Tsuru has spent a great deal of time trying to expose the weaknesses in conventional national accounting structures, particularly the overemphasis on flow magnitudes. These structures are very much "Keynesian" contributions to applied economics. Tsuru's point here is most interesting. It is because Marx is aware of the social relations behind the surface phenomena of what appear to be natural attributes of things that he takes both the flow and stock dimensions into account in the aggregates of his system: the former a measure of the short-term and the latter that of the long-term creation of the produc-

tive potential. This is, of course, also the difference between Domar and Harrod. It is all very intriguing, original, and fascinating, even though I am not convinced that this is the best way to compare the institutional underpinnings of the systems of economics built by Marx, Keynes, and Schumpeter. Indeed, I do not believe Schumpeter had *any* such system of economics. He did, however, have many kaleidoscopic, but obscure, visions of social dynamics that had all sorts of institutional features that I find as interesting as the impossible diagrams of Escher, Reutesvärd, and others.

But what about the role of ideology in that triumvirate of technology, evolution, and institutions? Surely, without a role for ideology in the functioning of this triumvirate, Marxism will be like Hamlet without the Prince! I mention this for another reason, too. I want to bring in Vico. I do this in a roundabout way.

Douglass North has made valiant efforts to harness the cognitive science literature to try to theorize about the personalistic generation of ideology—somewhat like the way de Finetti generates probabilities—the evolution of institutions and the performance of economies in the aggregate built from *bottom-up* considerations. I do not fully subscribe to North's results [Denzau and North 1994; and other references therein] because I believe that the connectionist credo of the cognitive science literature that he appeals to is flawed. But *that* is quite another story. The point is that North and his coworkers are aware of the need to encapsulate, within an evolutionary framework, the existence and transformation of institutions against the backdrop provided by ideologically motivated behavior by rational agents—although not the substantively rational agents of orthodox theory.⁹ However, it must be emphasized that North's methodological roots remain formalist and within the framework of the neoclassical closure.

Where does Marx stand, as an institutionalist, in relation to North and others on the fringes of orthodoxy? To answer this, we must go back to Marx's roots in Giambattista Vico. I will speculate a little bit, but with Schumpeter's support this time. Surely, Herder, Michelet, and Hegel were all directly instrumental in fashioning Marx's path toward historical materialism. This episode is equally replete with coincidences. Michelet came by Vico at almost the same time as Hegel was lecturing on the philosophy of history at Berlin in the early 1820s. A few years later, Marx arrived in Berlin as a student. But back to Vico. Schumpeter's pithy characterization of Vico is worth recalling:

[Vico's] new science (*scienza nuova*) is best described by the phrase "an evolutionary science of mind and society." But this must not be interpreted to mean that the evolution of the human mind shapes the evolution of human society; nor, though this would be nearer the truth, that the historical evolution of societies shapes the evolution of the human mind; but that mind and society are two aspect of the same evolutionary process. Reason, in the sense of the rational or logical operation of the human mind, is no causal factor in this process which Vico conceived in a thoroughly anti-intellectual spirit.

Neither has reason, in the sense of goals or meanings perceived by the observer's reason, anything to do with it: Vico's theory of recurrent processes (*corsi e recorsi*) emphatically denies any tendency toward, and in fact the existence of, any such goals and meanings. In this scheme philosophy and sociology had become one—thought and action had become one—and this unit was essentially historical in nature. [This scheme, very obviously, points forward toward Hegel and, less obviously, toward Husserl] [Schumpeter 1954, 127].

What happened to the Vicovian roots of Marx? I believe Marx's method to be Cartesian. He is, in this, at one with the neoclassicals. Great "inductor" though he was, in the final analysis, it was the deductive method that dominates. That he did not get from Vico.¹⁰ Secondly, Marx took, to paraphrase Keynes on Moore, Vico's vision but discarded his method. He took the vision of history as a process. As is well known, he then proceeded to turn poor Hegel upside down and graft his materialist philosophy to Vico's vision of history as a process; the institutionalists grafted pragmatism to Vico's vision of history as a process,¹¹ and, in my opinion, North grafts Hegel, put right side up now, to Vico.

Here it will be useful to recall McCall's important observation that "the prime mover in Vico's evolutionary theory of society was the quest to control uncertainty" [McCall 1995, 5]. This is neither just the narrow uncertainty of formal analysis, nor even of the Keynes-Knight variety. It is more encompassing and best encapsulated, if it must be, in terms of the newer, complexity-based approaches to probability and information via randomness. A word that has been used to describe this type of uncertainty is *patternlessness*: forms, events, and movements from which induction is impossible. To tame these, so that life can be reasonably predictable, institutions come into being; dogmas, magic, and miracles are also institutions since, of course, churches are too. In Vico's world, the emergence of institutions facilitate an evolutionary societal development. This is as far from conventional Cartesian deductivism and rationality as it is from contemporary Social Darwinism. In it, suboptimal structures persist. It is what theorists call *path dependence*, the sort of pattern of events that lock us into the QWERTYs of this world.

To get back to Marx, the institutionalists, and North—the latter taken to represent "orthodox theorists," which is unfair to North—I think Tsuru stretches the points too far to include Marx in the institutionalist fold. The institutionalists are the true descendants of Vico; Marx and North must remain in the fringes with Hegel and his upside-down image to support them.

I come, finally, to the Alchian-Coase school, the asymmetric-information school, and the game-theoretic approach to evolutionary institutional economics. All of them accept the neoclassical closure; all of them are instrumentalists; all of them, moreover, are ahistorical. This last point must be made with care and reservation, especially with regard to Alchian. Accepting the neoclassical closure means subjecting the emergence of institutions, whether evolutionary or not, to economic analysis

in the classic Stigler-Becker sense of *De Gustibus Non Est Disputandum*. That they are instrumentalists is another way of stating the dominance of *no-arbitrage* interpretations of observable events. The world is Panglossian—almost. That they are ahistorical means that they are not Vicovians in any of the three senses mentioned above, i.e., the Vicovian pragmatists, the historical materialists, or the Hegelian evolutionists, as I would like to call North and his followers.

There is much else in this book: the accounting foundations of aggregates; the institutional foundations of business cycles and a critique of Schumpeter's incomplete foundations (compared to Marx) for analyzing the vagaries of capitalist dynamics; and, finally, Tsuru's lifelong work to refound the accounting underpinnings of the welfare measures of national income aggregates.

I have two further minor observations. One concerns Myrdal. Tsuru's knowledge of the work of that great man is admirable. Myrdal's dissertation was never translated in any of the major European languages, but a better translation of the original title¹² would be *Dynamic Pricing* (as Lindahl translated it in his brief review for the *Economic Journal*). I myself prefer a literal translation: *The Problem of Price Formation and Change*, awkward though it may sound to musical ears.

The other point is strictly methodological. Tsuru admonishes Eatwell as follows:

. . . the somewhat imperious attitude of abstract economic theorists typified by John Eatwell's remark to the effect that "if the world is not like the model, so much the worse for the world . . . [Tsuru 1993a, 83].

On the face of it, this is, indeed, an imperious remark. But I believe the origins of a statement like this go back to the early part of this century and Einstein. When asked by some half-baked journalist what if the Eddington expedition to measure the red shift of the perihelion of Mercury turned out to give the "wrong" results, Einstein is supposed to have replied: "Bad luck for the real world; the theory is right!" At least that is the gist of the story. More specifically, Einstein had written, already in 1914, to his friend Besso as follows: "I do not doubt any more the correctness of the whole system, whether the observation of the solar eclipse succeeds or not" [Pais 1982, 303].

Here the discussion is about predictions of precise magnitudes on the basis of deep mathematical *and* philosophical foundations for phenomena that *could* be measured. Are we in that position in economics? Perhaps not. Some leading theorists believe that *theory is ahead of measurement* [Prescott 1986]. The institutionalist objection should not be the one that Tsuru gave in the book, nor the one suggested by Pasinetti in the discussions. The institutionalist point should be that we have not yet localized sufficiently the principles for our theory. That is why an open system is needed. The neoclassical closure reminds me of the story of some far-off islanders building airports and then waiting for planes to land—because their leader had noted, in one of his journeys, that wherever there were airports, planes were seen to land. We have noted that the hard sciences have built up effective closures and guid-

ing principles: symmetry, gravitation, etc. We think we can become a hard science by imitating the glittering surfaces. *That* is the imperious attitude. Perhaps Professor Cutolo, in the discussions in the book, was more to the point about how to learn from the practice of the hard sciences. Ours is a subject that can never have a Tycho Brahe; but Galileos and Einsteins are possible because they were also thinkers about new principles and concepts to facilitate the taming of patternlessness.

Let me return to the ground floor. In this age of growthmanship and miracles, who else but a man of tempered visions like Tsuru can have the courage to challenge conventional statements with this kind of computations:

. . . it appears to me that the net change in the welfare stock value of residential buildings in Japan over the forty years between the middle of the 1920s and the 1960s was, if at all, in a negative direction. Even if we err grossly on the optimistic side, the stock approach gives us at best a 15 percent rise, which is to be compared with the 2.2 times rise in the flow approach [Tsuru 1993a, 99].

This is empirical social science at its best, and *Japan's Capitalism* is an embodiment of such enlightened analysis. To that book I now turn.

Japan's Capitalism

*These horned islands that the tossing seas
Rake and rumple to their dragonish end . . .*

*Hokkaido, tufted devilfish in ragged rocks,
Honshu, rough-spined, vigorous eel,
Shikoku, a ribbed and plated crab, and green
Kyushu, seahorse on a capering keel. . . .*

The neat, tremendous garden of Japan

—James Kirkup

“The neat, tremendous garden of Japan” lay in war-inflicted ruin when “the Emperor broadcast over the radio the historic message of the termination of the war on August 15, 1945” [Tsuru 1993b, 11]. From this came a “. . . creative reaction to defeat: Japan is an example of a fantastically creative response to defeat” [Tsuru 1993b, 67]. Tsuru chose Boulding’s apt phrase to subtitle his highly unusual story of Japan’s economic performance since that historic mid-August day. The book is an engrossingly written text on applied macroeconomics of the highest quality. There are no apologetics, no revisionist tales about the beneficial effects of the war machinery, and no exaggerations.

The tone of the book is that of a custodian of a humane future in view of the great responsibilities that wealth has brought to Japan. It is humble in its approach to the past, generous in its visions, and constructive in its hope for the future. The author's lifelong commitment toward an understanding of the vicissitudes that an economy undergoes because of the *real-value* (real-monetary?) dichotomy is evident not only in the written lines of the book. By way of explaining his own perspective on the longer-range implications of this dichotomy—particularly in view of the scandals that have afflicted the bubble economy that was Japan in the late 1980s—the author restates one of Schumpeter's great parables: "You can ride on a horse, but not on a claim to a horse, whereas a claim to money or to objects of money is as good as money itself" [Tsuru 1993b, 2].

Thus, in reading this book it is not only necessary to remember that the author is writing from an institutional standpoint, but also that he has a peculiar scepticism about the monetary system of a capitalist economy but that scepticism does not extend to the real system. This is not only an economic scepticism, but also a moral one, for Tsuru. As he notes, I think with some poignancy:

A recent opinion survey of high-school students showed that more than 50 percent of them responded affirmatively to the statement that "the present-day Japan sets too much store by money and material objects and makes little of warmth of heart." Further, on the question: "Which colour do you choose to characterize the future of Japan?", their response was "grey" 38.8 percent, "black" 15.7 percent and "rose-color" 3.1 percent. The survey suggests clearly that the younger generation in Japan is groping for concrete guiding principles for themselves, *living as they do in the money-contaminated materialistic society* of today [Tsuru 1993b, 3; emphasis added].

The sad thing, of course, is that similar responses would probably be elicited from high-school students in any of the world's poorer nations, which actually, underscores Tsuru's point even more forcefully. And then, as the custodian of a decent future, Tsuru concludes with a hope about the commitment his generation owes to the "grey" generation:

It is certainly high time, I believe, that a generation like mine, which has gone through Japan's vicissitudes of war and peace, hardship and affluence, fulfill its responsibility of transmitting the lessons learned and of bringing out in bold concrete visions for the future of the country, so that hopefully at least 30 percent of the high-school students and not a mere 3.1 percent will be able to see "rose-colour" prospect for themselves [Tsuru 1993b, 3-4].

Even the hopes, as can be seen, are moderate.

The book is about the "growth and development of the Japanese economy since the last war" [Tsuru 1993b, xi]. But why another book on this well-mined topic? Tsuru's answer is interesting but not unexpected. It is because very few, if any at

all, of the legions of books that have been written on this topic have done so from the point of view of institutionalism: "But very few, have related this subject matter to the evolution of Japanese capitalism in its institutional characteristics" [Tsuru 1993b, xi].

So, the *Mattioli Lectures* and this book form a couplet. The methodology and philosophy described and discussed in the former are given an applied platform in the latter. As mentioned above, without this in mind, the unwary reader would have an unjustified sense of *deja vu*.

I will concentrate on three issues dealt with in the book. Two of the issues have a clear institutional backdrop to them; the third is more controversial—less unorthodox, to put it paradoxically. The first two are

1. The description of the economic and structural implications of an undervalued yen.
2. The discussion of the *Zaibatsu* consolidation (toward the modern *Keiretsu*?).

The third issue is, ostensibly, about an accounting analysis of the high-growth period. I am not sure that it is strictly an accounting analysis and hence may fall foul of the very strictures that Tsuru has employed against the use of *growth accounting* in the introductory chapter on the basis of his trenchant analysis of the *real-value* dichotomy in Tsuru [1993a]. This is not the usual real-monetary dichotomy because Tsuru has an institutional interpretation of *value*. But then, there are those who have that interpretation of money too! In any case, the third issue is

3. The accounting analysis of the period of high growth.

All three of these issues are discussed in the important third chapter, "The Period of High Growth Rate."

This chapter is preceded by an exceptionally lucid account of the economics of the period from September 2, 1945, to April 28, 1952, the total, official, occupation period. Tsuru divides this period into two: Chapter 1 is a summary of the *facts* of defeat and the ensuing initiation of reforms (that were not always implemented) by the occupation authorities; Chapter 2 is a description of the economic settings—internal, as instituted by the occupation authorities, and the international, exogenous, setting for the functioning of the Japanese economy in that crucial period.

The quintessential examples of the time are the various calisthenics of Joseph Dodge,¹³ the most memorable one being the setting of the yen/dollar exchange rate at 360. The exogenous events that had deep economic impacts in that period were the defeat of the Kuomintang in Mainland China and the eruption of hostilities on the Korean Peninsula. That they were yet another incarnation of the legendary di-

vine winds are recognized by Tsuru with the touch of sadness that one can discern only in great minds. Tsuru recognizes that these calamitous events brought great economic benefits to Japan at the expense of immense socioeconomic upheavals in these neighboring countries to whom Japan owed so much—culturally and economically. These two chapters, therefore, set the stage and pave the way for the illuminating Chapter 3 to which I now turn.

Let me tackle the issue of accounting for growth. In his introduction, Tsuru makes it clear that he will not use the traditional growth accounting framework in his empirics. His reasons are clear and acceptable, and I have pointed them out earlier. There are, however, similar infelicities in the identities he derives from accounting *and* behavioral considerations. The relation he uses is

$$G = xy = \alpha/\beta$$

where:

G = rate of growth of real GNP

x = rate of change of the labor force (or of aggregate working hours)

y = rate of change of labor productivity (or of per man-hour productivity)

α = gross savings ratio

β = incremental capital coefficient.

First of all, I read this as the Harrod-Domar equation. Second, I do not see how Tsuru can escape his own strictures against growth accounting when he uses an aggregate capital measure to derive his relation. Third, he is assuming some form of *behavioral* equilibrium in the goods market, as in Harrod-Domar. Fourth, the *tentative* assumption of a constant capital-output ratio is never removed. Finally, the assumption that xy is small and negligible is tenuous.

Take this last point. The rate of change of labor productivity for the high growth period was *at least* 0.0175, but more likely greater than 0.02; the rate of growth of the labor force was close to 0.02. The compound figure of more than 0.04 percent can, perhaps, be neglected in growth accounting exercises. But Tsuru himself has figures of .089 and .178 for the rate of growth of labor productivity for the 1960-65 and 1965-70 periods. It is not clear to me that xy can be neglected, and thus the equilibrium between the “warranted rate” (R.H.S) and the “natural rate” (L.H.S) will require either quite abnormal saving ratios—even for Japan—or the assumption of a constant capital-output ratio has to be dropped, or both.

The constancy of β is, in fact, an implicit assumption about returns to scale. It would be quite difficult to square up all the empirical facts that Tsuru enumerates with constant returns to scale technologies. So, the constancy of β must go, and with it the whole set of identities collapse. But, of course, we can return to growth accounting in its new guises to make sense of the high-growth period. Since Tsuru

seems to have no qualms about using a measure of aggregate capital, we can, provisionally, ignore his strictures against growth accounting.

But his strictures against flow accounting remain; these mitigate against the use of a conventional production function, because they relate flow inputs to flow outputs. But, then, we can ignore the production function and work with Kaldorian *technical progress functions*—and use only gross capital concepts. I suggest that the mysteries of the high savings ratio of Japan will be dispelled in conventional ways once this exercise is done seriously.

On the other hand, Tsuru's empirical intuition does not require even elementary identities strung together as relations between dubious aggregates to make the points he wishes to make: the institutional structure that determines the high savings ratio and its channelling toward the corporate structure; the sources of effective demand that warranted the high rate of growth; structural change induced by technical progress via high investment ratios; and the role of government. These are all conventional issues for which orthodoxy has given many satisfactory answers, some of which are also discussed by Tsuru. However, the two issues we have enumerated as (1) and (2) above are given institutional twists by Tsuru, i.e., the fate of the *Zaibatus* and the evolution of business organizations and the impact of the setting of a single exchange rate by Dodge.

Let me take the latter issue first. I do not think there is full information on the process by which Dodge arrived at his celebrated figure of yen 360/dollar.¹⁴ I do not even think we have the full story of the situation Dodge faced, from the Japanese side, in spite of the fact that Tsuru has a lucid discussion of what he calls *the exchange rate problem* [pp. 49-52] and the lucidity is substantiated by the fact that Tsuru himself had been responsible for drafting a recommendation, submitted to the Economic and Scientific Section of SCAP¹⁵ on "the exchange rate problem." The problem itself was basically about the grounds for determining a single exchange rate¹⁶ and the dynamics of achieving it *and* maintaining it at that level.

I state all this somewhat tentatively and remain sceptical about whether the full background information on the deliberations has ever been disclosed, for the following reason. Being an admirer of Tsuru, I have had occasion to read his writings in the internationally available literature. In the famous Oxford IEA conference, Tsuru presented a characteristically informative piece on fluctuations in postwar Japan up to 1952 [Tsuru 1955]. In that publication, Tsuru was more optimistic about the correctness of the choice of yen 360/dollar. Using PPP figures, with political and socioeconomic reasons for choosing 1934-36 as base years, Tsuru was able to satisfy himself that the Dodge choice was correct. However, in 1993 Tsuru was less convinced that it was the theoretically correct rate: "[Dodge's] decision turned out, just as some of us commented on it at the time, *to be erring on the side of undervaluation of yen*" [Tsuru 1993b, 52-3; emphasis added].

Surely, then, the problem was not “how to determine the equilibrium rate of a currency” [p. 51]—particularly an equilibrium concept that uses flow accounting magnitudes and their balances, as suggested on page 51. If we take Tsuru at his word and take stock equilibrium conditions seriously and construct balance sheets, perhaps the 20-23-year Bretton Woods era is the sort of time span needed to get the trade accounts right. After all, it was only in the very late 1960s that the trade accounts ceased to be a constraint on growth in Japan, which is also why they took a tough stance in that period’s textile negotiations.

I come, finally, to the issue of *Zaibatsu* reconsolidation. Tsuru is quite explicit, with statistics to buttress his arguments, that the occupation authorities gave up on the deconcentration of prewar *Zaibatus*. They were allowed to reconsolidate to various degrees, and Tsuru’s concern is the impact of this reconsolidation on the structure of the postwar Japanese economy.

Tsuru enumerates three implications of the looser, reconsolidated *Zaibatus*:

1. The emergence of the so-called “one-set” principle; i.e., the practice of trying to have the capacity to produce “one-set” of every conceivable product.
2. The possibilities for “new entries” into innovative industries required investment funds that had to be sunk for longer gestation periods than commercial capital markets were prepared to accept.
3. The advantages, and the necessity, of internalizing external economies.

One clear implication of these behavioral and institutional innovations was endemic overinvestment. It is here that we must seek one of the causes for the aggressive export drives of many of the Japanese firms that belong to these loose and reconsolidated *Zaibatus*. It is this that Tsuru is trying to tell us from his institutional approach to a description of the Japanese economic facts.

There is so much else that is truly refreshing in this book that one begins to feel some hope for the “poor little rich nation.” The most poignant are the final, hopeful, and pleading pages. There is a three-point program for making Japan a humane country; a country that would feel compassion, take responsibilities, and share burdens; a country that would preserve and rebuild its natural resources; a country that would give new impetus to cultural and aesthetic values.

This is a work of almost literary exquisiteness, but it is also that of a passionate pamphleteer; above all, it is the work of a great economist writing in the classical traditions that have become a lost art in the mathematical maze that has become economics.

Notes

1. As Lucas [1981, 9] stated: "If Wesley Mitchell could view agents as 'signal processors' in 1913, then I saw no reason to regard my own adoption of this viewpoint in 1973 as unduly speculative." But did Mitchell view agents as "signal processors" in 1913, or ever? I certainly found no evidence in his massive work of 1913.
2. This word, normally associated with theological writings, is increasingly emerging as the code word for Japan, the four tigers, and the newly industrializing economies of Malaysia, Indonesia, and Thailand, in view of their alleged exceptional economic performances. The World Bank's recent monograph on these economies is appropriately titled *The East Asian Miracle* [World Bank 1993]. In my opinion, the analytical underpinnings of this monograph are theological. In this sense, therefore, the book will not mislead readers who go by the title.
3. *Pace*, Paul Streeton (cf. the opening lines of his "Introduction" to Myrdal 1958, ix).
4. Whatever I know about institutionalism is knowledge imparted to me in conversations, instruction, and advice from my friend and former colleague John McCall. My subsequent readings on the subject, inadequate though they are, emanate from *this* influence, especially on Vico.
5. In "How to Make Our Ideas Clear" [*Popular Science Monthly* 1878], Peirce first used the word "pragmatism." Years later, when he did not like William James's popularization of pragmatism, Peirce invented the term "pragmaticism" to describe his theory on the grounds that it was such an ugly word that no one would steal the ideas embedded in it! [Hacking 1983, 58].
6. Gödel, in a different but not unrelated context, replaced truth with provability.
7. Recall that Feyerabend's classic, *Against Method*, has the following dedication: "To IMRE LAKATOS Friend and fellow-anarchist."
8. This point has, of course, been frequently and loosely made a countless number of times ever since Friedman took it upon himself to be explicit about neoclassical methodology. The point here is that the institutionalists are not positivists [see Hausman 1992, esp. chap. 9, for an illuminating discussion of these methodological points].
9. I found it somewhat mysterious that North does not refer to Simon's fundamental work on exactly these issues.
10. But was Descartes a Cartesian? In his brilliant *Daves Hicks Lecture* of 1973, Ian Hacking [1973] makes a cogent case for answering this query in the negative.
11. But this grafting must have been easy. After all, Vico points toward the phenomenologists, at least according to Schumpeter, and I have always felt that the pragmatists, the phenomenologists, and the intuitionists provide the soundest foundations for a latter day *scienza nuova*!
12. *Prisbildningsproblemet och Föränderligheten*, Almqvist & Wiksell, Uppsala, 1927.
13. A Detroit banker who arrived in Japan, with the rank of Minister, as President Truman's personal representative.
14. Anecdotal evidence of an amusing sort suggests that this rate was chosen almost frivolously. Apparently, Dodge had asked for the meaning of the Chinese character for "yen" and was told that it stood for "circle." Hence the choice of "360"!
15. Supreme Commander for the Allied Powers.
16. In a country that, at this juncture and on the eve of the arrival of Joseph Dodge in early 1949, had commodity exchange rates varying between 180 and 800 yen to the dollar, it was understandable that the search for principles on which to determine a single rate and a time-phased plan to achieve it were of paramount importance.

References

- Denzau, Arthur T, and Douglass North. "Shared Mental Models: Ideologies and Institutions." *Kyklos* 47 (Fasc.1 1994): 3-31.
- Hacking, Ian. *Leibniz and Descartes: Proof and Eternal Truths*. Dawes Hicks Lecture on Philosophy. Oxford: Oxford University Press, 1973.
- _____. *Representing and Intervening*. Cambridge: Cambridge University Press, 1983.
- Hahn, Frank H., ed. "Introduction." *Readings in the Theory of Growth*. London: Macmillan, 1971.
- Hausman, Daniel M. *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press, 1992.
- Lucas, Robert E. *Studies in Business-Cycle Theory*. Oxford: Basil Blackwell, 1981.
- McCall, John. "Viconomics: An Evolutionary Science of Mind and Society." Forthcoming in *Essays in Honour of Axel Leijonhufvud*, edited by Daniel Vaz and Kumaraswamy Velupillai (Proceedings of the *Montevideo Conference*). London: Macmillan, 1995.
- Marx, Karl. *A Contribution to the Critique of Political Economy*. 1859. Reprint. New York: International Publishers, 1970.
- Myrdal, Gunnar. *Value in Social Theory*. London: Routledge & Kegan Paul, 1958.
- Pais, Abraham. *Subtle Is the Lord*. Oxford: Oxford University Press, 1982.
- Prescott, Edward C. "Theory Ahead of Business Cycle Measurement." *Federal Reserve Bank of Minneapolis Quarterly Review* 10, no.4 (Fall 1986): 9-21.
- Putnam, Hilary. *Realism with a Human Face*. Cambridge, Mass.: Harvard University Press, 1990.
- Schumpeter, Joseph A. *History of Economic Analysis*. London: George Allen & Unwin, 1954.
- Tsuru, Shigeto. *Institutional Economics Revisited*. Cambridge: Cambridge University Press, 1993a.
- _____. *Japan's Capitalism*. Cambridge: Cambridge University Press, 1993b.
- _____. "Business Cycles in Post-war Japan" In *The Business Cycle in the Post-War World*, edited by Erik Lundberg, 178-200. London: Macmillan, 1955.
- World Bank. *The East Asian Miracle: Economic Growth and Public Policy*. Oxford: Oxford University Press, 1993.