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THE INFLUENCE OF CRITICAL REALISM ON MANAGERIAL PREDICTION

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The influence of critical realism on managerial prediction

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Abstract

In this conceptual paper we suggest that a critical realist perspective alters managerial views on prediction and strategy formation. For that purpose, we review alternative notions of prediction in economic theory in the light of critical realism. In addition, we review the role of prediction in alternative streams of thought on strategy formation. The corollary of such a literature review is a new conceptualisation of the organisational environment as an open system which is not prone to prediction but can be a source of learning for strategists. Such critical realist view alerts researchers and practitioners for the provisional nature of scientific explanations and the dangers of premature prediction. Critical realism is thus a promising avenue for further research on strategy formation and more appropriate strategizing.

Keywords: critical realism, prediction, economic modelling, strategy formation

Introduction

Critical realism is a perspective within the philosophy of social science that has been subsequently applied to the study of economics and management – see, for example, Lawson (1997, 2003) or Fleetwood (1999). According to the critical realist view, social systems are dynamic open systems, constituted by internally related phenomena. This means that constant regularities, such as those constructed under laboratory conditions in the natural sciences, are not ubiquitous in the social realm. Therefore, prediction of events is not possible in the social realm in the same sense as in the natural sciences. This topic has received much attention recently in light of the Global Financial Crisis, and the role played by mathematical economic and financial models of prediction, which have failed to predict successfully the macroeconomic situation before the crisis, during the crisis, and after the crisis – see, for example, Lawson (2009) for a discussion.

The fact that prediction of events is not possible in the same sense as in the natural sciences clearly has implications for strategic management, which must be informed by a clear understanding of the global macroeconomic environment. In this paper, we argue that economic and strategic management theory would benefit from the critical realist view, particularly in terms of how the environment is conceived in the process of strategy formation.

The following section thus introduces the role of prediction in economic theory. The third and fourth sections discuss the inappropriateness of deductivist models to study economic phenomena as open and closed systems, respectively. The fifth section contrasts isolation in deductivist models and abstraction in critical realist explanations. The sixth section applies such a critical realist view of prediction in economic theory to strategy formation. The final section discusses the implications of a critical realist view for strategy formation in theory and in practice.

The role of prediction in economic theory

> “My article [on the nature of the firm] starts by making a methodological point: it is desirable that the assumptions we make in economics should be realistic. Most readers will pass over the opening sentences (Putterman omits them when reprinting my article),

> and others will excuse what they read as a youthful mistake, believing, as so many modern economists do, that we should choose our theories on the basis of the accuracy of their predictions, the realism of their assumptions being utterly irrelevant. I did not believe this in the 1930s and, as it happens, I still do not." (Coase 1993: 52)

The methodological perspective that Ronald Coase is criticising here is the view normally attributed to Milton Friedman (1953:21), who argues that "the ultimate goal of a positive science" is the prediction of "phenomena not yet observed", and that models can (even should) be unrealistic as long as they predict. Friedman's methodological perspective was criticised by prominent economists like Paul Samuelson (1963), amongst others, advocates that successful prediction, whilst a necessary condition for successful economic practice, is not a sufficient condition.

This is an old debate in economic theory, but it has not been addressed by subsequent mainstream economists with the same degree of detail as Friedman and Samuelson did. Furthermore, the issues at stake in this debate have been brought back to the forefront of economic and public debate in light of the Global Financial Crisis, which raised again the appropriateness of the methods employed by economists, which were to a great extent defined during the mid-twentieth century, under the influence of Friedman and Samuelson – see Lawson (1997, 2003, 2009).

In fact, contemporary mainstream economists still employ the methods advocated by Friedman and Samuelson long ago, and the only question that emerges within the mainstream community when employing those methods concerns whether they are used in an instrumentalist perspective as Friedman advocated, or in a realist perspective as Samuelson advocated (albeit inconsistently) – see Lawson (1997, 2003) for a discussion. We will address this debate here, but emphasising a perspective that is present in some economic methodology literature under the heading of *critical realism* (e.g. Lawson, 1997; Martins, 2013). Essential to our argument is a rejection of the idea that successful prediction of events is necessary or indeed always possible. Most critics of Friedman have failed to question these premises. However, we shall argue that for successful modelling endeavour, successful prediction of events is neither necessary nor sufficient. It is also the case that prediction of events is only achievable under certain very special conditions. It is only when we realise that prediction of events is not necessary, and sometimes impossible, that we can properly address (and refute) Friedman's claim that it is sufficient.

Prediction is often identified as the main goal of science. This concern with prediction figures prominently within economics too. In his 1953 essay "The Methodology of Positive Economics", Milton Friedman argues:

> "The ultimate goal of a positive science is the development of a 'theory', or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed. Such a theory is, in general, a complex intermixture of two elements. In part, it is a 'language' designed to promote 'systematic and organized methods of reasoning'. In part, it is a body of substantive hypotheses designed to abstract essential features of complex reality." (Friedman 1953: 21)

One of the implications that Friedman draws from his assessment that prediction is the only purpose of economic modelling is that the "assumptions" (the "substantive hypotheses designed to abstract essential features of complex reality", 1953: 21) underlying the abstract models must be "false", in the sense that models and theories are only instruments. But note that to be coherent with the idea that theories are only instruments, one should say that it does not matter whether theories are true or false, instead of saying that they are "false".

Friedman's choice of the term "false" seems to imply that the world must be more complex than any "simple" and "fruitful" theory, which are Friedman's (1953: 23) criteria for model selection. Most economists disagree with Friedman when he argues that prediction of events is a *sufficient* condition for economic modelling. Samuelson, one of the economists who criticised Friedman's position, argues:

> "[U]nrealistic, abstract models often prove to be useful in the hint of […]regularities. This psychological usefulness should not be confused with empirical validity. […] Such abstract models are like scaffolding used to build a structure; the structure must stand by itself. If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies" (Samuelson 1963: 236).

According to Samuelson, one can use Friedman's models to find a "structure", but one has also to "jettison" those models after using them, if they contain empirical falsities. Samuelson did neither focus on the fact that Friedman regarded prediction of observable events as the "ultimate goal" of economics, nor criticise the use of Friedman's false models.

Most of the critiques of Friedman's position within the field of mainstream economics share Samuelson's concerns. Most mainstream economic practice subscribes the "abstract models", typically mathematical models, advocated by Friedman. Also, even though many economists agree with the claim that prediction of events (using mathematical models) is a necessary condition for economic modelling, few would agree that prediction of events is sufficient for economic modelling.

In order for prediction of events to be possible, a model of the economy must establish constant and exact relations between events. We will use the expression *deductivist models* to denote models for which regularities of the form 'if event X then event Y' are a necessary condition. Notice that in many cases these mathematical regularities are given a stochastic form. Nevertheless, even in those cases the model will still consist in a functional relationship of the form 'if event X then event Y' (combined with some stochastic component, represented through a random variable) and hence the model still assumes a deductivist form.

In the following section we will argue that if the sort of abstract models that both Friedman and Samuelson suggest are *deductivist* models (of which mathematical models are an example), then they are inappropriate in open systems. In the fourth section, we will argue that Samuelson's methodology can never perform better than Friedman's in closed systems. To make this argument, we will refer to the distinction between open systems and closed systems.

Deductivist modelling in open systems: events vs. underlying structures

According to Tony Lawson (1997, 2003), closed systems are systems in which constant conjunctions of the form "whenever event X then event Y" occur. Systems where these constant conjunctions do not always occur, on the other hand, are open systems.

Deductivist models are the most common type of models in modern economic analysis. These models rely on the presence of observable constant conjunctions, that is, presuppose “closed systems”. Afterwards, one can obtain predictions of events using these models.

Now, as Tony Lawson (1997, 2003) notes, a problem with this deductivist methodology is that these exact regularities (which we intend to model) do not typically occur in economics. In the natural sciences, deductivist models are used with great success. But this is so because natural scientists put much effort into creating an experimental arrangement where a closed system will be generated, so that deductivist models can be applied to the regularities that are generated in the experiment. These experimental set-ups are artificially built in order to trigger and thus identify the underlying causal mechanisms, structures or tendencies that drive the observed events – and the underlying causal mechanisms, structures or tendencies can be identified using deductivist models only because a closed system was first generated.

Once the underlying causal mechanisms, structures or tendencies are identified, it is possible to predict their effects. But outside the conditions of the experimental situation, the relevant conception of reality is that of an open system, where the exact regularities that deductivist models presuppose are not ubiquitous; the effects of underlying causal mechanisms, structures or tendencies will not necessarily be manifest in an exact way due to other countervailing tendencies that may be at play out of the experimental situation.

In Friedman’s conception, science is only concerned with prediction of events, or “phenomena *not yet* observed” (1953:21), without providing any realist account of the underlying structures and mechanisms that cause the events we observe. According to Lawson’s conception, on the other hand, the aim of economic explanation is not the prediction of events, but rather the identification of the real structures, powers, mechanisms and tendencies that cause the events we observe – something that natural scientists achieve through experimental control. Hence, in Lawson’s realist methodology, theories and models are about real structures, powers, mechanisms and tendencies that cause events. So if any prediction is possible, it is about underlying structures, powers, mechanisms and tendencies, not about events (or “phenomena not yet observed”).

Notice that this concern with underlying causes seems to be in accordance with Samuelson's (1963: 236) idea that deductivist models are useful to "build a structure", where this structure "must stand by itself". But note too that in economics, and in the social sciences in general, it is not possible to create laboratory conditions with the same precision as in the natural sciences. In the natural sciences, deductivist models can always be employed as long as a closed system situation can be artificially created in a laboratory.

In economics, however, since a closed system will very hardly be created in a laboratory situation, it will become very difficult to use deductivist models, unless the closure conditions are already present in the analysed phenomena. Thus, *when in the presence of open systems* (i.e., when constant conjunction of the form "if event X then event Y" are not ubiquitous), the deductivist methodology both Friedman and Samuelson employ is inappropriate.

To reject Friedman's position, however, one must first show that: (i) the social realm (or at least a significant part of it) is an open system; and (ii) the social realm is an open system in which closed systems cannot be generated (for example, in an experimental arrangement) in the same way as in the natural realm. We will turn to these issues now.

Proposition (i) follows from observing a generalised feature of experience: that regularities of the sort 'if X then Y' are not ubiquitous in the social realm. In fact, they are not ubiquitous in the natural realm either (with the notable exception of celestial mechanics), and that is why an experimental arrangement must be generated under laboratory conditions so that deductivist methods can be applied. There are, of course, *some* closures in the social realm (as there are also in the natural realm), and there are therefore some cases of success in using deductivist models in economics without experimental control whenever these closures are present – on which see, for example, Engle et al. (1997). But many interesting situations for scientific analysis (perhaps the *most* interesting cases) constitute open systems.

Proposition (ii) springs from the fact that, unlike natural sciences, social sciences deal with a type of material that cannot be isolated in a closed system without it losing properties that are essential to explain social behaviour. One of the reasons for this is that human beings do not always follow laws of behaviour like natural phenomena, and always have the possibility of choice – that is, any action could have been otherwise. Another reason is the existence of *internal relations*. According to Lawson (2003: 17):

> "[T]he social realm is […] highly internally related. Aspects or items are said to be internally related when they are what they are, or can do what they do, by virtue of the relations in which they stand".

If we are modelling a system of isolated atoms, to regard a given part as (to some extent) independent from others can lead us to a model that captures the essence of the analysed phenomena – for the object of analysis will be *per se* already constituted of isolated parts, closed to external influences, and so deductivist models can be successfully used.

But if our object of analysis consists of *internally related phenomena* (whose interrelations within its constitutive parts are essential to our understanding of the phenomena), to isolate some aspects from others that are influencing the latter (in a laboratory situation, for example), will lead to a model that ignores fundamental relations within the analysed phenomena. Furthermore, the whole may display properties that "emerge" out of the parts and their relations, but are irreducible to the parts regarded as isolated. Elsewhere, Lawson writes:

> "Emergence may be defined as a relationship between two features or aspects such that one arises out of the other and yet, while perhaps being capable of reacting back to it, remains causally and taxonomically irreducible to it." (Lawson, 1997: 63).

Thus, to model parts of a highly internally related whole as if they were isolated will not deliver us a model that captures emergent properties. In an open system, which comprises internally related and emergent phenomena, economic analysis will very hardly find underlying structures and tendencies using deductivist models or any sort of isolationist procedure. Since the existence of internal relations and emergent properties does not allow us to isolate a fragment of the phenomena to be analysed (for example, under some experimental arrangement), then a closed system cannot be generated. And hence, a *deductivist* methodology will not be appropriate.

When in the presence of open systems, internal relations and emergence (where the two latter features make it very hard to create a closed system under laboratory conditions) there is no reason to believe that prediction of event regularities (as Friedman puts it, of “phenomena not yet observed”) can be done.

The core of most critiques of Friedman’s position (including Samuelson’s critique) agrees in that prediction of events using deductivist models is a necessary condition for economic modelling (deductivist models and prediction of events are essential to economic analysis), but not a sufficient condition for economic modelling. But the more fundamental critique of Friedman, and of his instrumentalism, is rather that *prediction of events might not even be a necessary condition for economic analysis*, for closed systems are not ubiquitous.

Deductivist modelling in closed systems: correlation vs. explanation

If, for some reason, the analysed sphere of reality spontaneously constituted, or at least approximated, a closed system, even without it being subject to experimental control, deductivist procedures based on closure assumptions could be undertaken – constant conjunctions could be modelled by means of correlation analysis.

Edward Leamer (1985), for example, proposed a methodology of analysis with the purpose of testing whether the relations between the variables we are interested in are “robust” (as opposed to “fragile”) enough to changes in a subset of other variables (this procedure is known as “Extreme Bounds Analysis”), since “[a] fragile inference is not worth taking seriously” (Leamer 1985: 308). If the change in the subset of the latter variables does not affect significantly the coefficients associated with the variables of interest, then the correlation is said to be “robust”. This can be interpreted as a test of how robust is a closure, i.e. testing the extent to which the observed correlations in the variables of interest can be viewed as independent from other variables (since we cannot insulate them in a laboratory).

It is worth noting that even though all “robust” relations constitute a closed system, not all closed systems are necessarily “robust”. A system that is insulated from everything else in a laboratory situation may be nevertheless extremely fragile out of the laboratory situation (of course that once it is insulated, it is then “robust” in the sense that changes in what it was insulated from will not affect the artificially insulated system). In economics one can rarely obtain a laboratory situation, and hence Leamer uses the stronger concept of robustness, which can be interpreted as meaning that closure conditions already hold in the analysed phenomena without laboratorial manipulation.

Now, an important question in this situation, where closure and empirical observable regularities are *already present* in the analysed sphere of reality, is whether we can develop criteria to choose between competing “more complicated and realistic hypotheses” (that we can entertain using a deductivist framework, as Samuelson suggests) on the one hand, or Friedman’s methodology on the other hand.

Can we identify underlying causal mechanisms when using deductivist models, in a reality that displays empirically observable closures but cannot be subject to laboratory control? In the natural sciences, one can construct different experimental arrangements where causal powers can be triggered or not, and the underlying structures can be identified because of the different observations that arise under different experimental settings. But if, on the other hand, we only observe regularities of the sort ‘if X then Y’ in a closed system (that cannot be subject to experimental control in the same way as in the natural sciences), what can we call a “causal relation” apart from this correlation? And what can our scientific theory consist in apart from these correlations?

Zellner (1979), in a paper titled “Causality and Econometrics”, quotes Feigl as an account of a “philosophical definition of causality” in the context of econometric analysis, arguing that “[t]he clarified (purified) concept of causation is defined in terms of *predictability according to a law* (or more adequately, to a set of laws)” (Feigl, 1953: 408, emphasis in original).

This reinforces the idea that when it comes to applying economic models in order to uncover causal mechanisms presupposing that a closed system is *already present* in the analysed sphere of reality, all that can actually be done is to define “causality” as the correlation between y and x, through a mapping f. In this sort of analysis, not much more can be said apart of this correlation. This correlation exhausts what we define as “causality” in a “natural” closed system.

Besides, Feigl's account of causality also entails that *causality* will be *predictability*, which is precisely Friedman's criterion. Thus, under the assumptions that are implicit in the deductivist models that both Friedman and Samuelson employ, and in a sphere of reality that constitutes a closed system and is not subject to experimental control, Samuelson's methodology cannot perform better than Friedman's, since in such a case "causal" explanation cannot go beyond correlation of events towards the uncovering of structures (as Samuelson suggests).

Even if we assume causality to be a mapping *f* of x to y, and thus assume that something can be said about "causes" in this sense, Friedman's "abstract" models would lead to the same "causal relations" (i.e. correlations) as Samuelson's "abstract" models. Thus, prediction of events is possible using mathematical deductivist methods in the presence of closed systems, but under such conditions economic explanation will not be able to go beyond Friedman's methodology: we cannot undertake causal explanation under such conditions, unless by "causal" explanation we mean event correlation.

From correlation of events to explanation of underlying structures

The question remains as to what alternatives are there for economic modelling. Amartya Sen argues:

> "[…] it is necessary to consider the distinction between realism in the sense of 'nothing but the truth' and that in the sense of 'the whole truth'. An assumption can be realistic in that it is true without the claim being made that it is exhaustive in capturing all aspects of the reality. Advocates of realism in the sense of 'nothing but the truth' need not demand 'the whole truth'. The dissatisfaction with Friedman's position on the part of critics such as Samuelson does not arise from Friedman's rejection of 'the whole truth', but from his rejection of 'nothing but the truth'." (Sen, 1980:358)

This idea of departing from 'the whole truth', but not from 'nothing but the truth', can be interpreted in line with Tony Lawson's notion of abstraction. Abstraction (as defined in Lawson, 1997, 2003) consists in focusing on one aspect while leaving other aspects aside momentarily, but *without supposing* that other things left aside are not playing a role in what is observed. Isolation, on the other hand, consists in picking up a part of the analysed reality *while supposing* that other things left aside are not playing a role in the part of reality we are analysing.

Now, if the parts one is treating as isolated are *not* in fact isolated (for example, when they are *internally related*), to see them as such becomes a *fictionalising* exercise: since one would not account for the fact that other aspects of reality left aside are playing a role in the analysed sphere of reality, our description of such sphere of reality would then be a fiction – and a departure from 'nothing but the truth'. But through abstraction, one can focus on some aspects of reality in order to understand such sphere of reality without assuming that these aspects are isolated from all others – departing from 'the whole truth', but not from 'nothing but the truth'.

Lawson's notion of abstraction seems to be an appropriate starting point for modelling when in the presence of open systems. This is an alternative to Friedman's and Samuelson's "abstract [deductivist] models", which are actually "fictions" rather than "abstractions" in Lawson's sense, as both Friedman and Samuelson recognise when saying that they are "false" or may contain "empirical falsities".

Now, which features of reality should be abstracted, selected or chosen in order to initiate an explanatory procedure? Lawson suggests starting from demi-regularities, which are defined as partial regularities over a definite region of time-space. For Lawson, analysing such demi-regularities (and contrastive demi-regularities in particular) is essential for the process of explanation.

But this does not yet answer to the question of how one can identify underlying causal factors of a given phenomenon, if the sphere of reality under analysis constitutes an open system that cannot be subject to experimental control. How can one move from abstractions and demi-regularities towards causal explanation of structures, powers, mechanisms and tendencies?

Lawson argues that even though an experimental arrangement might prove impossible in the social realm, *conditions similar to those of an experimental arrangement might nevertheless occur in a given sphere of reality*. In the same way that a causal structure, power, mechanism or tendency is insulated in an experiment (and hence identified), it may be that, under some circumstances, a given causal mechanism is observed at play while relatively insulated from other causal mechanisms.

Lawson (2003) gives the example that the yield of a given crop field may *contrast* with the yield of the other fields. This contrast may be the result of the fact that a given causal mechanism (or set of causal mechanisms) was at play on that crop field, but not on the other crop fields. Hence, a causal mechanism was at play while relatively insulated from other causal mechanisms, and could therefore be identified.

Lawson argues that two ingredients are essential for contrast explanation: "an informed (if often tacitly formed or implicit) judgement about conditions operating over the contrast space" (Lawson 2003:92); and "a relation between outcomes within the contrast space is eventually recorded that is regarded by the researcher (or whoever) as surprising or in some way of concern or interest" (Lawson 2003:93).

There is of course an important difference between experimental analysis and contrast explanation. While the experiment is forward looking, contrast explanation will be backward looking. In an experiment the scientist actively constructs an arrangement that will insulate a given causal mechanism. But when using contrast explanation, one must wait and hope that an interesting and surprising contrast (and contrastive demi-regularities) will arise (generated by the fact that a causal mechanism became somehow relatively insulated), so that the underlying causal mechanism (or set of causal mechanisms) that generated the contrast can be identified. Given the difficulty of constructing experimental arrangements in the social sciences (such as economics), contrast explanation seems to be the best methodological procedure available that allows us to observe a causal mechanism relatively insulated in an open system.

The role of the environment in strategy formation

The conception of social reality we adopt, and in particular our conception of economic reality, has implications for the methods adopted when studying it, and when navigating within it. The approach to management we follow depends heavily on our ability to identify causal mechanisms at play within the organisation, and within the environment faced by the organisation. If we accept that the economic environment faced by the company is an open system, then we must take this fact into consideration in strategic choices. Strategy formation must deal with a wide range of organisational and environmental factors in an open system where events cannot be predicted with certainty, and all we can achieve is knowledge of underlying structures, powers, mechanisms and tendencies which contribute to produce a given outcome.

This clearly means that strategy assessment must focus on long term processes where underlying structures, mechanisms and tendencies which are present in the organisation and in the environment, become manifest over time, rather than on specific events which cannot be predicted. It also means that a particularly useful methodological procedure available consists in case studies where causal mechanisms at play within an open system can be identified through contrast explanation. As Lawson (2003) notes, contrast explanation is the best methodological alternative to laboratory experiments available when it is not possible to generate artificially a closed system in order to apply mathematical-deductivist methods.

Of course, when engaging in case study research, or indeed in any type of research, we must possess some previous conception of the processes under study. In a complex open system, it is impossible to address reality without some explanatory framework that guides us in our initial approach to reality. The framework used may, of course, turn out to be inaccurate, or in need of revision, when confronted with the facts. Scientific progress consists precisely in the transformation of existing conceptions. But some previous conception is needed when addressing reality.

In an extensive review of over 2000 publications in the field of strategic management, Mintzberg et al. (2009) distinguish between ten schools of thought on strategy formation (Table 1 below), which point towards different approaches to the problem of prediction.

Table 1. Schools of thought on strategy formation (adapted from Mintzberg et al., 2009)

Stream of thought	Strategy formation	Observations
Design school	Conception process	Prescriptive, 1960s
Planning school	Formal process	Prescriptive, 1970s
Positioning school	Analytical process	Prescriptive, 1980s
Entrepreneurial school	Visionary process	Descriptive, individual

Cognitive school	Mental process	Descriptive, individual
Learning school	Emergent process	Descriptive, organisational
Power school	Negotiation process	Descriptive, organisational
Cultural school	Collective process	Descriptive, organisational
Environmental school	Reactive process	Descriptive, incremental
Configuration school	Transformation process	Descriptive, quantic

The design school regards strategy formation as a process of conception in which environmental opportunities and threats are scanned, forecasted and matched with internal strengths and weaknesses, leading to 'play to your strengths' strategies that are rationally evaluated, selected, and implemented (e.g. Christensen et al., 1982). The planning school attempts to formalise such strategy formation endeavour as a process of near 'paralysis by analysis' (e.g. Ansoff, 1965). The positioning school also tries to match the environment and the organisation, but focuses on content rather than process, conceiving strategy formation as a rational choice between alternative generic strategies (Porter, 1980).

These first three schools – design, planning, and positioning – are prescriptive in the sense that they prescribe rather than describe a certain approach to strategy formation. The seven remaining schools, by contrast, describe strategy formation in practice rather than prescribing it.

The entrepreneurial and cognitive schools focus on the individual level of analysis. The entrepreneurial school regards strategy formation as a visionary process by which the entrepreneur is able to anticipate change, whereas the cognitive school considers strategy formation the result of individual mental processes by which information is processed.

The learning, power, and cultural schools, by contrast, focus on the organisational level of analysis. The learning school describes strategy formation as an emergent rather than linear process, in which doing does not follow but overlaps with thinking. The power school focuses on processes by which strategy is negotiated, whereas the cultural school regards strategy formation as a consequence of shared values.

The last two schools – environmental and configuration schools – focus on the rhythm of change. The environmental school emphasises evolutionary change in reaction to the environment, whereas the configuration school stresses quantic transformation based on insights of all schools.

Mintzberg et al. (2009: 302-303) claim that in the ten schools of thought the environment is conceived rather vaguely, either as a factor or as the actor of strategy formation:

> "Among the actors at centre stage of the schools so far discussed – the chief, the planner, the brain, the organization, and so on – one has been conspicuous by its absence. That is the set of forces outside the organisation, what organisation theorists like to call (rather loosely) the 'environment'. The other schools see this as a factor; the environmental school sees it as an actor – indeed *the* actor. (…) What, then, is this thing called 'environment'? Not much, in fact, even here. It is usually treated as a set of vague forces 'out there' – in effect, everything that is not organisation."

The design school, "without question, the most influential view of the strategy-formation process" (Mintzberg et al., 2009: 24), regards the environment as a factor of strategy formation. This school is associated with two books at the University of California (Selznick, 1957; Chandler, 1962) as well as with the General Management group at the Harvard Business School (Learned et al., 1965; Christensen et al., 1982; Rumelt, 1997; Hambrick and Fredrickson, 2005).

Common to these contributions is the assumption that organisational strengths and weaknesses should fit environmental opportunities and threats, establishing the basis for creation, evaluation and choice of strategy. According to Rumelt (1997), a key test in the evaluation of strategy is 'consonance', that is, the strategy must represent an adaptive response to the external environment and to the critical changes occurring within it. Popular analytical tools such as the SWOT and PEST frameworks are thus inspired by the design school, which also assumes "foreseeable change in the social, political, and macroeconomic context" (Christensen et al., 1982: 179-80).

Such emphasis on forecasting implies that the design school holds more optimistic assumptions concerning managerial prediction than practitioners (e.g. World Economic Forum, 2014). In fact, Mintzberg et al. (2009: 33) consider that one of the key assumptions of the design school, a sharp distinction between strategy formulation and implementation, follows classical notions of rationality which separate thinking and acting. The authors question this assumption by claiming that the environment is not sufficiently predictable or stable to allow such a distinction (Mintzberg et al., 2009: 43):

> "The external environment is not some kind of pear to be plucked from the tree of external appraisal. It is, instead, a major and sometimes unpredictable force to be reckoned with. Sometimes conditions change unexpectedly so that intended strategies become useless. (…) Behind the very distinction between formulation and implementation lies a set of very ambitious assumptions: that environments can always be understood, currently and for a period of well into the future, either by the senior management or in ways that can be transmitted to that management; and that the environment itself is sufficiently stable, or at least predictable, to ensure that the formulated strategies today will remain viable after implementation."

According to Mintzberg et al. (2009: 28-29), the design school provides the basis for other schools which elaborate specific aspects of strategy formation such as formality in the planning school, analysis in the positioning school, and adaptability in the learning school. In the planning school, the environment is regarded as a set of economic forces such as industry, competition, and market. In the positioning school such a view becomes more deterministic, since managerial choice is confined to a small set of generic strategies (Porter, 1980).

In the cognitive school, the environment is regarded as a source of confusing signals for the strategist which are too complex to be understood. Such complexity is also present in the learning school which emphasises active experimentation rather than passive reaction. In the remaining schools "the environment has tended to be absent, incidental, or at least assumed" (Mintzberg et al., 2009: 303), with the exception of the environmental school which makes the environment the main actor of strategy formation.

Within the environmental school, at least three different views can be distinguished: contingency theory, population ecology, and institutional theory. Contingency theory (e.g. Donaldson, 2001) postulates that the 'one best way' of classical management theory should be replaced by the 'it all depends' view of organisations based on contingencies such as size, technology, competition, and environmental stability. In order to systematise the match between different situations and respective behaviours, contingency theory describes the environment in terms of dimensions and delineates organisational responses in terms of structure (e.g. Pugh et al., 1969) and strategy (e.g. Miller, 1979). Mintzberg (1979), for instance, synthesises environmental dimensions into four main characteristics: stability, complexity, diversity, and hostility.

Population ecology (e.g. Hannan and Freeman, 1977) shares the focus of contingency theory on the environment, but assumes that actions of managers in the early life of an organisation create inertia that subsequently reduces their freedom to adapt. External pressures towards inertia include, among others, barriers to entry and exit, access to information, resistance to change, and collective rationality. Especially important is the notion of ‘fixed carrying capacity’ borrowed from biology, according to which an industry has a finite amount of resources dictating the ‘survival of the fittest’ among its member organisations.

From this perspective, strategy is a process of continuous adaptation which by accident can lead to success based on efficiency in maximising environmental fit or flexibility in reserving excess resources for the future. As a result, the adolescence of an organisation tends to be problematic since it lacks the flexibility of earlier days and the resourcefulness of older organisations. Such problems are labelled liabilities (e.g. Henderson, 1999), namely of adolescence, newness and smallness. According to Mintzberg et al. (2009: 309): “Some liabilities will only occur under certain circumstances, whereas in other cases liabilities may actually compete for influence. The interaction of liabilities can therefore be complex and unpredictable, which from a managerial point of view makes population ecology rather limited in usefulness”.

Institutional theory is also focused on environmental pressures, but they are regarded as norms that organisations must comply with, rather than sources of entropy. In particular, the environment is regarded as a repository of economic (tangible) and symbolic (reputational) resources that protect the organisation from uncertainty in its environment (e.g. loss of credibility). In order to accumulate such resources, organisations thus tend to adopt similar structures and practices through ‘institutional isomorphism’ (Meyer and Rowan, 1977) of coercive (e.g. regulations), mimetic (e.g. imitation) or normative (e.g. professional norms) nature.

The role of managerial prediction in strategy formation is therefore present in at least six of the ten schools of thought presented by Mintzberg et al. (2009). In the design and planning schools the environment is regarded as a set of general yet predictable forces such as political, economic, social, and technological opportunities and threats (e.g. Christensen et al., 1982). In the planning school the level of analysis is reduced from macro to meso, focusing on aggregated forces at the industry level such as competitors, customers, and suppliers (e.g. Porter, 1980).

In addition to these three prescriptive schools, the environment is present in three descriptive schools: cognitive, learning, and environmental. In the cognitive school, the level of analysis is individual and therefore the environment is regarded as a source of informational ambiguity for the individual strategist. In the learning school, the level of analysis is organisational with the environment being regarded as a source of complexity. The environmental school shares this view of the environment as characterized by a certain degree of complexity, in addition to other dimensions such as stability, diversity, and hostility (Mintzberg, 1979).

In addition, the environment can be a source of inertia (Hannan and Freeman, 1977) as well as of coercive, mimetic, and normative 'institutional isomorphism' (Meyer and Rowan, 1977). Table 2 below synthesises the role of the environment in strategy formation.

Table 2. Schools of thought on strategy formation (adapted from Mintzberg et al., 2009)

Stream of thought	Conception of environment	Observations
Design school	Predictable opportunities and threats at the macro level	Prescriptive, 1960s
Planning school	Predictable opportunities and threats at the macro level	Prescriptive, 1970s
Positioning school	Aggregated forces at the industry level	Prescriptive, 1980s
Cognitive school	Source of informational ambiguity at the individual level	Descriptive, individual
Learning school	Source of complexity at the	Descriptive, organisational

	organisational level	
Environmental school	Source of complexity, stability, diversity, hostility as well as inertia at the organisational level and institutional isomorphism at the meso and macro level	Descriptive, incremental

Due to its focus on agency-structure interplay, the critical realist conception of the environment appears to be closer to the assumptions of the cognitive and learning schools of strategy formation. By contrast, the extreme notions of prediction and control on the one hand, and of passive reaction on the other, implicit in the three prescriptive schools of strategy formation – design, planning, and positioning – and on the three variants of the environmental school, respectively – contingent, ecological, and institutional – appear to be less aligned with a critical realist view of the environment.

In fact, if the world is an internally related open system, it is only natural that we must not only take an holistic view of the organisation and of the environment, but also an integrated view of both. By focusing on how the organisation adapts to the environment by incorporating knowledge of the environment and acting upon it, the cognitive and the learning school capture the relational and open nature of social reality.

Of course, to acknowledge the relational and open nature of social reality means also to acknowledge that exact prediction of events is not possible in the same way as in the natural sciences where closed systems can be obtained in laboratory experiments (or exist spontaneously as in astronomy). The best guide to strategy formation under those circumstances consists in the study of the underlying structures, mechanisms and tendencies that cause events. In the case of the cognitive and learning school, the focus is on psychological, social and cultural mechanisms through which knowledge-processing takes place. It is not possible to predict events using this method, but it provides a more solid basis for strategy formation than the attempt to use methods that presuppose closed systems in an open system.

Conclusion

Samuelson argued long ago that economic models should uncover "more realistic and *complicated* hypotheses". It could be the case that what Samuelson refers to as "more realistic and *complicated* hypotheses" (emphasis added) would be regularities that are not immediately empirically observable. The vision that Samuelson presupposes is not one where empirically observable regularities exhaust the object of economic analysis, but rather one where the world is a complex mixture of different tendencies and causal relations that we must uncover using our "abstract models".

Friedman also argues that "a hypothesis is important if it 'explains' much by little, that is, if it abstracts the common and crucial elements from the mass of *complex and detailed circumstances* surrounding the phenomena to be explained" (Friedman, 1953; 26, emphasis added). And when Friedman claims that assumptions must be "false" and "simpler" than the "complex" phenomena they analyse, he also seems to be making this point: assumptions are "false" because the world is "complex" and the assumptions are "simple". So there is more to the real world than observable empirical regularities.

It seems therefore that both authors (Samuelson and Friedman) agree that the world has underlying structures, tendencies and causal relations that are more complicated than what a deductivist model can represent. The divergence between Samuelson and Friedman lies neither in their conception of the world, nor in the use of deductivist models (which both agree are useful). The difference between both consists in their views of what is the goal of economic analysis: for Friedman, it is successful prediction with false models, while for Samuelson, it is uncovering an underlying structure using these same "empirically inadequate" models.

But it cannot be argued that both Friedman and Samuelson's methodology can uncover structures, powers, mechanisms and tendencies under open systems, apart from those that are continuously actualised and hence empirically observable as constant conjunctions: deductivist models are only appropriate if closure conditions hold.

A different sort of economic explanation consists in causal explanation of underlying structures, powers, mechanisms and tendencies, using partial regularities as a starting point for abstraction and contrast explanation. After recognising this, it is then possible to propose alternatives to Friedman's methodology, and to say that prediction of events is not sufficient, because it is not necessary (and it may not even be possible or desirable) for economic explanation.

Those alternatives are relevant not only for our understanding of the economic environment, but also for our understanding of the organisation which acts in that environment. Underlying structures, powers, mechanisms and tendencies do not always manifest themselves in observable events, but over time they become manifest as long-term partial regularities. This means that fields of management research more concerned with structural and long-term processes are those that can benefit more from this type of analysis. Strategy is, evidently, the field which stands more in need of looking at structural long term-processes, and of integrating the multitude of varied information that the organisation receives.

In order to improve strategy formation, a crucial step is to look beyond the vast amount of information that always emerges in open systems such as the organisation and the environment, and look at the underlying structures and mechanisms through which knowledge of the environment is processed by the organisation. In fact, since contemporary mainstream economists still maintain the deductivist approach advocated by Samuelson and Friedman fifty years ago, despite its inability to explain socio-economic reality in a satisfactory way (on which see Lawson, 2003), it seems that there is much work to be done not only in the study of the organisation, but also in the study of the economic environment.

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