At first glance, this book is three books in one: (i) a history of Japanese monetary economics and policy spanning the years 1868 to 1936; (ii) a delineation of the Japanese contribution to ‘neoclassical economics’ understood, on the one hand, to be Marshallian demand and Walrasian general equilibrium theory, and on the other, econometrics; and finally, (iii) a portraiture with profiles of Amano Tameyuki\(^1\) and Martin Bronfenbrenner. As such, it is indeed “a history of economic science in Japan,” and one rendering of the Japanese chapter in “the internationalization of economics in the 20th century.”

As regards (i), the relevant question is how Chapters 3 and 9, supplemented by 2, complement Metzler (2006): a work now widely acknowledged to be a brilliant and beautifully written book, indispensable for understanding ... the international monetary system and the financial power structures embodied in it, [one that] draws out family resemblances between the Japanese banking crises of the 1920s, involving as they did crony capitalism, moral hazard and international capital flows, and the Asian financial crisis of 1997, or sees in the policy prescriptions of the great international bankers of the interwar years the forerunners of IMF and World Bank structural adjustment packages.”\(^2\)

Ikeo’s discussion undergirds the drama of Japan’s return to the gold standard in 1931 by a focus on doctrine:\(^3\) texts of Fujisawa Rikitaro on Marshall’s fixed-ratio-mintage proposal; Yamazaki Kakujiro, Kanbe Masao, Takashima Saichiro, all on Fisher’s indecis; Fukai Igeo and Hijikata Seibe on Cassel’s 1926 memo; and Fukuda Tokuzo, Ishibashi Tanzan and Takagaki Torajiro on Keynes.\(^4\) These texts, all in Japanese, are brought to bear on what Metzler saw as “perhaps the greatest economic policy mistake in Japan’s modern history (p. xiv),” and elaborated as the contrast between Inoue Junnosuke’s deflationary negative policies and the expansionist (sekkyoku seisaku) Keynesian positive policies of Takahashi Korekiyo. Footnote 2, Chapter 3 draws attention also to the more recent work of Kato Toshihiko (1983) and Shizume Masato (2009). There is much more to be done however, and it is this reviewer’s hope that the author will return to these

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1 I follow the convention of the field in stating the last name first; see Metzler (2006; p. xxi).
2 See Frances (2007). Giraldez (2007) calls it a “classic in the historiography of Japanese and world economic history” from which historians and economists would “profit very much.” Also see Lee (2009) for a reading of the book as a counterpoint to the Hobson-Lenin theory. In any case, the book was glowingly and widely reviewed, and its exclusion from Ikeo’s bibliography is an obvious blemish.
3 Note that this is the one weakness that Lee (2007) sees in Metzler’s book: “he does not directly study the impact of disciplinary economics on the training of the key personnel.” He writes, “It would be interesting to hear the author’s views as to how one may use the empirical evidence he has garnered to construct a more theoretical framework for understanding a second-rate power’s role in shaping the international system.”
4 Note that the last three are introduced in Chapter 9, but the others crop-up all over.
texts and possibly develop a more comprehensive reading based on the “reflection of international political and economic hierarchies juxtaposing the metropole versus the periphery both globally and within the Japanese empire”; what Hunter (2007) sees as Metzler’s emphasis. One gripe is that Chapter 9 could be more illuminatingly titled “Keynesian policy prior to Keynes” rather than lifting Leijonhufvud’s clichéd title.

As regards (ii), it is best to differentiate between Walrasian general equilibrium theory (Chapters 4 to 6, also supplemented by 1 and 2) from econometrics (Chapter 7). Ikeo introduces her Chapter 6 on “the existence question” with the assertions that

The proofs of the existence, stability and uniqueness are important topics for the research of general equilibrium theory. The history of the research of the existence question is so complicated that we also have to pay attention to the equally complicated history of modern algebra. Section 2 summarizes the beginning and the ending point of our history of the proof of the existence of general competitive equilibrium and the current condition of the historical study of this question and its related subjects.

Leaving aside the questionable relevance of the history of abstract algebra, surely there is more to general equilibrium theory than proofs. Its founders – Cassels, Hicks, Keynes and Samuelson undoubtedly among them – conceived of their subject as pertaining to the representation of the economy as a whole. Even if one privileges Walras, the connection between him and Cournot, or between him and Edgeworth, constitute important research programs. More specifically, this reviewer has never fully understood the simple-minded preoccupation with the Arrow-Debreu paper, and extended narratives concerning all those who have been “robbed” of scientific credit as a result of its publication. Lionel McKenzie is one of the staples, and the author now adds Nikaido Hukukane. She writes

Nikaido’s existence paper might have been accepted at Econometrica after a normal refereeing process, if Arrow of 1968 or Debreu had refereed it. It seemed to me that Nikaido was hiding something which he was very reluctant to tell me and this suggested to me that something strange had occurred in the referring [sic] process of his existence paper.5

From the point of view of the history of economic analysis, all this is rather dreary reading, especially if the gossip is not transcended by substantive analyses of the specific contributions. However, the tone of the times being what it is, one has to be prepared for more public washing of linen, that of Nash, Gale, Uzawa, Koopmans, Solow, Kuhn, Isard, and undoubtedly of others.

But Ikeo does make an attempt at substance: an Appendix outlines the proof of Uzawa’s 1962 result; Brouwer’s fixed point theorem, albeit in a special case, is stated; theorems of Eilenberg-Montgomery and Begle discussed. But in this reviewer’s judgement, it does not go far or deep enough. Bracketing yet again the digression of van der

5See pages 143-144. Earlier, the author talks of Nikaido “not talk[ing] much about the project as a whole ... and feel[ing] very uncomfortable with the fact that the project had been financially supported by the ONR.”
Waerden’s algebra (pp. 126-128), or the red herring concerning Kakutani’s contribution to the cleaning-up of the von Neumann-Morgenstern manuscript (p. 133), or the blemish concerning set-to-set transformations (p. 119), the point is that the proofs of Gale, Nikaido and Debreu of their lemma are substantively, and insightfully, different. Debreu in 1959, and Nikaido in 1968 and 1970, have both gone into this in their texts. There is no conspiracy here: this reviewer learnt the KKM lemma and the Nikaido mapping at the hands of Scarf in the Fall of 1971; also see Nikaido (1954). Ikeo writes

McKenzie did not refer to John Nash’s papers in game theory, which were very important for other economists in solving the existence question (p. 136).

But neither did Nikaido in his two texts – Nash is Banquo’s ghost at all these banquets. The relation of Nikaido’s papers to those of Nash, not in terms of the vulgarity of citation counts, but in terms of substance, has yet to be worked out.

Chapters 4 and 5 do break new ground, however. In the first, Ikeo frames Fukuda Takuzo’s 1925 edition of the Lectures on Economics as a turning point in the mathematization of economics in Japan, and then follows Bronfenbrenner in calling Takata Yasuma the Japanese Marshall – presumably on the basis of his 1929-1932 five-volume work New Lectures on Economics. She lists the topics it covers, and notes its distinction of “general equilibrium theory in the manner of Walras from the partial equilibrium theory in the manner of Marshall (p. 78).” Its influence on the Japanese Samuelson (Yasui Takuma), on Sono Masazo and on a host of figures of lesser importance, is ably traced.

Yasui introduced the Walrasian tâtonnement process in the literature of economic stability for the first time in the world – was the first to introduce the word (p. 106, 109). Yasui delightedly stated in 1990, “It is me that introduced Walrasian tâtonnement in the economics literature for the first time (Footnote 14; p. 106).”

Even though introduced in Chapter 2, Yasui’s work in economic dynamics gets fuller treatment in Chapter 5; also see the author’s references to Weintraub and Tsuru Shigeto.

Indeed, the author’s treatment of Yasui’s work suggests a problematic of some sociological and anthropological importance. Why was Yasui’s work of the forties ignored?

Negish had a chance to write a survey article on stability analysis ... and he refrain[ed] from referring to articles written in Japanese such as Yasui’s. It is very hard to specify reasons why Negishi did not (p. 37).

One can couple Morishima with Negish in this regard, and observe that it stands on its head the complaint of Russian mathematicians in the sixties and the seventies as to why their work was read but not cited by Western authors. Indeed, this use of the

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6 There is an unfortunate typo in the title of Nikaido’s 1968 text; see pp. 122 and 257.
7 Negishi (1990) lists his reasons, but only in Japanese. Morishima (1964) does not cite Negishi’s survey.
adjective “Japanese” appended, as if by spasmodic reflex, to Western names leads one to ask what it is about our thinking that sounds discordant and odd if we are to refer to Keynes as the English Takahashi or to Aumann as the Israeli Arrow or to McKenzie as the Rochester Samuelson. Pushing this question of identities and identification, one may ask why Fujisawa Rikitaro or Sono Masazo, constantly identified as mathematicians, do not also merit an economist’s identity. The point is that not unlike native Americans in Hollywood westerns of the fifties, we do not know as much of the adjectivized noun, as the proper noun itself. The author’s strength, and her book’s value, lies in her translations of Japanese texts, and in giving an English voice to those who did not have it. It is the treatment of authors such as Yagi Yoshinosuke and Sugimoto Eiichi that gives Chapter 7 (and Section 3 of Chapter 4) especial interest: the Morishima-Nikaido-Uzawa triumvirate, and the two Michios (Morishima and Hatanaka), already wrote in English.

As regards (iii), the entire book is imbricated with vignettes that go towards embodying what are cardboard/cartoon characters to non-Japanese eyes, and therefore to the general portrayal and embodiment I mentioned above. As Metzler (2009) writes in his review of Smethurst:

If Takahashi, the most influential economic policymaker of 20th-century Japan, was a Keynesian before Keynes, he was also a “Listian before List,” (he argued for protectionist and developmentalist policies before List’s work was translated into Japanese) and a market-oriented developmentalist: Japan’s Friedrich Hayek as well as Japan’s Maynard Keynes.

Thus, for example, Takahashi, Yasui, Bronfenbrenner all also figure elsewhere in the book. But it is really Chapter 8 on Amano Tameyuki, and Chapter 10 on Martin Bronfenbrenner, that I read with absorbed fascination. Ikeo’s identification of the Morishima-Negishi omission of Yasui is not the only problematic worth pursuing: one may also focus on Amano or Bronfenbrenner. Who was Bronfenbrenner primarily working for during the Occupation? What did his employers assign him to do? What was his role in the purge of Ishibashi, in the Shoup (Vickrey) Mission, in drawing up the Dodge line?

I began this review by seeing three books in one, but after having worked through its contents, and reflected on its basic thrust, I now see a more unified framing of the subject, see aspects that are perhaps more implicit than explicit. The author attaches importance of history to theory, recognizes that behind every theorem there are personalities and problematics, and for substantive impact, it must of necessity go beyond its mathematical register to have a say on the policing done by those who have been entrusted with the making of policy. The common objectives of macroeconomics and general equilibrium

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8 As pointed out to me, there was also another Takahashi relevant to this period; see the chapter on Takahashi Kamekichi in Hoston (1986). I thank Tatsuo Hatta for this pointer; and should also like to take this opportunity to thank Maia Linask and Nobusumi Sagara for their careful reading of an earlier version of this review.

9 Also see Ikeo (2008), a book dedicated to Bronfenbrenner, and Nolte (1986).
theory, recognized in Section 4 of Chapter 4, and a discussion of issues of monetary policy before launching into homotopy or Lyapunov theory, is the forging of a methodological road that ought to find important wayfarers and fellow-travelers.

If there is any criticism of this book, it has to be in its exclusions: turnpike theory, the univalence problem, the two-sector model, surely subjects that are nothing if not Japanese. But then this reviewer had to remind himself that the task of a historian is never finished: there is more to be covered, always more to included, and to be further uncovered in what is already included. But this book is surely a worthwhile and pioneering beginning as far as the history of Japanese economic science is concerned.

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