

Schumpeter on the integration of theory and history*

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1. Introduction

‘Economic historians and economic theorists can make an interesting and socially valuable journey together, if they will’, Joseph Schumpeter (1989b [1947]: 221) wrote. ‘It would be an investigation into the sadly neglected area of economic change’. But the journey he envisaged has not happened. If anything, the gulf between economic theory and history has widened since Schumpeter’s death (see Hodgson 2001). Still, many economists – and presumably historians – worry about how to transcend this situation; and in this context it seems at least reasonable that Schumpeter’s specific agenda for the integration of theory and history should be recovered and scrutinized. This has been done; and Schumpeter’s agenda has been defended in some recent, distinguished contributions (see Ebner 2000, Shionoya 1990, 1997 [1995]).

In this paper I want to assess to what extent this defence is warranted. But, for the sake of brevity, I do not explicitly confront any writings on Schumpeter.¹ Instead, I begin with a discussion of the requirements of a historically sensitive theory (Section 2), drawing on Kurt Dopfer (1993) and Tony Lawson (1997). I then argue that, although Schumpeter’s aim was to arrive at a historically sensitive theory, the strategy he outlined for reaching this objective was misconceived (Sections 3 and 4). A note on Schumpeter’s substantive work follows (Section 5), and the paper concludes with some comments as to why the detected schism between Schumpeter’s aims and his strategy came about and persisted (Section 6).

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2. A framework

The problem of integrating economic theory and history has a long and distinguished lineage. Gustav von Schmoller, among others, intended such an integration. His objective was not to do without theory altogether but to construct a new, historical, or historically sensitive, kind of economic theory (see, e.g., Schumpeter 1954b [1926]). What, though, is a historical theory? This is the first problem that must be addressed.²

2.1 On 'theory', 'history' and 'historical theory'

Let us define 'theory' as a consistent set of statements embodying some kind of general knowledge. 'History', in contrast, highlights singularity; it does not seek to generalize (see Dopfer 1993: 160). Let us note, furthermore, that 'theory' is not confined in its temporal scope—in principle it can refer to our past as well as our future; whereas, in Dopfer's expression, there is no 'history' of the future. 'History' always refers to our past only.³ What, then, is a 'historical theory'? I believe that an answer can be tentatively given once we examine the conceptions of 'theory' and 'history' provided above from an ontological perspective—i.e., once we focus on the common *object* of 'theory' and 'history'.

To this effect, it is convenient to introduce two contrasting conceptions of the nature of reality. Following Lawson (1997), let me define a *closed system* as a system characterized by universal constant conjunctions of events of the form 'whenever event *x* then event *y*'. *Open systems*, in contrast, are systems in which such conjunctions do not typically obtain. Let me also define *philosophical empiricism* as the theory of knowledge according to which epistemic claims can be made only about objects of experience.

Now, a theory of knowledge is, unavoidably, a theory of knowledge *of something*. It presupposes an ontology. Hence philosophical empiricism is a theory of knowledge presupposing an ontology constituted by the category of experience—by events as they experienced. It follows, then, that the possibility of general knowledge—of 'theory'—rests upon the identification of constant conjunctions of events of the kind specified above: regularities of this kind are the only conceivable generalizations. If such regularities are not ubiquitous, however, it does not necessarily follow that general knowledge cannot be obtained. It merely follows that philosophical empiricism is an inappropriate theory of knowledge *of open systems*.

Indeed, we do possess general knowledge, however fallible, of the natural world—where, if we discard the particular case of celestial closure, spontaneous occurrences of constant event conjunctions are hardly pervasive (see Lawson 1997: 29–30). This knowledge is expressed in the

form of 'natural laws'. But these laws refer not to events as they are experienced but to the operation of structures which contribute to the production of, yet are irreducible to, events.

Put otherwise, reality encompasses distinct levels, which are typically not synchronized with one another (see *ibid.*: 21–2). Events are real, yet they are not the same thing as perceptions of them—which are likewise real, though they may differ and be inadequate. Beyond experience and the objects of experience, however, there exist entities which co-produce events, even if they are not reducible, and may not be straightforwardly accessible, to perception. For only if reality is so constituted is it possible to rationalize the fact that knowledge expressed in the form of 'natural laws' is successfully applied outside the experimental closures set up by natural scientists (see *ibid.*: 27 ff.).

In short, it is possible to establish, as Lawson (1997) does, the existence of structures that are causally efficacious independently of the conditions under which their empirical identification occurs. But this argument also yields what a 'historical theory' is, at least at a very abstract level. For the object of any theory that can be so described must be an open system, in which events are (on ontological, rather than merely epistemic grounds) singular and unpredictable. Given the nature of general knowledge of open systems, it follows, then, that a 'historical theory' is a *consistent set of statements about some (enduring) structure, and its mode of operation*.

Indeed, we can also specify how 'history' and 'historical theory' relate to one another. Just as we can distinguish different ontological levels, it is possible to distinguish various analytical levels. In particular, it is possible to differentiate two analytical orientations: the identification of enduring structures and conceptualization of their mode of operation, on the one hand; and the *ex post* explanation of specific empirical occurrences on the basis of the simultaneous, possibly countervailing, operation of various such structures, on the other. These two types of endeavour—which Lawson (1997: 220–21) labels 'pure' (or 'abstract', or 'theoretical') explanation and 'applied' (or 'concrete', or 'practical') explanation—match, respectively, 'historical theory' and 'history' as defined above. 'Applied explanation', or 'history', is always and only reconstruction of a unique past. To the extent that structures endure in the relevant time span, 'pure explanation' can also hold for the future. It qualifies as 'theory' in the strongest sense.

Even though their viewpoint and interests differ, then, there is no opposition between 'theory' and 'history'—at least in the natural realm with which we have hitherto been concerned. *All* theories of this realm are 'historical' to the extent that they are theories of an open system.⁴ Our interest lies in the nature of a historical *economic* theory, though—a 'historical theory' of the economy, which is part of the social realm.⁵ In the

conventional sense, 'social' means 'dependent upon human agency'. So a new set of problems opens up, at any event if it is accepted that human agency cannot be reformulated merely in terms of natural laws. What are the implications for the notion of 'historical theory'?

Here, too, I follow Lawson (1997: 30 ff.), and start from the conception that *choice* exists. This presupposes that the social world is open, and ontologically irreducible to the natural world. It presupposes, moreover, that human agents are intentional, that they have some conception of their activity. Intentional behaviour, in turn, demands some degree of knowledge of the conditions that render purposeful action possible. And knowledge requires that its object be relatively enduring. If constant conjunctions of events are untypical, as is the case in an open world, then the relatively enduring objects of (possibly tacit) knowledge must lie at the level of structures—which facilitate, and constrain, but do not pre-determine, intentional human action. In as much as they are social, these structures, or rules, are in turn dependent upon human agency.

Choice, in sum, necessitates that the social world be open as well as structured, and the reproduction and transformation of social rules presupposed by the exercise of choice is in turn dependent upon the choices actually made. I.e. there is a social history—but in a different sense in which there is a natural history. Because social history is *made* by intentional actors, social structures cannot be expected to endure *in the sense* that natural structures do. And, because of their dependence upon human agency, they will usually be more time-specific (see Lawson 1997: 34).

In principle, then, our previous definition of 'historical theory' does hold for the social realm. A 'historical theory' can still be defined as a consistent set of statements about some (enduring) structure and its mode of operation. But a social (or economic) explanation will often need to account for the emergence of social structures, and perhaps for their demise as well; and social history, to repeat, is made by intentional actors who, in the process of acting, reproduce or transform the structural conditions of their actions. While it is possible to (fallibly) identify social structures contributing to the production of particular historical patterns, these structures must in turn be explained in terms of actions conditioned by a potentially different set of structures. In practice, this entails that 'historical theory' as defined above has to interact with 'history' so as to attempt to render the social process intelligible. Such an interactive endeavour could (provisionally) culminate in the formulation of a theory of this process—a theory *of* history. Whether it does or does not is an empirical matter. But, of course, empirical matters are themselves conditioned by 'theory', just as 'theory' is, or ought to be, empirically conditioned; which leads us to my next point.

2.2 Recognition, transformation and the structure of scientific theories

As I suggested earlier, Schmoller envisaged a historically sensitive kind of economic theory. In fact, he envisaged an ‘evolutionary’ theory of the unfolding of the social process. Surprisingly perhaps, Carl Menger did not disagree with this aim (see Dopfer 1993: 152). It appears to be the case, then, that the fundamental reasons for the *Methodenstreit* are to be found not so much in different views about the scope of economics – which in turn are, at least in Schmoller’s and Menger’s cases, bound up with views about the nature of reality – as in disagreements with regard to epistemology and method. Indeed, as Dopfer (*ibid.*: 157) points out, ‘it is not only the ontic status of reality, i.e. the different views about its “essence”, that makes for different approaches to economic theory. It is also the theoretical preconceptions themselves that determine the choice of an epistemology which, in turn, supports the scientific work that results from that theoretical preconception’.

Let me elaborate on this crucial issue (see *ibid.*: 147 ff.). At the outset of theoretical efforts of any kind there is a problem of *recognition*. It is necessary to form a preliminary picture of reality, to pre-select something as a meaningful object. Clearly, this recognition cannot be a-theoretical. Yet the theoretical notions involved in this phase will not be thoroughly scrutinized. The theoretical process is, in the main, the process of transforming this preliminary ‘vision’ – if we want to use Schumpeterian language – into a piece that provides some sort of general knowledge. And in so far as this piece is to be a *scientific* theory it must accord with some criteria as to what science is. In other words, it is necessary to arrive at a solution to the question of how reality can be expressed in the form of theoretical statements – or, as we may say, to the problem of *transformation*,⁶ yet this solution is in turn influenced by views about the form, or *structure*, of a *scientific theory*.

Since recognition involves a set of preconceptions about the nature of reality, and transformation is likewise affected by preconceptions regarding the structure of a scientific theory, it is clear that theory construction is, potentially, a process beset with tensions. Preconceptions have a resilient quality; they may, and often do, endure.⁷ It is conceivable, for instance, that a ‘vision’ survives even if it cannot be validated ‘scientifically’.⁸ It is also conceivable that preconceptions about the structure of a scientific theory – or about a particular mode of explanation – crystallize. Indeed, much of modern economics can be regarded as an extreme case of the latter vice, where the problems of recognition, hence of transformation, are deliberately assumed away and appropriate ‘axioms’ and ‘assumptions’ are brought in (see Dopfer 1993: 151). Clearly, this can be done only at a

heavy price. Given that a mode of explanation implicitly presupposes an epistemology, thus an ontology, no mode of explanation can provide knowledge but of a world of the sort which it implicitly presupposes. If a mode of explanation is selected *a priori*, then, similarities between the world which a theory so constructed presupposes and the existing, or even a possible, world can arise only as a result of some lucky accident.

As it happens, Schmoller of all people turns out to be affected by preconceptions regarding the structure of scientific theories not dissimilar from those of mainstream modern economics. To be sure, his is not an extreme case. He did not dismiss the recognition problem. But, as Dopfer (ibid.: 174) explains, '[h]is historical method was directed towards finding empirical regularities that could serve as deductive systems appropriate for theoretical prediction'. Ultimately committed to this empiricist scheme as he was, he could not escape from the fallacy of time-symmetry, and did not succeed in developing a theory that expounded historical features.

2.3 *Two propositions*

I have discussed the nature of 'historical theory', and pointed to a set of problems that impact upon the construction of scientific theories. These problems suggest the questions that need to be asked in assessing Schumpeter's agenda for a historically sensitive economics. They are similar to the questions which Dopfer (1993) poses and answers in his assessment of the *Schmollerprogramm*. Did Schumpeter envisage a 'historical theory'? What path did he propose for reaching his objective? Is this path appropriate?⁹

Given these questions, I want to put forward two propositions, which are the object, respectively, of Sections 3 and 4:

- 1 Schumpeter's aim is to arrive at a 'historical theory'. His ontological beliefs explain this aim.
- 2 Schumpeter's methodological agenda is inappropriate to achieve his aim. It is not an empiricist agenda, yet Schumpeter is committed to a positivist misconception of the structure of scientific theories.

3. Schumpeter's historical inclinations

3.1 *Historical theory as 'the programme of the future'*

Gustav v. Schmoller und die Probleme von heute (Schumpeter 1954b [1926]) deals with many issues and is a complex piece. Still, a few things can be

stated right away. First, this is a piece not just about Schmoller and the *Schmollerprogramm* but, to a considerable degree, about Schumpeter himself. Second, Schumpeter criticizes the usual perception that Schmoller simply lost the *Methodenstreit*, and indeed offers a sympathetic—if by no means unreservedly positive—assessment of Schmoller.

On the one hand, Schmoller's epistemology is (politely but unambiguously) argued to be inadequate (see, e.g., *ibid.*: 168, fn. 1, 196). On the other hand, though, Schmoller is connected with the 'programme of the future' (*ibid.*: 169), the objective of which is 'a unitary sociology or social science as reasoned ("theoretically" worked out) universal history' (*ibid.*: 193).¹⁰ In particular, Schmoller's work is said to point to 'a certain kind of development theory, a causal theory of social development based *upon*, but different *from*, the mosaic of partial explanations which, in the first instance, constitute the sociological universal history or universal historical sociology' (*ibid.*: 196).

'Development', of course, is Schumpeter's topic. Moreover, his aim in *Business Cycles*—where he revises his development theory with a view to providing an account of capitalist history—is explicitly described as reasoned history (see Schumpeter 1939: 220). Thus, his work seems to be intended as a contribution to the 'programme of the future'.

And in the Schmoller essay this programme and its aims are connected with the notion of historical theory—or, rather, with the most interesting of the many meanings that this term can take (see Schumpeter 1954b [1926]: 177–78). According to Schumpeter, a historical theory in this most interesting sense is a theory—of social classes, for instance, or of business cycles—that grows out of historical materials and is valid beyond the concrete materials that have led to its formulation, at least conjecturally and unless it is shown to be false.

Prima facie, then, it seems that such a theory cannot stand in conflict with history. And throughout the Schmoller essay Schumpeter attempts to demonstrate—this is arguably his main objective—that there is no opposition between theory and history. Analysis of individual historical phenomena, or their very recognition, is inconceivable without general categories (see *ibid.*: 173, 174), yet such categories are in turn revised in the light of history (see *ibid.*: 178, 186). If one understands 'concrete' to mean 'unanalysed', Schumpeter writes,

then there is nothing concrete either in history or in economics. If the contrast between concrete and abstract is identified with the contrast between individual and general, then interest in the concretely significant and interest in the generally true—better, in the widely applicable—are indeed conceptually distinguishable; but they immediately intermingle in practical work ... Concrete and abstract refer ... only to dif-

ferences in degree in the asymptotic approximation of reasoning to the details of specific cases.

(Ibid.: 173–74)

More needs to be said about the Schmolter essay. For convenience of exposition, though, let us simply bear in mind for the time being that Schumpeter rejects Schmolter's epistemology but not the objectives of the *Schmolterprogramm*. Indeed, Schumpeter appears to be aiming at a kind of theory that *he* considers 'historical'. But can such a theory be regarded as a 'historical theory'?

3.2 Schumpeter's implicit ontology

I think it *can*, at any event if we consider Schumpeter's conception of the nature of his object—or, which is the same, if we consider why he wants to contribute to a theory that he labels 'historical'. A theory that can be qualified in this way must be a theory of an open system; and even the most cursory analysis cannot fail to notice that Schumpeter draws attention to openness throughout his writings. This is why he is interested in history in the first place.

In his *History of Economic Analysis*, for instance, he argues that economic history is the most important technique of economic analysis, to begin with because 'the subject matter of economics is essentially a unique process in historic time' (Schumpeter 1954a: 12). Similarly, he often insists that prediction of events is typically impossible, or that there is no 'history' of the future: 'It is as unreasonable to expect the economist to forecast correctly what will actually happen as it would be to expect a doctor to prognosticate when his patient will be the victim of a railroad accident and how this will affect his state of health' (Schumpeter 1939: 13).

In addition to this, Schumpeter recognizes that the social world is structured, and indeed that social structures are reproduced and transformed by intentional human action which in turn they condition. As is well known, in his *Theory of Economic Development* (Schumpeter 1912, 1934) he emphasizes that human action is not a unitary category, even if deliberation of some sort is never absent, and distinguishes between habitual, rule-guided behaviour and entrepreneurial action. In their daily endeavours individuals are argued to draw upon rules, which are thereby reproduced; whereas entrepreneurial behaviour means transcending and deliberately changing these rules. Yet the exercise of entrepreneurship is itself conditioned by various rules or structures. In the capitalist setting with which Schumpeter is concerned, for instance, entrepreneurship is

facilitated by a particular institutional arrangement, of which certain powers of the banking system are a crucial part.

In fact, I would go so far as to argue that, *at some level*, Schumpeter's whole substantive work *can* be regarded as compatible with the conception that underlies my previous discussion of the nature of historical theory. In his *Theory of Economic Development* he proposes to explain how entrepreneurial, or innovative, action shapes capitalist motion. The reproduction in time of capitalism's institutional arrangement—involving an interaction between entrepreneurs and institutions—is argued to give rise, among other things, to a particular pattern: the business cycle. But such a historical theory—which can be interpreted as an attempt to establish the (sufficient) conditions for the existence of cycles in the capitalist period—cannot say anything concrete about innovations, or about specific cycles. As Schumpeter puts it,

one of the most annoying misunderstandings that arose out of the first edition of this book was that this theory of development neglects all historical factors of change except one, namely the individuality of entrepreneurs. If my representation were intended to be as this objection assumes, it would obviously be nonsense. But it is not at all concerned with the concrete factors of change, but with the method by which these work, with the *mechanism of change*.

(Schumpeter 1934: 61, fn. 1)

In other words, his theory of development, describing in general terms a set of structures and their action-dependent mode of operation, is insufficient for an understanding of the concrete history of capitalism. Rather, '[g]eneral history... , economic history, and more particularly industrial history are... indispensable... All other materials and methods, statistical and theoretical, are only subservient to [these historical materials] and worse than useless without them' (Schumpeter 1939: 13). Correspondingly, *Business Cycles* (Schumpeter 1939) aims at a reasoned history of capitalism, an analysis of capitalist history in which historical materials lead to theoretical reformulation. Naturally, this process whereby theory is revised in the light of history is always unfinished, which is why Schumpeter insists on the necessity of co-operation between theorists and historians.

But the capitalist process entails an action-dependent transformation at *various* levels of structure. Notably, it can lead to a change in capitalism's *constitutive* structural features, not merely *within* those boundaries. It is conceivable, then, that capitalism evolves a tendency to metamorphose into something that cannot anymore be described as capitalist. This, of course, is the topic of *Capitalism, Socialism and Democracy* (Schumpeter 1942) – or, for that matter, of *Sozialistische Möglichkeiten von heute* (Schumpeter 1920/21). Schumpeter's aim now is to fully develop his historical theory into a theory of (capitalist) history.

4. Schumpeter on the structure of science

Let us turn to Schumpeter's methodological agenda. My purpose is to argue that this agenda is at odds with his historical proclivities. It does not result from sustained ontological reflection, which is essential if a method, or mode of explanation, is to be appropriate to its object. I first focus on *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (Schumpeter 1908), as well as on a rather more straightforward lecture on how to do social science (Schumpeter 1915 [1910]), with a view to showing that Schumpeter's preferred conception of the structure of scientific theories presupposes that the social world is a closed system. I then turn to the last chapter of the original *Theorie der wirtschaftlichen Entwicklung* (Schumpeter 1912), omitted in later editions, to which Yuichi Shionoya (1990) has drawn attention. In the outline of his research strategy included in this chapter, Schumpeter is particularly clear about the necessity of taking a closed system framework as the starting point in the scientific treatment of change and novelty. And his subsequent methodological rationalizations – including the Schmoller essay, to which I return – indicate that his allegiance to closed systems theorizing endured.

4.1 'Pure theory' as the framing of exact regularities

Wesen is a methodological defence of 'pure theory' in economics (see Shionoya 1997 [1995]: 91 ff.). And 'pure theory', as Schumpeter repeatedly stresses, amounts to framing 'regularities' – meaning strict, or exact, regularities of the form 'whenever event x then event y '.¹¹ This is apparent from his insistence that the results of 'pure theory' are 'certain', 'uniquely determined'; and, correspondingly, from his pronouncements in favour of 'the most exact, the only truly exact form of reasoning, mathematics' (Schumpeter 1908: 454). 'For the sake of greater precision', Schumpeter states, 'we want to talk not about "causes" of phenomena but only about functional relations between them. The concept of function, which has been carefully developed by mathematics, has a clear, unambiguous content; not so the concept of cause' (ibid.: 47). In fact, investigations into the 'causes' of phenomena are argued to be irrelevant for 'pure theory'. In Schumpeter's words:

What the economy actually *is*, and whether its driving force is the individual or must be searched for elsewhere, is of no consequence to us. In general, we are happy to accept everything that ... historians tell us about this matter ... But what is important for us is not how these things really are; rather how we must model or stylize them for our purposes – i.e., which conception is the most practical from the standpoint of the results of pure economics ... Is the essence of the economy really indifferent to the economist?

We have no doubt in answering in the affirmative . . . We want to describe – within very narrow boundaries only – certain economic processes. Their deeper grounds may be interesting but they do not affect our results.

(Ibid.: 93–4)

Pure economics, in sum, appears to be *defined* by the structure of its results. Nevertheless, this conception is not entirely *a priori*. Consider the following statements:

It is the case that [social] facts, as they are given to us in reality, only show permanent change. But nature around us also shows an unending richness of variety. We would arrive at no result if we wanted to describe each individual stone that has been observed. We must resolve phenomena into their elements and consider each of these elements by itself. Then the otherwise invisible regularity becomes apparent. And this we must do as well in the social sciences. This is what we call doing ‘theory’.

(Schumpeter 1915 [1910]: 17–18)

It is of course in the essence of every theoretical science that it examines in all its consequences the individual elements of phenomena with which it is concerned, excluding – by means of adequate assumptions – the influence of other elements. In this sense one can say that the theoretical social sciences only describe tendencies of reality, never the whole of reality. They consider, for instance, economic behaviour as if there were no other kind of behaviour. In so doing they do not, of course, claim that in fact there is no other kind of behaviour. Analogously, I can say that each part of my body has a tendency to fall to the ground, but this is clearly not a claim to the effect that I am actually falling. Why is it that this way of expressing things shocks no one, and yet one often prohibits the same method in the social sciences?

(Ibid.: 23–4)

Let us attempt to unravel what Schumpeter is implying here. First, he makes it clear that, in economics as in the natural sciences, strict regularities are not simply given to us in experience. Rather, they are constructs, based on assumptions which, as he repeatedly observes (see, e.g. Schumpeter 1908: 46, Shionoya 1997 [1995]), are heuristically useful creations of the theorist. Indeed, in his view, ‘in *all* exact sciences and so in our field, too . . . our exact system, correctly presented and thought out up to its root, is a construct foreign to reality’ (Schumpeter 1908: 272).

Second, the above quoted statements seem at least to suggest that ‘invisible regularities’ *constitute* ‘laws’ in the same sense in which one can speak of ‘laws of nature’. This is in fact Schumpeter’s typical stance:¹²

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: 43)

In its methodological and epistemological essence pure economics would be a 'natural science' and its theorems 'laws of nature'... It is only its limited development and the organization of scientific endeavours that produce the impression that it does not belong to the realm of exact disciplines.

(Ibid.: 536)

And – third – this conception of 'laws' explains why pure economics should proceed as natural science is thought to have proceeded. True, pure economics seems unable to address 'practical problems' (see, e.g., *ibid.*: 574 ff.), indeed it can never say anything concrete (see, e.g., *ibid.*: 561) – notably because things change and pure economics involves a *ceteris paribus* proviso (see *ibid.*: 577). Yet natural science has achieved practical success, and pure economics offers the same promise.

Now, as has been argued before, Schumpeter is correct in his observation that the empirical world shows no 'exact regularities'. However, his conception of laws is incorrect. Certainly, the laws of natural science are not to be confused with non-empirical exact regularities based upon arbitrary, heuristically useful assumptions. Otherwise it is difficult to see how such laws could possibly have led to any practical achievements. Schumpeter, then, recognizes that there exist no empirical regularities of the form 'whenever event *x* then event *y*' – thereby implying that the world is *not* a closed system; yet he sublimates this inconvenience, so to speak, into the *creation* of regularities of the form 'whenever conditions *x* then outcome *y*' in an imaginary, fictitious world – thus accepting a structure of scientific results which *presupposes* closure.

Not unexpectedly, this contradiction leads to confusion. Schumpeter repeatedly points out that pure economics covers a very wide amount of facts, yet he also concedes not just that it cannot explain entrepreneurial profit or interest, capital formation or crises (see Schumpeter 1908: 307 ff., 384 ff., 431 ff., 587–88) – i.e., the topics with which his *Theory* is concerned – but that, 'strictly speaking, our system excludes any variation' (*ibid.*: 463) and supposes that human beings do not grow old or change in any other respect (see *ibid.*: 588).

Still, he remains convinced that 'pure theory' cannot be dispensed with. In particular, he seems to think that it has a decisive advantage over history. For historians can never strictly prove their answers, to the effect that 'it is easy to contradict any historical argument' (Schumpeter 1915 [1910]: 16). '[T]he judgements of historians', Schumpeter submits, 'do not have scientific certainty. They are closer to the creations of the artist than to the results of the researcher' (*ibid.*: 17).

4.2 Closure as a necessary first step

So far I have not brought in the concept of equilibrium. But I think it is clear (see Shionoya 1997 [1995]) how it relates to Schumpeter's conception of the structure of scientific results:

[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 legitimizes their separate treatment provided that they are uniquely determined... 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: 33–4)

In the last chapter of *Theorie der wirtschaftlichen Entwicklung* (Schumpeter 1912) Schumpeter comments on why this concept of equilibrium is indispensable in the analysis of change and novelty. Indeed, as Shionoya (1990) has shown, this chapter includes a fairly comprehensive statement of Schumpeter's research agenda from a methodological viewpoint.

'The first step towards an analysis of the overall process of economic life', Schumpeter (1912: 464) writes, 'is done with static theory' – which, as he puts it in *Wesen*, describes an important 'aspect of human action', 'the first that must be solved and understood before one can proceed' (Schumpeter 1908: 568–69). And 'the second step towards an overall picture of the economy consists in the investigation of the phenomenon of development' (Schumpeter 1912: 465). The purpose of this investigation is to find out, 'independently of the concrete changes which take place, *how* they come about' (ibid.: 467) – to describe the mechanism of innovation rather than its specific historical content.

Why, though, must one start with 'pure', or static, theory? This turns out to be quite comprehensible given Schumpeter's conception of science, which presupposes a closed system. *Under such ontological presuppositions*, equilibrium, or the global coherence of a set of functional relations, is indispensable as a notion of order. Yet, as Schumpeter recognizes, development, or innovation, is a phenomenon that does not of itself exhibit any tendency to equilibrium. Therefore – given his identification of equilibrium with order – he is convinced that development is not amenable to *scientific* treatment unless it can somehow be connected to, or expressed within, an equilibrium framework (see Shionoya 1990: 321):

Is the theory of development a correction of the picture of the static economy? Must this picture... give way to a new theoretical reconstruction of reality? ... We know already... The core of static theory shall not be replaced.

(Schumpeter 1912: 511)

Those static rules are the basis of a scientific understanding of the economy.
(Ibid.: 471)

[T]here exists no dynamic equilibrium. In its innermost essence development is a disturbance of the existing static equilibrium... Development and equilibrium, in our meaning of these terms, are opposites, and exclude each other. It is not the case that the static economy is characterized by a static equilibrium and the dynamic economy by a dynamic equilibrium.

(Ibid.: 489)

But this position leads to a very serious problem. In an equilibrium setting, it is meaningless to speak of choice in that 'actions' are completely determined by 'given data'. Capacities are expressed as, or conflated into, actual outcomes. And it is precisely the distinction of these two realms – or the recognition of structure as a condition of, which however does not determine, action – that makes it possible to *explain* choices which nevertheless are *made*. In attempting to express his 'vision' of a world where change and novelty are essential in a form compatible with his preconception of the structure of science Schumpeter is in fact splitting up human action into two components: one of which is fully determined by 'given data' whereas the other appears as a kind of unconstrained 'free will'. This evidently distorts his conception that human action, even if it is not a unitary category, always involves choice and deliberation, and always presupposes some rules. Under such circumstances, 'entrepreneurship' can be coherently conceptualized only as an 'exogenous shock'. And the purpose of scientific endeavours must be to endogenize this factor within a broader equilibrium framework, thus achieving a closed system representation which embodies more knowledge of its (closed system) object. There is no way in which such a methodology can, if coherently followed, be useful for the construction of a historical theory.

We can conclude, then, that Schumpeter's methodological agenda is inadequate from the outset in that it involves identifying order with equilibrium, and attempting to connect entrepreneurship to an equilibrium framework. But there is a second part to this agenda. Schumpeter points out that it is possible to distinguish creative and adaptive behaviour in all areas of social life. Consequently, he argues, his framework can be generalized, legitimizing a series of separate, autonomous social sciences of the same nature as theoretical economics (see Schumpeter 1912: 535 ff.). Yet 'achievements in each field of social action ultimately have an impact on all areas of social life and change the presuppositions and conditions of human action in all of them' (ibid.: 547). According to Schumpeter's metaphysics, all areas of social life are internally related to a significant degree or, in his phrasing, the culture of a time has an organic unity (see

ibid.: 546): ‘[t]he social process is really one indivisible whole’ (Schumpeter 1934: 3). Thus, the final theoretical objective is an analytical integration of all areas of social life – hence of all separate social sciences in a ‘universal social science’ – so as to reconstruct the overall social process (see Shionoya 1990: 322). But why, then, consider these areas separately in the first place? Indeed, how is it *possible* to integrate a set of equilibrium constructs so as to arrive at an analytical representation of an organic process?¹³

At any event, Schumpeter’s remaining methodological comments suggest that he did not quite manage to overcome the belief that the framing of ‘exact regularities’ is essential, at least as a first step. This is apparent, to give but one example, in the Schmoller essay (Schumpeter 1954b [1926]). While committing himself to a ‘unitary social science as reasoned history’, Schumpeter insists on the value of ‘pure theory’ – and, in particular, on its value as a means of *establishing* ‘interesting results’ (see also, e.g., Schumpeter 1954a: 14–15).

Recall that in this essay Schumpeter stresses that there is no opposition in principle between theory and history. He recognizes, again correctly, that analysis of historical phenomena is inconceivable in the absence of theoretical categories. Unfortunately, though, he also thinks that such theoretical categories *as are offered by received orthodox theory* – presupposing a closed system – cannot be discarded in the analysis of a world which, as I have argued before, he recognizes as an open system.

Thus, at one point, he remarks that the theory of cycles must be a historical theory. He also remarks that all attempts to construct a theory of cycles on the basis of ‘facts’ alone are flawed; whereas all successful contributions to such a theory, in particular Arthur Spiethoff’s self-styled ‘historical’ approach, read like strict ‘theoretical’ work. But this, he argues,

does not mean that there exists a nomothetic system of truths that explains certain phenomena, amongst them crises... – which would be the... core claim of the true theorist against the claims of historical investigation (*Detailforschung*). Rather, it is simply due to the fact that analysis of cyclical fluctuations is something sufficiently complicated to demand particular interpretative procedures (*Auffassungsweisen*) – i.e., [‘pure’] theory [understood as means of obtaining interesting results]. This, however,... does not contradict the claims of historical investigation as a way of obtaining economic knowledge. Because every understanding of history is meta-historical in the sense that it demands theoretical means (*gedankliche Mittel*)... and because, therefore, even a mere description resorts to theory in this sense – if only the popular theory of ‘common sense’ – there is no transition to another set of principles when one resorts to interpretative procedures [i.e., ‘pure theory’] which are nothing but logic adapted to our purposes, refinements of our theoretical means, developed, sharpened, polished ‘common sense’ – as any method of mathematical statistics.

(Schumpeter 1954b [1926]: 187–88)

5. A note on Schumpeter's substantive work

It should not be assumed that Schumpeter follows his methodological agenda in a mechanical fashion. In fact, I have already implied that his ontological beliefs shine through in his substantive writings. Still, to the extent that these writings are influenced by the conception that science requires strict regularities, or equilibria, it follows from my analysis that they must be inconsistent. I now want to provide some evidence for this, and to this effect I shall briefly turn to *Business Cycles* (Schumpeter 1939) – or, rather, to a small fraction of this gargantuan work.

Since the original reviews of this book it has been suggested that Schumpeter's *historical* description is at odds with his *theoretical* model. This, of course, is *prima facie* surprising in a work aiming at a theoretically worked out history of capitalism. If my previous analysis is correct, though, this fact can be rendered intelligible; and I propose to do so by considering, albeit in a very summary fashion, the strategy of successive approximations that Schumpeter elects to follow in developing his theoretical framework. More specifically, I want to argue that this strategy culminates not in a model able to illuminate capitalist history but in a bit of a confusion; and that this confusion originates in Schumpeter's attempt to reconcile his view of capitalism as an open and structured system with a method presupposing closure. Naturally, the fact that Schumpeter does not succeed in offering a coherent theory does not, and could not, mean that his historical chapters are a-theoretical. Rather – I cannot enter into this topic here, though – these historical chapters are largely informed by another framework.

5.1 A strategy of successive approximations

Schumpeter's commitment to (a variant of Walrasian) equilibrium is explicit in the theoretical chapters of *Business Cycles*. And he goes to considerable lengths to dismiss or downplay any factors – expectations, imperfect competition, etc. – that might collide with determinate equilibrium solutions. But how is it possible to start from an equilibrium model, preserve equilibrium, and yet arrive at a theory appropriate to render capitalist history intelligible? Schumpeter's strategy of successive approximations is an attempt to overcome this apparently insurmountable difficulty.

The intellectual origins of his first approximation are fairly clear. Schumpeter appeals to the reader to 'grant provisionally all simplifications' – such as the assumption of 'a state of perfect equilibrium from which to start', or the 'absence of certain elements which in reality are very important – notably, errors in diagnosis or prognosis and other mistakes' (Schumpeter 1939: 130) – and brings in the 'entrepreneur'. By heroic

assumption, all those who follow this 'entrepreneur' in the path of innovation are also assumed to be endowed with perfect foresight. Innovation, however, eventually stops, notably because of difficulties in calculation and planning – though it is unclear how this can be squared with the characteristics attributed to 'entrepreneurs' in this approximation; and an adaptative process, recession, 'leads up to a new neighborhood of equilibrium, in which enterprise will start again' (ibid.: 137) – though it is not made clear what a neighbourhood of equilibrium precisely is.

On the whole, then, the first approximation is hardly satisfactory. Indeed, if coherently followed, Schumpeter's methodological preferences would entail the reduction of entrepreneurship to an 'exogenous shock', to be subsequently endogenized in a broader equilibrium model – as has been explained earlier. Instead, Schumpeter concedes that it is a long way to the point of junction with historical fact, and presents a second approximation.

'[E]rrors, excesses of optimism and pessimism and the like are', he writes (ibid.: 146), 'not necessarily inherent in the primary process' described in the first approximation. Yet the major elements involved in undertaking innovations 'simply cannot be known' (ibid.: 100). Of those entrepreneurs who manage to get their projects under sail 'nine out of ten fail to make a success of them' (ibid.: 117). Moreover, prosperity is characterized by 'speculation'. Ephemeral earnings and fantastic prospects are capitalized, 'deposits' are created to finance general expansion, and each loan tends to induce another, as does each price rise. Such phenomena engender a 'secondary prosperity', superimposed upon the error-free primary wave. And, because of errors made during prosperity, a 'normal' recession evolves into an 'abnormal' depression.

However 'ideally' confined to 'essential' or primary processes, prosperity always induces a period of liquidation which, besides eliminating obsolete firms, involves a painful process of readjustment; but now there is much more to liquidate and to adjust (see ibid.: 148), and depression may actually destroy businesses that could otherwise have survived. Indeed, it sets into motion 'a mechanism which, considered in isolation, could... run on indefinitely under its own steam' (ibid.: 151). Still, Schumpeter (ibid.) submits, 'proving from the properties of such a mechanism, the elements of which have been taken out of their setting in the economic organism, that the process will go on intensifying itself, does not amount to proving that its real counterpart will actually do so'. Eventually, disequilibrium will induce a recovery leading to a neighbourhood of equilibrium, even if the case for government action in depression is 'incomparably stronger than it is in recession' (ibid.: 155).

But, if the major elements involved in innovation 'simply cannot be known' – which *is* essential to the notion of entrepreneurship – then error

and ignorance cannot be conceptualized as a mere disturbance, and the distinction of recession and depression is unsatisfactory. To the extent that it brings in a new element, then, the second approximation is incompatible with the first, and yields its irrelevance. And this inconsistency arises only because Schumpeter chooses to start from a framework presupposing a closed system and yet recognizes that the social world is open.

There is a third approximation still.¹⁴ Schumpeter remarks that, for several reasons, innovation will set into motion an indefinite number of wavelike fluctuations, which will roll on simultaneously and interfere with one another (see *ibid.*: 161). And, in so doing, he appears to be trying to bring in explicitly some sort of structure. Indeed, in his view, innovation tends to proceed along some roads opened up by, and modified in the course of, its history; and, in particular, some important innovations end up producing the structural conditions for innovations at some lower level—which in turn impact upon these conditions. Electricity, for instance, is said to have led to the appearance of new industries and commodities, and to have upset industrial locations (see *ibid.*: 398)—in fact, to have led to new forms of social action and reaction—although its cost advantage was initially small, if at all positive.

But how can the recognition that innovation is a structured process be represented in an equilibrium framework? Schumpeter settles for the reduction of the multiplicity of fluctuations to three cycles—long waves of somewhat less than 60 years ('Kondratieffs'), intermediate waves of somewhat less than 10 years ('Juglars'), and short waves of somewhat less than 40 months ('Kitchins')—with each Kondratieff containing 6 Juglars and each Juglar 3 Kitchins. Clearly, though, it is now untenable to argue that (a neighbourhood of) equilibrium must be the starting point of each of these cycles. Certainly innovation is now conceivable in situations which, in the first as in the second approximation, are argued to prevent the emergence of entrepreneurial action. As Schumpeter puts it:

Since, of course, shorter waves must in most cases rise from a situation which is not a neighborhood of equilibrium but disturbed by the effects of the longer waves in progress at this time, we must now modify our previous proposition that the process of innovation starts from such neighborhoods only, as well as our concept of neighborhood of equilibrium itself.

(*Ibid.*: 173)

In sum, his strategy of successive approximation does not culminate in a coherent theory suitable to illuminate capitalist history. But this is what one would expect, given Schumpeter's ontological beliefs and his commitment to preserving equilibrium.

6. Concluding comments

I have argued that Schumpeter's agenda for a historically sensitive economics is inadequate in that he is committed to a conception of the structure of science at odds with his ultimate aim. Rather than summarizing my argument, I now want to underline why Schumpeter's misconception of the structure of scientific theories arose and endured.

Recall, first, that his methodological choices do not result from sustained ontological reflection. They derive, rather, from an insufficiently critical acceptance of two propositions, one of which happens to be false. On the one hand, Schumpeter correctly recognizes that regularities of the 'whenever x then y ' type are not empirically manifest, either in the natural or in the social realm. On the other hand, he accepts that natural science rests upon the framing of non-empirical 'exact regularities', which he tends to identify with 'laws'; whereupon he infers that social science ought to proceed in a similar fashion. As has been observed, however, experiments of natural scientists do not constitute laws. They are attempts to isolate, and so empirically identify, structures that are causally efficacious both inside and outside the artificial set-up in which their identification is achieved.

In misunderstanding experimental situations Schumpeter is, of course, quite in tune with the most modern philosophical position of his time; *which is the most that one could reasonably demand*. But, moreover, the preconception that science rests on the framing of 'exact regularities' turns out to be especially hard to dislodge in that it tends to reduce metaphysical reflection to an irrelevant distraction. In a closed system knowledge is certain and its growth monistic; accordingly, there is hardly any scope for philosophical criticism (see Lawson 1997: 41).

It is therefore not surprising that Schumpeter sometimes lapses into regrettable (and, of course, self-contradictory) comments on metaphysical or methodological reflection.¹⁵ Neither is it surprising that he should recognize the relative, socially conditioned—in his terminology, 'ideological'—character of 'visions'¹⁶ and yet spend much time trying to persuade us that 'ideology' is in the last instance an evanescent category in scientific discourse. As 'ideologically' contaminated 'visions' are scientifically treated by resorting to certain techniques, he contends, they tend to be cleared of 'ideological bias' and metamorphose into objective knowledge.¹⁷

Before I finish, let me return to Dopfer (1993). At the beginning of his paper he writes: 'Our brief outline may suggest that we are on a collision course with Schmoller's work. However, such fundamental views are also always a matter of perspective. Just as we may say a glass of water is half full or half empty, so we may stress either agreement or disagreement' (ibid.: 145). Analogously, it does not follow from my critique that Schumpeter's

writings are not important for the integration of theory and history. I have only attempted to show that emulating his methodological agenda pre-empts progress towards that aim.

So the basic message is very simple: if you want to contribute to a historically sensitive economics, you should discard closed-system conceptions. It is a mistake to regard such conceptions as valid elements, or heuristically useful building blocks within a 'pluralist agenda', as many Schumpeterians seem to think or implicitly accept. It is time that positivist injunctions be ignored.

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Notes

- * A previous version of this paper was presented at the *Workshop on Realism and Economics*, King's College, Cambridge in March 2001. I am grateful to Tony Lawson and the other participants in this seminar, especially to Geoff Harcourt for his written comments, as well as to António Almodovar and two anonymous referees. This paper is part of a research project in methodology and history of economic thought undertaken within CEMPRE, Faculdade de Economia, Universidade do Porto. CEMPRE – Centro de Estudos Macroeconómicos e Previsão – is supported by the Fundação para a Ciência e a Tecnologia, Portugal, through the Programa Operacional Ciência, Tecnologia e Inovação (POCTI) of the Quadro Comunitário de Apoio III, which is financed by FEDER and Portuguese funds.
- 1 A critique of Shionoya's 1997 [1995] interpretation of Schumpeter can be found in Graça Moura 2002 – a paper that uses the same approach to Schumpeter as the present one but does not focus on his agenda for the integration of theory and history.
 - 2 The exclusive focus on the historical dimension throughout the present paper should not be read as to imply that the geographical dimension is less important.
 - 3 Throughout this section, 'history' refers to the subject, history to its object (or, rather, to one of its features). But I do neither subsume the work of historians under 'history' nor assume that only historians do 'history'.
 - 4 Indeed, *a fortiori* so in the sense that these theories refer to structures that are only relatively enduring. In this context, however, the term 'relative' often transcends the time span in which we are interested.
 - 5 The nature of the connections between 'the economy' and 'society' is not discussed here. It is implicit throughout that society is internally related to a significant degree, and accordingly its constituent elements cannot typically be understood independently of their connection to one another.
 - 6 Dopfer (ibid.: 148–49) concisely explains the nature of this problem by invoking Saussure's distinction between *signifié* and *signifiant*: 'The concept of a transformation rule indicates that an adequate use of human language does not [consist] solely in the ability to comprehend reality sensorically and cognitively nor in the brain's capacity to think in abstract symbols, but that it requires an *intermediale agent* which provides for a simultaneous comprehension of the two'.
 - 7 Indeed, violent eruptions punctuating the history of thought can be explained along these lines. In Schumpeter's words, '[o]wing to the resistance that an existing scientific structure offers, major changes in outlook and methods, at first retarded,

- then come about by way of revolution' (Schumpeter 1954a: 46). 'Science progresses, so Böhm-Bawerk once told a restless and recalcitrant young man, through the old professors' dying off' (ibid.: 850).
- 8 According to Schumpeter, Marx provides a good illustration of this inability to effect a radical 'conversion', and thus of the survival of 'ideology'. Marx's scientific work, Schumpeter (1989c [1949]: 281–82) argues, was to implement rather than correct his vision, which 'implies a number of statements that will not stand the test of analytic controls... But some elements of his original vision... that were untenable were... too closely linked to the innermost meaning of his message, too deeply rooted in the very meaning of his life, to be ever discarded'.
 - 9 Of course, this similarity does not entail that the answers to these questions are the same as Dopfer's. In fact, Schumpeter and Schmoller propose and follow different paths for the integration of theory and history. As is argued below, Schumpeter rejects Schmoller's epistemology, if not the ultimate aim of the *Schmollerprogramm*.
 - 10 Translations from Schumpeter's German works are my own. When the passages I quote have been translated before, notably in Shionoya 1997 [1995], I have taken this translation into account.
 - 11 Or, more precisely, regularities formally equivalent to these, which yet are not empirically manifest (see below). In this sense it is perhaps better to speak of regularities of the form 'whenever conditions x then outcome y '.
 - 12 There are, however, some oscillations in his conception of 'laws' (see Graça Moura 2002).
 - 13 As Schumpeter puts it elsewhere, 'Pareto accepted [the *homo oeconomicus*], comforting us with the assurance that another time we would consider the *homo religiosus*, the *homo eroticus*, and so on in turn. This, however, only shows up a very serious limitation [of 'rational models']. Even if I could—I cannot—visualize the laborious life of a *pure homo eroticus*, any attempt at combination of all those homines would only serve to show, by the absence of any rational rule of combination, that life is ontologically irrational, at least as much as "nature"' (Schumpeter 1991b: 336–37).
 - 14 Before moving on to this third approximation, Schumpeter discusses 'a few other facts' that he also wants to take into account—e.g., the fact that each wave is affected by the results of previous evolution, rather than starting from a perfect equilibrium. Clearly, these facts have been discarded earlier so as to achieve a closed system formulation. And Schumpeter (ibid.: 157) still insists that, through all qualifications, 'we must hold on' to the assumption that the wave starts from a neighbourhood of equilibrium.
 - 15 Two examples: '[T]he exasperating slant of some of us towards "Grundlagenforschung" [is]... out of place in a science still in the pioneer stage' (Schumpeter 1989a [1934]: 130). 'Hundreds of books about methods appeared in Germany, and out of a hundred articles on economics in periodicals I have counted thirty-six of a methodological nature. That is dreadful. There may be some good ones, but these are cancelled by the waste of energy. If you are a good economist you ought to work on economic problems, not take up the work of the logician. If epistemology provides for our needs, why go out of the way to enter a region not our own and deal with questions we are just as well without?' (Schumpeter 1991a: 291–92).
 - 16 'Roughly up to the middle of the 19th century', Schumpeter (1989c [1949]: 275) writes, 'the evolution of "science" had been looked upon as a purely intellectual process... Marx was the first to turn this relation... into a relation of dependence of [science] on the objective data of the social structure and in particular on the social location of scientific workers that determines their outlook upon reality and

hence what they see of it and how they see it. This kind of relativism . . . spells a new philosophy of science and a new definition of scientific truth. Even for mathematics and logic and still more for physics, the scientific worker's choice of problems and of approaches to them, hence the pattern of an epoch's scientific thought, becomes socially conditioned - which is precisely what we mean when speaking of scientific ideology rather than of the ever more perfect perception of objective scientific truths'.
17 In Schumpeter's words, 'economic theory is a technique of reasoning' and 'such a technique is neutral by nature'; indeed, 'a piece of ground on which it [is] possible to build objectively scientific structures' (Schumpeter 1954a: 884). '[T]he rules of procedure that we apply in our analytic work are almost as much exempt from ideological influence as vision is subject to it. Passionate allegiance and passionate hatred may indeed tamper with these rules. In themselves these rules, many of which, moreover, are imposed upon us by the scientific practice in fields that are little or not at all affected by ideology, are pretty effective in showing up misuse. And, what is equally important, they tend to crush out ideologically conditioned error from the visions from which we start. It is their particular virtue, and they do so automatically and irrespective of the desires of the research worker' (ibid.: 43).

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Abstract

This paper assesses Joseph Schumpeter's agenda for the integration of theory and history. On the basis of a critical realist conception of the nature of historical theory it is argued that Schumpeter's aims are at odds with his analytical strategy: his implicit ontology cannot be reconciled with his conception of theory. An illustration is provided as to how this mismatch is reproduced in Schumpeter's substantive attempts to integrate theory and history, and brief reflections are offered as to why this mismatch arose and endured.

Keywords

Schumpeter, historical economics, methodology, ontology, critical realism

