Metatheory as the key to understanding: Schumpeter after Shionoya

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In his recent book, Yuichi Shionoya departs from previous Schumpeterian studies. He sets out to reconstruct Schumpeter’s metatheoretical framework, which he argues is the key to a correct interpretation of his substantive writings. The centrepiece of this project is the thesis that Schumpeter has a coherent methodological position—instrumentalism—which consistently underpins his theoretical work. The present paper examines this thesis, and in so doing paves the way for an alternative hypothesis on Schumpeter. This hypothesis is based on Tony Lawson’s work on the philosophy of economics.

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JEL classifications: B3, B4

1. Introduction

You are somewhat confused. Having consulted Schumpeter’s major works and the literature devoted to his writings, you find (a) that it is often the case that interpreters present contrasting views on him and (b) that almost invariably there exists textual support for their contrasting theses. In fact, you are beginning to wonder whether you are ever going to be able to understand Schumpeter. This could demand that you study his entire work—a considerable enterprise, at the end of which it could be that you remain confused.

As it happens, this enterprise has meanwhile been undertaken by Yuichi Shionoya. In his Schumpeter and the Idea of Social Science (1997), which builds on several of his papers, he argues that attempting to interpret Schumpeter on the basis of fragments of his oeuvre involves the risk of a misunderstanding. ‘[R]ather than reading him in snatches’, it is necessary to ‘consider all of his work’ (ibid., p. xi)—not least his metatheoretical writings. Shionoya’s book is actually subtitled A Metatheoretical Study, for he endeavours to show that Schumpeter’s metatheoretical framework provides the key to understanding the underlying unity and real significance of his scientific achievements.¹

¹ Shionoya has meanwhile returned to Schumpeter, along the same lines. See his papers in Shionoya (ed.) (2001).
The centrepiece of this project is Shionoya’s attempt to demonstrate Schumpeter’s consistent adherence to a particular version of instrumentalism. In Section 2, I consider the scope and meaning of this thesis, decomposing it into a series of claims. I then proceed to examine these claims (Section 3). I conclude that Shionoya does not succeed in establishing what he aims to establish and, indeed, that he is sometimes simply wrong. This critique paves the way for an alternative route to understanding Schumpeter’s discourse. A very different hypothesis on Schumpeter is put forward (Section 4), and its contribution to rendering his mercurial work intelligible is assessed, albeit incompletely (Section 5). This hypothesis, too, is of a metatheoretical nature. It is influenced by Tony Lawson’s writings, which are at the centre of critical realism (see Lawson, 1997). Brief concluding comments follow in Section 6.

2. Shionoya’s thesis: context and meaning

2.1 Preliminaries

Shionoya’s book endeavours to reconstruct Schumpeter’s metatheoretical framework with a view to clarifying the meaning of his substantive theories. This metatheoretical framework is presented as a system comprising the methodology of science, the sociology of science and the history of science; whereas Schumpeter’s substantive theory is argued to consist of three blocks—economic statics, which turns on general equilibrium theory; economic dynamics, which builds on statics and attempts to encompass innovation and its implications; and economic sociology, which focuses on institutions and their evolution, attempting to integrate theory and history.

These definitions cannot convey the complexity of Shionoya’s plan. But they are indicative of its scope. In fact, Schumpeter connoisseurs will have no doubt that it is impossible to do justice to an argument of this breadth—a fortiori to any subtleties, of which there are many in Shionoya’s book—within the space available. For my purposes, however, it suffices to concentrate on a single, original element of Shionoya’s argument, which is ultimately decisive for the success of his book. I refer to the claim—for which I henceforth reserve the label ‘Shionoya’s thesis’—that Schumpeter is an instrumentalist.

The centrality of this thesis within Shionoya’s book is apparent from the fact that it is repeatedly retrieved in order to illuminate Schumpeter’s theories, to the effect that all of Schumpeter’s substantive work is ultimately claimed to have an instrumentalist cast. In addition to this, Schumpeter is argued to defend instrumentalism in his metatheoretical writings.1 Consider the following passages:

Chapter 5 takes up [Schumpeter’s] maiden work, Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie, and interprets his methodology as instrumentalism, which was first developed to lay a foundation for neoclassical economics but later functioned as an anchor in his attempt to construct a universal social science designed for the integration of theory and history. (Shionoya, 1997, pp. 9–10)

When we leave Wesen and come to dynamic theory and the analysis of historical development in Schumpeter’s subsequent works, we find him dealing with facts explicitly in terms of statistical and historical concepts . . . But this neither meant, nor was there a reason, that he had to change his instrumentalist view. (Ibid., p. 121)

I shall show that Schumpeter applied his instrumentalist methodology, as forged first for static economic theory, mutatis mutandis to economic sociology . . . (Ibid., p. 196)

1 Schumpeter’s main work on statics is actually a metatheoretical treatise: Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie (Schumpeter, 1908).
The broad contours of Shionoya’s thesis are apparent from these statements. An investigation of its merits requires, however, a more precise formulation. Let us attempt to specify what exactly Shionoya is claiming.

2.2 Shionoya’s claims about Schumpeter’s instrumentalism
Shionoya’s thesis claims that Schumpeter is an instrumentalist in two senses. On the one hand, he is claimed to defend instrumentalism. On the other hand, his substantive work—all of his substantive work—is deemed to have an instrumentalist basis. In other words, Schumpeter is argued to practice consistently the methodology that he preaches—a feature that it would be unwise simply to take for granted.

Shionoya implies, moreover, that recognising Schumpeter’s instrumentalism is the key to solving interpretative controversies and avoiding interpretative mistakes. These mistakes, of which there seem to be quite a few (see, e.g., Shionoya, 1997, pp. 75–6, 82, 243–4), are apparently due to ‘reading Schumpeter in snatches’ and failing to take account of his metatheory. In Shionoya’s words, ‘locating [Schumpeter’s substantive contributions] within our metatheoretical framework will clarify the logical connection between them and dispel existing misinterpretations about them’ (ibid., p. 10). Accordingly, it is incumbent upon Shionoya to demonstrate that, by interpreting Schumpeter as an instrumentalist, one exposes, and avoids, flaws that blemish Schumpeterian exegesis.

Finally, there is a third claim implicit in Shionoya’s thesis. His aim is to unveil the underlying unity and consistency of Schumpeter’s work. ‘In Schumpeter’s system’, Shionoya (ibid., p. 310) writes, ‘statics, dynamics, and economic sociology are coherent, though they are concerned with different problems and methods.’ The key to grasping this coherence is Schumpeter’s metatheory. It follows, then, that the latter must be coherent. This implies, in turn, that Shionoya regards instrumentalism as a coherent position. In fact, Shionoya himself seems to be committed to instrumentalism. ‘My interpretation’, he writes (ibid., p. xii), ‘might be criticized for being too well ordered and artificial. Yet in science theoretical models imposed on reality are always systematic and artificial creations of the human mind, and both the depiction and the reconstruction of theories and thought are in themselves theoretical activities.’

To sum up, Shionoya’s thesis encompasses three claims, which can be summarised as follows:

Proposition 1: Schumpeter is an instrumentalist, and consistently so.
Proposition 2: Recognising Schumpeter’s instrumentalism is the key to understanding his theories.
Proposition 3: Schumpeter’s instrumentalism is a coherent position.

We have already gone some way towards describing the content of these propositions. It remains to specify Shionoya’s conception of Schumpeter’s instrumentalism.

2.3 Shionoya’s conception of Schumpeter’s instrumentalism
In the minds of economists, instrumentalism probably recalls Milton Friedman’s irrepres- sible 1953 essay; and instrumentalism is typically regarded as the conception that theories are neither true nor false but only convenient ways of generating successful predictions logically. Shionoya dissents. To begin with, he argues that it is Schumpeter—not Friedman—who should be credited with bringing instrumentalism into economics. But he adds that it is inappropriate to restrict instrumentalism to its currently more usual
meaning. Instrumentalism, Shionoya observes, entails that theories are nothing but tools, but not that they must be computational rules for prediction; and it is in this broader sense that Schumpeter is an instrumentalist:

[I]t is more appropriate to interpret [Schumpeter’s] point of view in light of the broad conception of instrumentalist philosophy that was held by his contemporaries and by which he was actually affected. Hence we should understand the central claims of instrumentalism as the belief that, first, with regard to the role of theories, they are merely tools, and second, with regard to the cognitive status of theories, they are neither true nor false. (Shionoya, 1997, p. 98)

The instrumental roles of a theory . . . include organization, classification, reconstruction, and—through all these efforts—the understanding of otherwise chaotic facts. For Schumpeter, the latter roles were much more important than prediction . . . It was from this standpoint of moderate instrumentalism that Schumpeter opposed . . . the conception of prediction as the ultimate test of a theory. (Ibid., p. 122)

As is apparent, the first passage above implies that there are some subsidiary claims of instrumentalism. And Shionoya goes on to argue that some, but not all, variants of instrumentalism are opposed to realism—a rather problematic point.

Schumpeter’s position, however, is suggested to be anti-realist (see, e.g., Ibid., p. 110).

In short, Shionoya’s definition of Schumpeter’s alleged stance can be accepted in that it incorporates the constitutive traits of instrumentalism as traditionally understood in philosophy, i.e., the conception that theories are nothing more than tools, hence neither true nor false. We can now proceed to examine his thesis.

3. Diagnosing Shionoya’s thesis

3.1 On instrumentalism as the key to understanding Schumpeter²

As remarked, Shionoya sometimes points to errors and confusions in Schumpeterian exegesis, which the recognition of Schumpeter’s metatheoretical position is supposed to dispel. One of these interpretative errors concerns Schumpeter’s thesis that the capitalist process has a tendency to self-destruction. In Shionoya’s opinion,

Schumpeter’s] thesis that capitalism will decline because its economic success will lay the groundwork for social circumstances unfavorable to it should not be interpreted as historical determinism. It has nothing to do with a historical hypothesis or prediction; rather, it is a theoretical hypothesis derived from certain assumptions about the interaction between economic and social factors, and its validity rests on the instrumental roles [of theories] in understanding reality. (Shionoya, 1997, p. 220)

The claim that Schumpeter’s thesis on the self-destruction of capitalism is not a deterministic prediction is actually true. Clearly, though, Schumpeter could be an instrumentalist if this claim were false. And the fact that it is true does not require that Schumpeter be -

¹ Shionoya (1997, p. 99) writes that some instrumentalists ‘admit that some theoretical entities are real. This is the reason why the difference between instrumentalism and realism is sometimes viewed as blurred’. Yet this difference is hardly blurred (see Lawson, 2000). The implicit or explicit acceptance that theories, or theoretical entities, can be considered true or false does not in itself imply a (realist) commitment to truth. Indeed, it may even be possible to define instrumentalism as the thesis that falsity is irrelevant to the theoretical enterprise.

² My examination of Shionoya’s thesis begins with Proposition 2 because this proposition is relevant to assessing the truth of Proposition 1.
Metatheory as the key to understanding committed to instrumentalism.¹ This case, then, provides no grounds to accept that the recognition of Schumpeter’s (alleged) instrumentalist orientation dispels misinterpretations of his substantive theories.

Now, the most obvious difficulties in Schumpeterian exegesis ultimately turn on his repeated, and not merely rhetorical, invocation of both Walras and Marx. Schumpeter is committed to general equilibrium theory, and yet innovation, or creative destruction, and capitalism’s structural transformation are his main topics. This has always puzzled Schumpeterians. If there is one issue that we would want Shionoya to explain, then, it is certainly this one. He should convince us that Schumpeter’s apparent dualism is intelligible—i.e., does not yield an inconsistency—once we recognise his adherence to instrumentalism. This is what Shionoya offers in his most elaborate comment on Schumpeter’s commitment to Walras and Marx:

More than a few authors writing about Schumpeter have criticized the paradox and inconsistency in his admiration for, and indebtedness to, both Walras and Marx. This criticism is rooted in the popular misinterpretation . . . that fails to understand the coordination of statics and dynamics in Schumpeter’s thought; it is no wonder that this misinterpretation is now extended to relations between economic and noneconomic areas in a wider perspective . . . Social events are related to each other, not only simultaneously but also intertemporarily. Simultaneous relationships are the subject of the Walrasian general equilibrium theory; intertemporal relationships are the theme of the Marxian theory of evolution. Both relationships are necessary . . . to explain any event at any point in time . . . The apparent contradiction [in Schumpeter] is refuted by the idea, based on historical experience, that the very success of the capitalist economy will produce noneconomic factors that are inconsistent with it . . . Accounting for the fact that the changes in noneconomic factors are the result of economic development, we can assume a grand general equilibrium between the economic and noneconomic spheres and its evolution over time. This was Schumpeter’s integrated vision . . . (Shionoya, 1997, p. 82)

But this, I think, is hardly what we are entitled to expect. Surely this ‘grand general equilibrium’—or ‘enlarged version of the general equilibrium analysis’, as Shionoya (1986, p. 759) previously called it—has nothing to do, and is incompatible, with Walrasian general equilibrium? And, in any case, where is instrumentalism? Elsewhere in the book Shionoya gives a clue:

[T]here is the often repeated criticism that [Schumpeter’s] simultaneous acceptance of Walras’s idea and Marx’s idea is a contradiction. [But i]t is quite natural that a theoretical structure differs according to the objectives and problems of researchers. It is unnecessary from a methodological standpoint to demand that all theories be monolithic even if they are entertained by one person. To use Schumpeterian rhetoric, does it cause any inconvenience or contradiction that one has a key for a room and another key for a car? Because only the usefulness of instruments is important, no one thinks that one must have one master key. (Shionoya, 1997, p. 310)

But this statement is somewhat at odds with the one quoted before. And if it is intended to suggest that theories are just tools—and therefore it is legitimate to resort to Walras for

¹ Consider the following passage, which Shionoya quotes: ‘Analysis . . . never yields more than a statement about the tendencies present in an observable pattern. And these never tell us what will happen to the pattern but only what would happen if they continued to act as they have been acting in the time interval covered by our observation and if no other factors intruded. “Inevitability” or “necessity” can never mean more than this’ (Schumpeter, 1942, p. 61). It is at least conceivable that this statement means (a) that capitalism is subject to various tendencies—which, if they persist and fully work themselves out, would bring about a particular observable pattern; (b) that these tendencies need not persist nor work themselves out, in that they may be counteracted by other tendencies; and (c) that, whatever observable outcome ensues, these tendencies are real because they impact upon empirical reality and are not invented by Schumpeter.
some purposes, and to Marx for other purposes—then it misses the target. For, even if instrumentalism allows for mutually inconsistent theories, it presumably requires that they be internally consistent; and it is emphatically not the case that some of Schumpeter’s writings are consistently ‘Walrasian’ and others consistently ‘Marxian’. What renders Schumpeterian exegesis most difficult is precisely his proclivity to be inconsistent within the same piece.

There are several instances of this but I shall select just one. As is well known, Schumpeter’s *Theory of Economic Development* (Schumpeter, 1912, 1934) provides an account of the emergence of innovators, or entrepreneurs, who disrupt the routine-based ‘circular flow of economic life’. Yet this circular flow is identified with a competitive equilibrium system (which, the absence of interest apart, is basically Walrasian); and this equilibrium system seems to rule out the possibility of real novelty, i.e., to emasculate Schumpeterian entrepreneurship. How a creative entrepreneur is to emerge in such a scenario as Schumpeter fashions is something of a mystery.

Now, can instrumentalism serve to demonstrate that this apparent inconsistency is actually an optical illusion? I am not convinced—particularly since Shionoya does not really discuss this issue. The verdict must be that Proposition 2 is unsubstantiated.

3.2 On Schumpeter as an instrumentalist

The fact that Proposition 2 is unsubstantiated makes it harder for Shionoya to defend Proposition 1. But he attempts to validate the latter following yet another route. He argues that Schumpeter advocates instrumentalism in his writings on the methodology of economics (first and foremost *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (Schumpeter, 1908)) and on the methodology of economic sociology (primarily *Gustav v. Schmoller und die Probleme von heute* (Schumpeter, 1926)). Since theorists do not necessarily follow their methodological prescriptions, these pronouncements are insufficient to establish that Schumpeter is—or is not—an instrumentalist. But they can be suggestive. Consider the economic methodology implied in the following passages:

If mechanics had wanted to satisfactorily answer what ‘force’, ‘movement’, ‘mass’, etc. actually ‘are’, then the proud edifice which we now admire could never have been erected. (Schumpeter, 1908, p. 24)

One usually contrasts explanation and description, and demands that theory find the ‘causes of facts’, and the ‘forces’ and ‘laws’ that ‘rule’ them. But, on closer observation, it is easy to convince oneself that the core of any theory, *what it truly says*, is always and only a statement about functional relationships between some quantities; all the rest is . . . unessential. This is most evident in those sciences that are farthest away from speculation, the exact natural sciences, and has found its clearest expression in Kirchhoff’s famous definition of mechanics. To find . . . causes is impossible for us, but we do not need them either in order to arrive at concrete results. (*Ibid.*, pp. 37–8)

1 I return to this inconsistency below. But, as I say, there are other examples of ‘internal inconsistencies’. Thus, Schumpeter’s quite Austrian argument on competition as a process in *Capitalism, Socialism and Democracy* (Schumpeter, 1942) is irreconcilable with his positivist analysis of the economics of socialism in the same book. In addition to this, Schumpeter is inconsistent in yet another way. His comments on the ontological status of equilibrium (see below)—or on perfect competition—vary in the course of his work. Yet it is not the case that he revises his stance for good at some point; rather, he oscillates between irreconcilable positions. Naturally, inconsistencies such as these entail that contrasting claims about Schumpeter can be easily documented on the basis of quotes. ‘With Schumpeter’, as Stephan Boehm (1990, p. 233, n. 19) puts it, ‘that is hardly ever a problem’.

2 Translations from *Wesen* are my own.
The conception of theory defended here does seem to point towards Shionoya’s thesis.1 There are, however, some complications. Compare the following excerpts:

The explanation that our theory provides is a description of functional relations between the elements of our system by means of formulas as concise and general as possible. *These formulas we now call ‘laws’. (Schumpeter, 1908, p. 43)*

We do not philosophise about what must be because of some ‘necessity’ but instead describe what in many cases *is*. In so doing we expect that the same *be* in other cases which we have not observed . . . *(Ibid., p. 44)*

 *[T]here are no grounds to refer to our laws as ‘statements of tendencies’, as Marshall does. What this is intended to mean is simply that circumstances may intervene which lead to results different from those that our laws entitle us to expect. But this is evident . . . Each law in natural science is also subject to this possibility. A stone on a table cannot fall to the ground. If, for this reason, one wants to classify the law of gravity as a ‘description of tendencies’, one may do so: in principle, there is nothing to object. But there is no characteristic in here that would be specific to *our* laws. *(Ibid., p. 45)*

If what the theory predicts does not occur, then the ‘practical person’ simply dismisses the theory as false. This is unjustified. Our laws are at work even then, and the concrete result would be different if they were not. *(Ibid., p. 466)*

In short, whilst Schumpeter puts forward an instrumentalist conception, he also seems to be implying that theories (or ‘laws’), rather than being mere creations of theorists, refer to (causal) mechanisms which *exist* and, even if they are not fully manifest, make a difference. This is a realist stance.

If we turn to the methodology of economic sociology, we find further difficulties. ‘From Schumpeter’s standpoint’, Shionoya (1997, p. 195) writes, ‘if economic sociology is a theory at all, then its epistemological status must be construed as an instrument’. And he adds: ‘Schumpeter’s view was that economic sociology is part of theory . . . , although its subject matter is different from that of economic theory’ *(ibid., pp. 205–6).* In short, because for Schumpeter economic sociology is a kind of theory, and he regards theories as *(mere)* instruments, his economic sociology is instrumentalist.

Yet it is not clear that economic sociology is a theory in the sense that Shionoya’s inference demands. Thus, in his *History of Economic Analysis* Schumpeter refers to ‘theory’ as a ‘box of tools’ (Schumpeter, 1954, p. 15); he argues that history is more important, to begin with because ‘the subject matter of economics is essentially a unique process in historic time’ *(ibid., p. 12)*; he submits that ‘theory’ is nonetheless neither impossible nor useless, because ‘economic history itself needs its help’ *(ibid., p. 13, fn. 3)*; and eventually he brings in ‘economic sociology’, on the grounds that history and ‘theory’, ‘while essentially complementing each other, do not do so perfectly’ *(ibid., p. 20).*

However, he also distinguishes between historical and theoretical hypotheses (see *ibid.*, pp. 14–15)—a distinction already present in *Wesen* (see Schumpeter, 1908, pp. 531–2; Shionoya, 1997, p. 112). The former are claimed to imply a cognition, whereas the latter

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1 Schumpeter (1908, p. 32) actually distinguishes between ‘pure theory’ (*theoretische Ökonomie*) and ‘economic theory’. ‘Pure theory’ is static neoclassical economics, whereas ‘economic theory’ is a broader category, intended to cover ‘dynamic’ phenomena. However, the latter kind of theory is insisted to rest upon the former (see, e.g., Schumpeter, 1912, pp. 511ff.). Statements coherent with those quoted above can be found throughout Schumpeter’s work, up to his *History of Economic Analysis*. See, e.g., Schumpeter (1954, p. 15, 824, fn. 25), where the purely instrumental nature of theory is emphasised.

2 Schumpeter also brings in statistics as a ‘technique of economic analysis’. But that is irrelevant to the present argument.
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are methodological tools only. Since there is no possibility of a compromise between realism and instrumentalism, it follows, then, that the epistemological status of economic sociology—which, somehow, is supposed to serve as a bridge between ‘theory’ and history—becomes obscure.

Shionoya does attempt to strengthen his argument in other ways. In his discussion of Gustav v. Schmoller... he argues that

Schumpeter wanted to put a brake on what might have appeared to be an endless process of data collection in Schmoller’s research program, a bottomless pit into which historical economists were liable to fall. To do so, he had to resort to a methodological perspective... Instrumentalism facilitates deductive attempts even when empirical data are not sufficient according to the [realist] Schmollerian standard... One need not engage in the never-ending process of fact-finding in order to finally develop realistic assumptions. (Shionoya, 1997, pp. 202, 206–7)

Yet this is, again, unacceptable. Shionoya is, amongst other things, confusing realism with ‘realisticness’ (see Mäki, 1994). He is implying that realism entails the belief that assumptions can, and indeed must, be ‘finally... realistic’. And, to the extent that he lapses into this misrepresentation, he is ultimately implying that it is impossible not to be an ‘instrumentalist’—and thus depriving his own thesis of meaning.

It is, I think, unnecessary to persevere. Shionoya does not succeed in showing that Schumpeter’s pronouncements consistently yield a defence of instrumentalism, much less that he is an instrumentalist. This, of course, does not entail that he is not. Is it possible, then, to falsify Shionoya’s claim?

As François Perroux (1965, pp. 45ff.) observes in his critique of Schumpeter’s Theory of Economic Development, if a theory is to have explanatory value, then, starting from the ‘essential economy’ proposed, however abstract, one should be able to reconstruct the ‘concrete economy’. If, on the contrary, this reconstruction attempt leads to contradictions, then the theory is une simple construction de l’esprit. In the latter case, that is, a method or form of reasoning has been adopted independently of, or with insufficient attention to, insights into the nature of things.

Now, as Lawson (1997) explains, a method necessarily presupposes a theory of knowledge, which in turn presupposes a theory of what is—a theory to the effect that what is is knowable by resorting to that method. Ontology is a category that it is impossible to eliminate. Accordingly, an a priori commitment to any method (the logico-deductive method, for instance) may be conducive to a clash with a theorist’s actual world view—and give rise to tensions and inconsistencies.

Tensions and inconsistencies, as remarked, are pervasive in Schumpeter’s writings—which may suggest the existence of such an a priori commitment (we shall investigate this shortly). At any event, the existence of internal tensions and inconsistencies suffices to establish that Schumpeter cannot be consistently instrumentalist.

3.3 On Schumpeter’s instrumentalism as a coherent position

The considerations presented so far suffice, I think, to undermine Shionoya’s thesis. Before developing an alternative hypothesis along the lines just hinted at, however, it is worth establishing that Proposition 3 is also false. And to this end, it is convenient to introduce two contrasting conceptions of the nature of the social world—to distinguish between closed systems and open systems (see Lawson, 1997).

In a closed system, there is no proper distinction between action and structure (understood as ‘conditions of action’). Instead, such a system is characterised by regularities of
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the form ‘whenever conditions $x$ then outcome $y$’. In such a scenario, social order can only rest upon the global coherence of these regularities—whence the necessity of some orthodox notion of equilibrium. Since the future is determined, there is no room for choice in that, if it is to be real, choice cannot be confused with the conditions of its exercise. Real novelty is thus pre-empted; and, relatedly, knowledge is certain and its growth monistic.

In an open system, on the other hand, regularities of the form ‘whenever conditions $x$ then outcome $y$’ do not typically obtain: the future is indeterminate, as it must be if choice is to exist. Since choice presupposes intentionality and knowledgeability, and knowledge requires some endurability in its object, there must exist (relatively enduring) structures that facilitate, and constrain, but are not reducible to, human action. Capacities, then, are not identified with actual outcomes, and real novelty is possible. Social order now rests upon the dynamic interdependence of action and structure and is therefore an order of a transformational, perhaps evolutionary, kind. And knowledge is necessarily precarious and open to revision: ignorance and surprise are pervasive.

In the light of these definitions, it is clear that Friedman’s (1953) position rests on the implicit premise that the social world is a closed system (see Lawson, 1992). Accordingly, the possibility of prediction is taken for granted. And the truth of assumptions does not matter if they yield true predictions. In fact, explanation is not even an option in Friedman’s conception. He assumes that a theory is a hypothetico-deductive system, and correctly implies that one cannot logically infer the truth of its assumptions from the truth of its conclusions (see Boland, 1979). Accepting its premises, then, Friedman’s instrumentalism cannot be criticised on the grounds of coherence.

Schumpeter’s alleged instrumentalism, however, differs from Friedman’s. Schumpeter is categorical that prediction is typically impossible (see, e.g., Schumpeter, 1939, p. 13). But why? What kind of social world is Schumpeter implicitly assuming to reach this conclusion? Given his insistence on novelty and entrepreneurship, it would seem likely that he conceives of the social world as an open system. He certainly does not regard it as a closed system in our sense.

Yet, if this is correct, it appears to be the case that his putative instrumentalist position involves an incoherence. On the one hand, prediction is deemed typically unfeasible; on the other hand, if one is an instrumentalist one cannot legitimately claim an explanatory role for theories, in as much as explanations cannot usefully be based upon arbitrary assumptions. Such ‘explanations’ cannot provide but the pretence of knowledge. But theories which can neither predict nor explain would seem to be useless for the purpose of illuminating the social world or, in Shionoya’s (1997, p. 122) expression, for understanding facts.

4. An alternative hypothesis

The preceding critique points to an alternative (realist) hypothesis on Schumpeter. This hypothesis seeks to make sense of Schumpeter’s writings on the basis of a metatheoretical inconsistency, which is supposed to be reproduced in his substantive work. Whilst Schumpeter conceives of the social world as an open system, he simultaneously accepts a conception of the structure, or form, of scientific theories which presupposes a closed world.

1 Lawson (1997) uses the expression ‘whenever event $x$ then event $y$’.
That he conceives of the social world as an open system seems to follow from his views on entrepreneurship and innovation. Entrepreneurship is argued to involve creative idiosyncrasy, and accordingly innovation ‘cannot be predicted by applying the ordinary rules of inference from the pre-existing facts’ (Schumpeter, 1947, p. 222). Rather, it is a ‘creative response’—where ‘response’ means that entrepreneurship is not a ‘Shacklean leap into the void’ (Boehm, 1990, p. 226). As Schumpeter (1991, p. 409) notes, ‘objective’ conditioning factors ‘have as much claim to being dubbed “causal” as has the action they condition’.1

That he accepts a conception of science which implicitly supposes a closed world—the conception that science requires the framing of regularities of the form ‘whenever conditions $x$ then outcome $y$’—is suggested by his enduring commitment to equilibrium and its ultimate justification:

> [O]ur objects of investigation are certain relations of dependence or functional relations. The fact that economic quantities stand in such relations to one another legitimises their separate treatment provided that they are uniquely determined. The unique determination of a system of quantities is a scientific fact of the utmost importance. It means that, when certain data are given, we have all the necessary elements to ‘understand’ the magnitude of those quantities and their movement. In this case a separate, independent discipline about such phenomena is possible, and this is therefore what must be established before anything else. If a system of equations yields absolutely nothing but the proof of a uniquely determined interdependence, this is already very much: it is the founding stone of a scientific structure. (Schumpeter, 1908, pp. 33–4)

Whoever knows the origin and the workings of the exact natural sciences knows also that their great achievements are, in method and essence, of the same kind as Walras’. To find exact forms for the phenomena whose interdependence is given us by experience, to reduce these forms to, and derive them from, each other: this is what physicists do, and this is what Walras did. (Schumpeter, 1910, p. 79)

Lest [the reader] should . . . turn away from Walras’ construction on the ground of its hopeless discrepancy from any process of real life, I wish to ask him whether he ever saw elastic strings that do not increase in length when pulled, or frictionless movements, or any other of the constructs commonly used in theoretical physics; and whether, on the strength of this, he believes theoretical physics to be useless. (Schumpeter, 1954, p. 1015)

Let us analyse this justification. This is quite important in that my hypothesis may prima facie appear eccentric. I am proposing, after all, that Schumpeter is committed to equilibrium not because of but despite his Weltanschauung. Yet it is quite comprehensible that Schumpeter accepts the conception that science rests on regularities of the form ‘whenever conditions $x$ then outcome $y$’—a conception in which equilibrium, or the global coherence of a set of functional relationships, is the only conceivable notion of order. For his acceptance of this conception does not result from sustained reflection on the nature of the social world. Rather, Schumpeter believes that this conception of the structure of science is the conception of the ‘true’ sciences.

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1 Or, from a different angle: ‘Manifestly, the captured surplus value does not invest itself but must be invested. This means on the one hand that it must not be consumed by the capitalist, and on the other hand that the important point is how it is invested. Both factors lead . . . from the objective to the subjective . . . [T]he crucial factor is that the social logic or objective situation does not unequivocally determine how much profit shall be invested, and how it shall be invested, unless individual disposition is taken into account’ (Schumpeter, 1927, p. 242). Interestingly, Schumpeter writes (in a comment on Marschak, published in Stolper, 1994, p. 375) that his system is ‘ex ante indeterminate and in this sense “open” . . . [T]here is something of evolution créatrice about it (as there is . . . about every true evolution, a biological “sport” for instance or de Vries’ “mutation”).’
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In these sciences, exact regularities are non-empirical, and yet this has not precluded practical success. ‘Many a historian . . . who is so critical of [our] theory’, Schumpeter (1908, p. 559) submits, ‘would think just the same about exact mechanics, were it not for . . . its connection with the achievements of modern technology.’ Accordingly, the theorems of ‘pure economics’ should be regarded as ‘interesting scientific results and very promising beginnings’ (ibid., p. 579); but, as in physics, it is unwarranted to expect that they should yield immediate ‘practical gains’.

Yet is it true that ‘pure economics’ is on a par with the natural sciences? Is it correct to identify laws of nature with exact regularities? An affirmative answer would entail that laws of nature are created by natural scientists; which makes it incomprehensible why those laws should have facilitated any achievements. After all, natural scientists do not create the natural world.

Where, then, lies the confusion? As Lawson (1997, pp. 29–30) explains, it is true that exact regularities in natural science are, typically, not a spontaneous occurrence but a result of human contrivance. As a rule, they obtain under experimental conditions. However, scientific experiments do not constitute laws. Instead, they ought to be understood as attempts to isolate, and so facilitate the empirical identification of, laws which are at work independently of human intervention—mechanisms which go on acting both inside and outside the engineered conditions under which their empirical identification occurs. For on this conception, but not on the one that Schumpeter is making his own, the applicability of scientific laws outside experimental situations is intelligible (see ibid., pp. 27ff.).

Schumpeter, in sum, is misunderstanding the epistemic significance of experimental situations. From the observation that exact regularities in natural science are non-empirical, and yet have facilitated practical achievements, he is inferring that social science must rest upon exact regularities. He is overlooking that exact regularities in natural science are non-empirical but refer to real mechanisms, whereas those he wants to construct are not just non-empirical but unreal.

Of course, the misunderstanding is not specifically Schumpeter’s. It was propagated by the philosophy of his time. He can be charged only with rather uncritically (a priori) accepting the conception that science supposes the framing of regularities of the form ‘whenever conditions $x$ then outcome $y$’—as just about every economist did. In fact, tensions in the work of some of his contemporaries have been explained on the basis of a similar mismatch between their world view and the ontology implicit in their conception of science (see, e.g., Fleetwood, 1995; Pratten, 1994, ch. 2).

Let me emphasise, finally, that this mismatch has nothing to do with an acceptance or rejection of instrumentalism as a philosophy. What matters is that Schumpeter ends up defining economic theory in terms of a particular structure of explanation: ‘whenever conditions $x$ then outcome $y$’. In his words, a (theoretical) ‘explanation’ is ‘nothing but the specification of a uniquely determined magnitude for our unknowns and of their laws of motion’ (Schumpeter, 1908, pp. 340–1). This structure of explanation is sometimes defended on instrumentalist grounds and sometimes not—which in itself suggests that Schumpeter accepted it largely a priori, and strengthens the plausibility of my hypothesis.

1 Incidentally, this is why the differences between Schumpeter and Menger are ultimately less significant than Shionoya (1997, pp. 115ff.) believes. Schumpeter, unlike Menger, cautions against dabbling in ‘metaphysical’ issues. Yet he accepts the same structure of science as Menger, and his arguments on why an ‘exact orientation’ should be favoured likewise reduce to the assertion that it has proved successful in natural science.

2 Schumpeter recognises the fictitious, instrumental nature of equilibrium, but he also writes that ‘this mechanism for establishing or reestablishing equilibrium is not a figment devised as an exercise in the pure logic of economics but actually operative in the reality around us’ (Schumpeter, 1939, p. 47).
5. An incomplete assessment

A proper assessment of my hypothesis cannot be offered here. This would involve demonstrating that there is a pattern to Schumpeter’s tensions and inconsistencies throughout his entire work, and that this pattern is explicable on the basis of his metatheoretical stance. A brief illustration will nevertheless be sketched, focusing on the opening stages of Schumpeter’s *Theory of Economic Development*—a book marked by a ‘constant tension’ between an approach ‘in which the observed nature of reality is kept to the fore’ and ‘the logico-deductive approach’ (Oakley, 1990, p. 57).

I shall first provide some background notes on the *Theory*. Then I attempt to show that there are two mutually inconsistent models mixed up in Schumpeter’s text. One of them refers to an open and structured system, the other appears to rest on an *a priori* commitment to an inadequate conception of science—a commitment that leads him to frame his conception of capitalism in terms of a closed system.

### 5.1 Some background notes on Schumpeter’s ‘Theory’

Schumpeter’s *Theory* rests on the opposition of two categories: the ‘circular flow’ and ‘development’. The circular flow represents adaptive behaviour. Development, in contrast, is ‘spontaneous and discontinuous change in the channels of the flow’ (Schumpeter, 1934, p. 64). It is the carrying out of innovations, by entrepreneurs, encompassing the introduction of new goods and production methods, the discovery of new markets and sources of materials, and the implementation of new forms of industrial organisation.

Secondly, the opposition of these two categories is suggested to encapsulate the difference between acting within a framework of customs and traditions, on the one hand; and deliberately changing that framework, on the other. The circular flow is founded upon custom and experience, whereas development ‘consists precisely in breaking up old, and creating new, tradition’ (*ibid.*, p. 92).

Thirdly, there is another dimension to the distinction between ‘continuous’ adaptation and ‘discontinuous’ innovation. According to Schumpeter, the circular flow ‘describes economic life from the standpoint of the economic system’s tendency towards an equilibrium position’ (*ibid.*, p. 62); whereas development is ‘that kind of change arising from within the system which so displaces its equilibrium point that the new one cannot be reached from the old one by infinitesimal steps’ (*ibid.*, p. 64, fn. 1).

Fourthly, Schumpeter’s two basic categories, the circular flow and development, are not exclusive to capitalism. However, it is impossible to conceive of capitalism apart from development. This is because, for Schumpeter, the constitutive characteristics of capitalism are related to its *particular* method of generating development. In his *Theory of Economic Development*, Schumpeter focuses on this particular method.

In so doing, however, he does not want to address capitalist development as it is empirically manifest. He acknowledges that ‘[e]very process of development creates the prerequisites for the following’, so that ‘things will turn out differently from what they would have been if every concrete phase of development had been compelled first to create its own conditions’ (*ibid.*, p. 64). But he insists that, ‘if we wish to get at the root of

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1 A very preliminary attempt at such an assessment has been made in Graça Moura (1997).

2 I quote from the English translation of the second edition (Schumpeter, 1934). The original (1912) edition is different in some respects. For the limited present purposes, however, there seems to be no clear advantage in using the original German text.
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the matter, we may not include in the data of our explanation elements of what is to be explained’ (ibid.). He therefore starts his explanation of capitalist development from a circular flow that has not been affected by antecedent capitalist development. And this circular flow—which refers to a market economy rather than to Schumpeterian capitalism—carries no historical meaning (see, e.g., ibid., pp. 10, 245).

Finally, this circular flow is ultimately taken as a downright changeless system. Schumpeter submits that the assumption of stationarity ‘recommend[s] itself’ in that ‘one can exhibit the fundamental form of the economic course of events with the maximum simplicity in an unchanging economy’ (ibid., pp. 82–3, fn. 1).

5.2 Problems and tentative explanations: a sketch

For Schumpeter, in sum, entrepreneurship entails the ability to transcend and transform custom and tradition by exercising creative capacities. On the other hand, the immanent tendency of the circular flow is to reach a position of equilibrium. In order to highlight the ‘essence’ of development, Schumpeter actually assumes that the economy is in a full-employment equilibrium.

Under such conditions, innovation can consist only in ‘the different employment of the economic system’s existing supplies of productive means’ (ibid., p. 68), and entrepreneurs must command the power to draw these means from their traditional uses. But there are no reservoirs of purchasing power. Indeed, money ‘adds nothing new’ to the circular flow, and ‘nothing essential is overlooked in abstracting from it’ (ibid., p. 51). Innovation requires, then, the creation of purchasing power ex novo, which entails that money becomes essential (see, e.g., ibid., p. 71). And this method of financing innovation—the only one . . . available in strict logic’ in the absence of previous development (ibid., p. 72)—is capitalism’s differentia specifica. Significantly, the fund out of which the entrepreneur pays for productive means, outbidding traditional producers, is called ‘capital’.

Yet Schumpeter’s notion of equilibrium, whatever its peculiarities, is orthodox: it yields closure. As such, it rules out choice, creativity and entrepreneurship. How, then, is the entrepreneur to emerge in the first place? How is development conceivable?

In order to understand this, let us simply assume away Schumpeter’s conspicuous invocation of equilibrium mechanisms. As will be apparent, there are fragments left which enable us to reconstruct a conception of development.

According to this conception, individuals typically act on the basis of certain signals, the interpretation of which is facilitated by rules and traditions embodying knowledge that individuals do not consciously command. Long experience, in part inherited, is argued to have taught the individual (see ibid., pp. 5–6), who acts ‘according to certain symptoms of which he has learned to take heed’ (ibid., p. 21). ‘He acts in the ordinary daily round according to general custom and experience’ (ibid., p. 39). Still, there is always a degree of deliberation in tradition-based, everyday behaviour:

[T]he necessity of making decisions occurs in any work. No cobbler’s apprentice can repair a shoe without some resolutions and without deciding independently some questions, however small. The ‘what’ and the ‘how’ are taught him; but this does not relieve him of the necessity of a certain independence. When a worker from an electrical firm goes into a house to repair the lighting system, even he must decide something of the what and the how . . . Now the director or independent owner of a business has certainly most to decide and most resolutions to make. But the what and the why are also taught him. (Ibid., pp. 20–1)

Individuals, however, are capable of breaking up old, and creating new, traditions. They can act as entrepreneurs. Yet this kind of action necessitates a higher level of reflection
than everyday behaviour. It implies transcending, hence becoming consciously aware of, rules which, ordinarily, are not reflected upon:

[O]utside these accustomed channels the individual is without those data for his decisions and those rules of conduct which are usually very accurately known to him within them. Of course he must still foresee and estimate on the basis of his experience. But many things must remain uncertain, still others are only ascertainable within wide limits, some can perhaps only be 'guessed' . . . There will be much more conscious rationality in this than in customary action, which as such does not need to be reflected upon at all; but this plan must necessarily be open not only to errors greater in degree, but also to other kinds of errors than those occurring in customary action . . . Carrying out a new plan and acting according to a customary one are things as different as making a road and walking along it. (Ibid., pp. 84–5)

And the human mind is not powerful enough constantly to work at the level of awareness which entrepreneurship requires. '[E]very man would have to be a giant of wisdom and will, if he had in every case to create anew all the rules by which he guides his everyday conduct' (ibid., p. 83). Thus these rules will be relatively enduring; and this stability is a condition of entrepreneurship.

How entrepreneurs operate depends, naturally, on the institutional setting which, in turn, they contribute to transforming. In any case, however, entrepreneurial action, and hence development, are not exclusive to capitalism. They can occur independently of ad hoc credit creation. For there always exist innovative opportunities, which can be carried out depending on the availability of savings, etc.¹ As a market economy must be a money economy (see, e.g., Schumpeter, 1917), the emergence of capitalism in the Schumpeterian sense involves no analytical break.

Of course, the transformation of banks into producers of purchasing power is hardly a detail. This institutional meta-innovation has a profound impact on the morphology of development. But we need not discuss this here. The above considerations suffice to suggest that Schumpeter has a model in mind, which rests on a conception of the social world as open and structured and is coherent.

The problem is that he superimposes upon this conception a framework presupposing closure. There is a very different, more explicit model in the opening stages of Schumpeter's Theory. In this model, adaptive action founded upon custom and experience is made equivalent to a particular kind of rational conduct: '[i]n so far . . . as it is a question of adapting himself to the conditions and of simply complying with the objective necessities of the economic system without wishing to change them, one and only one particular way of acting commends itself to the individual, and the results of this action will remain the same as long as the given conditions remain the same' (Schumpeter, 1934, p. 40). Indeed, 'in practice people act in accordance with well-tried experience, and . . . in theory we regard them as acting in accordance with a knowledge of the best combination of present means under the given conditions' (ibid., p. 42).

The wages and rents of these people are determined by the marginal productivities of labour and land, whereas the prices of products are equal to the prices of the services of labour and land embodied in them. Production flows profitlessly because, if a surplus over the value of the services of labour and land accrues to employers, employees switch to the task of combining productive resources in accordance with the prevalent techniques.

¹ The circular flow admits of improvements which may, eventually, 'make a great department store out of a small retail business' (Schumpeter, 1934, p. 62), and there is no reason why the implementation of such changes should not be considered an entrepreneurial act.
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Since produced means of production are ‘accumulated productive forces which can at any moment and without loss or friction be turned into any specific commodities wanted’ (ibid., p. 10, fn. 2), ‘innovation’ is reduced to new combinations of (perfectly adaptable) ‘land’ and ‘labour’, and seems to become a trivial ‘action’ indeed—particularly if the ‘entrepreneur’ has ‘the capacity of seeing things in a way which afterwards proves to be true’ (ibid., p. 85).

In short, it is possible to detect, albeit in fragmentary form, two irreconcilable models that appear mixed up in Schumpeter’s Theory. What can explain this? In particular, what is the motivation for his closed system framework? Clearly, Schumpeter’s crucial insights—on variable levels of consciousness in human action, on the interdependence of individual creativity and institutional arrangements, on the essential nature of money and the specifically capitalist role of the banking system—cannot have their source in this equilibrium framework. This suggests that this framework is not so much a product of ontological reflection as of a priori methodological preferences: that it is the product of a preconception of how what is must be fashioned so as to qualify as science.

Schumpeter’s rationalisations of his own style of theorising certainly point in that direction. As already remarked, he argues that, ‘if we wish to get at the root of the matter, we may not include in the data of our explanation elements of what is to be explained’ (ibid., p. 64), and proposes to offer an account of development as it would arise out of a position without development. He wants to describe capitalist development starting from the logical, rather than historical, absence of the constitutive characteristics of capitalism. And this is surely not an ontologically grounded move. In fact, it cannot result but of a preconception of how to frame scientific results—a preconception causing unnecessary, but in Schumpeter’s case hardly avoidable, tensions. For, in as much as a phenomenon is truly new, its emergence cannot possibly be accounted for on a purely logical basis. This is why Schumpeter cannot explain how entrepreneurs, or capitalism, are to arise out of equilibrium.

6. Concluding comments

I have argued that Shionoya’s thesis is unsuccessful. The alternative hypothesis put forward is, like Shionoya’s, of a metatheoretical nature. But, as I now want to emphasise, it rests on an interpretative strategy distinct from Shionoya’s—and from the strategy underlying most Schumpeterian interpretations.

As observed earlier, it is easy to find irreconcilable exegeses of Schumpeter. Clearly, then, his work must exhibit tensions and inconsistencies; otherwise it is difficult to see how interpretative controversies could be so conspicuous. Yet the existence of tensions and inconsistencies does not necessarily mean that Schumpeter’s writings license a plethora of contrasting exegeses. It is at least conceivable that these interpretative contrasts be due to methods of exegesis that cannot adequately address the kaleidoscopic character of Schumpeter’s text.

And, as I see it, this is indeed the case. Many interpretations of Schumpeter rest upon the implicit assumption that there exists a Schumpeterian essence, which can be disentangled from random ‘noise’—even though the persistence of disagreement suggests that this assumption is not well founded. Facing Schumpeter’s mercurial text, interpreters have little difficulty in finding support for a variety of putative Schumpeterian ‘essences’.

In contrast, I reject the implicit postulate that tensions and inconsistencies must be of peripheral importance, and attempt to make sense of Schumpeter’s discourse.
by explaining why these tensions and inconsistencies obtain. This project rests upon the conception that inconsistencies, to the extent that they are interesting, are due to the resilience of discourses which imply different ontologies; and upon the theory that this enduring coexistence of contrasting ontologies can originate not so much in the fact that science is an ongoing, fallible project to understand the world as in a preconception of the structure, or form, of scientific theories. In accordance with this theory, I have proposed that the tensions and inconsistencies in Schumpeter’s writings reflect his inability to discard a conception of science the ontological presuppositions of which are not congenial to his Weltanschauung.\(^1\)

As for Shionoya’s argument, it represents a third strand in Schumpeterian interpretation. He argues that Schumpeter’s metatheory provides the key to solving the confusions that his interpreters have propagated, and denies that Schumpeter is inconsistent. Thus, to repeat, Shionoya (1997, p. 10) promises that locating Schumpeter’s substantive theories within his metatheoretical framework ‘will clarify the logical connection between them and dispel existing misinterpretations about them’; and he insists that ‘[i]n Schumpeter’s system, statics, dynamics, and economic sociology are coherent, though they are concerned with different problems and methods’ (ibid., p. 310).

In order to avoid misunderstandings, finally, it is perhaps not superfluous to contrast my hypothesis with Shionoya’s with regard to the importance of Schumpeter’s work. Prima facie, my hypothesis may appear to be more critical of Schumpeter than Shionoya’s. Yet does this impression stand up to scrutiny? Shionoya attributes to Schumpeter an instrumentalist position that has been severely criticised—even in its strongest, ‘predictivist’ version—though Shionoya is sympathetic to that position. My argument, on the other hand, emphasises Schumpeter’s enduring and a priori commitment to an inadequate conception of science. But is it really surprising that Schumpeter should have rather uncritically imported the conception of science of his time? More importantly still—and in contrast with Shionoya—I contend that much of what Schumpeter writes does not really depend upon that (now philosophically discredited, but still widely held) conception. Though this conception constrained the expression of his insights, he could not avoid transcending its limitations and producing a body of work as remarkable as that of any great economist. And that ought to be regarded as much more important than mere coherence.

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\(^1\) Whilst I think that there is a tendency to turn a blind eye to, rather than make sense of, Schumpeter’s protean inclinations, I am not suggesting that my interpretative strategy is somehow unique within Schumpeterian studies. Oakley (1990), for instance, follows similar lines. Others point to a mismatch between Schumpeter’s ‘vision’ and his ‘technique’ (see Minsky’s (1992) comment on a Shionoya paper). Indeed, all the studies that attempt to understand why Schumpeter is inconsistent, rather than merely registering or even ignoring that fact, ultimately point in the same direction. Still, critical realism could be the key for a thorough rationalisation of Schumpeter’s tensions and inconsistencies.
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