Unpredictability and nonlinearity in complexity theory: A critical appraisal

March 31, 2009 · Philosophy

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Kemp S, Kemp S. Unpredictability and nonlinearity in complexity theory: A critical appraisal. Emergence: Complexity and Organization. 2009 Mar 31 [last modified: 2016 Dec 4]. Edition 1. doi: 10.emerg/10.17357.b3ccd365dec1297285e21c3348693988.

Abstract

This piece explores potential problems with the focus on unpredictability and nonlinearity within complexity theory. Whilst not completely rejecting the application of ideas of nonlinearity and unpredictability within the social sciences, I argue that greater empirical and conceptual care is needed. The arguments made are illustrated by a critical examination of cases from John Urry's Global Complexity, including the dominance of the petroleum-fuelled car in the 20th century and the prevalence of wild-fires in Malibu. Empirically speaking, I argue that claims about particular instances of nonlinearity and unpredictability in the social world must be backed up by appropriate evidence, rather than analysts simply assuming that all social phenomena have these characteristics. Conceptually speaking, I suggest that care needs to be taken to distinguish genuinely unpredictable phenomena from those that are simply poorly understood at the present time. I also argue that predictability should be seen as a matter of degree.

Introduction

It's arguable that the emphasis in complexity theory on unpredictability and nonlinearity did not surprise social scientists, especially social theorists, but reinforced what they already believed. The existence of nonlinear and unpredictable phenomena may have come as a shock to those natural scientists who bought into a (roughly) Newtonian picture of a deterministic universe.[1] But the vast majority of social theorists, and some empirical social scientists, already believed that the social world was not predictable and could not be understood by undertaking linear operations on quantitative information about social phenomena. Defenders of interpretive social thought, (e.g., Winch, 1990 [1958]) social constructionists (e.g., Gergen, 1985), and even quasi-naturalists such as critical realists (e.g., Bhaskar, 1979), have argued against the possibility of scientific prediction in the social domain, and have preferred instead to focus on qualitative change and the richness of unquantifiable meaning. This is not to say that complexity theory adds nothing to previous understandings of the social world, and at the very least complexity theory brings its own vocabulary and explanatory repertoire to account for the social world. Furthermore, complexity theory offers concepts that are held to apply to both the social and natural worlds, rather than arguing for a clear-cut division between the two in the manner of interpretive social thought.[2]

Nevertheless, given the extent of common sympathies between complexity theory and its precursors, it may be that certain shared claims have not been as rigorously interrogated as they might otherwise have been. If many are already convinced that the social world is an unpredictable place, and one in which social changes and developments rarely, if ever, have a 'linear' character, then claims by complexity theorists in these areas may have been given a relatively easy ride. In this article I intend to build on the small amount of work of those who have questioned, to a greater or lesser extent, complexity theory's treatment of prediction and linearity in the social world (see McLennan, 2003, 2006; Thompson, 2004). My aim is not to attempt a demonstration that the social world is, contrary to complexity theory, fully predictable and linear in character. Rather, my intention is to argue two things: firstly, that notions of unpredictability and nonlinearity need to be used in a more careful and analytically precise way then they are in certain existing complexity arguments; and, secondly, that we need to carefully attend to the ways in which empirical cases actually do, or do not, fit in with ideas of unpredictability and nonlinearity as formulated in complexity theory.

Given the limitations of space, I will focus only on the work of one prominent exponent of complexity theory in the social sciences, John Urry, and particularly on his *Global Complexity* (2003). In *Global Complexity*, Urry strongly defends the relevance of complexity theory to the social sciences, and puts it to work in order to account for phenomena usually dealt with under the rubric of 'globalization', as well as applying it to numerous other examples. Urry's book offers a useful basis for discussion both because it makes these linkages between complexity theory and empirical examples, and because Urry draws heavily on ideas of nonlinearity and unpredictability when analyzing the social world. In this article I shall consider his treatment of these concepts in turn, and offer some critical commentary on the extent to which they successfully account for the empirical examples that Urry presents.

Nonlinearity, Small Causes and Large Effects

Although 'nonlinear' is a term used extensively by chaos and complexity theorists, there is some degree of dispute about its meaning. Strictly speaking, 'linear' and 'nonlinear' are terms that are used to distinguish two types of mathematical equation; whereas linear equations can be solved by breaking them down into smaller pieces then recombining, this is not possible with nonlinear equations (see McLennan, 2006: 145; Kiel & Elliott, 1996: 4). However, many less technically-minded discussions of chaos and complexity theory use the term to cover processes where there is something like a lack of proportionality between (changes to) the input and the outcome. One oft-discussed type of case is that of 'sensitivity to initial conditions' in which the minutest difference in the starting conditions of particular kinds of system (such as those whose trajectories are governed by nonlinear equations) may lead to large-scale differences in the state of that system when an extended period of time has elapsed. The most famous instance of this is Lorenz's discovery of the sensitivity of weather systems to initial conditions, often referred to as the 'butterfly effect' (Gleick, 1988). Lorenz argued that the minutest alteration in the initial state of a weather system, such as the tiny amount of turbulence generated by a butterfly flapping its wings, can lead to a vastly different outcome—a storm occurring in a far-away city—than that which would have occurred if the butterfly had decided to have a rest—a sunny day in that city.

In *Global Complexity*, Urry argues that the principle of nonlinearity can be used to explain the emergence and persistence of certain technical forms such as the QWERTY keyboard and the petroleum-fuelled car (Urry, 2003: 54-5). Urry does not offer a rigorous definition of the term 'nonlinear' (see McLennan, 2006)[3], but appears to be drawing on the broad idea that for some phenomena there is a lack of proportionality between inputs and outputs. In the realm of causal explanation this involves the claim that 'there are often massive disproportionalities between causes and effects' such that 'Causation can indeed flow from contingent minor events to hugely powerful general processes' (Urry, 2003: 7, 54). Insofar as this is a coherent notion, it suggests that small, apparently accidental or insignificant causes can come to have a major influence on the development of a system.

We will consider the example of the petroleum-fuelled car rather than the QWERTY keyboard here because its rise and persistence has had arguably more significant consequences. As we know, petroleum came to be the power-source for the vast majority of cars in the 20th century. Looking back at the history of this development, Urry suggests that in the 1890s petroleum was inferior to two other means of powering cars, electricity and steam. In his article 'The "System of Automobility"', Urry discusses what happened next:

> *Petroleum-fuelled cars were established for small-scale, more or less accidental reasons, partly because a petrol-fuelled vehicle was one of only two to complete a "horseless carriage competition" in Chicago in 1896 (Urry, 2004: 32).*

On this view, the petroleum-fuelled car became dominant not because of its technical superiority but through chance events. This dominance was possible because small-scale, contingent causes were magnified through processes of 'lock-in' in which 'an astonishing array of other industries, activities and interests came to mobilize around the petroleum-based car' (Urry, 2003: 55). Once one path of development came to be backed by enough of the players within the industry and in supporting industries, it became extremely difficult to produce competitive alternatives because a whole network of institutions and businesses—such as petrol stations, petroleum producers, and car part manufacturers—came to have a vested interest in the persistence of a particular technological form rather than its opponents. On Urry's view, small scale chance events led to one power-source rather than others becoming dominant and also unchallengeable through nonlinear processes of reinforcement present in institutional lock-in.

I want to make two general kinds of argument about Urry's analysis, one of which relates to the empirical basis for his claims, and the other of which is a more theoretical point. Beginning from the empirical, I would argue that there are problems with Urry's claim that the dominance of the petroleum-fuelled car is the result of 'more or less accidental reasons' (Urry, 2004: 32). For one thing, Urry offers little or no evidence in support of this claim, offering only one reference to back up his historical account in 'The "System of Automobility"' (2004). For another thing, if we look at relevant sources on the competition between different ways of powering cars (Motavalli ,2000; Cowan & Hultén, 1996; Foreman-Peck *et al*., 1995), the picture looks different to the one painted by Urry. The most opposed interpretation to Urry's is that put forward by Foreman-Peck *et al*. (1995) who argue that the petroleum car eventually won out in its contest with other power sources because it had the best match between its technical characteristics and those features that were desired by consumers. As with Urry, Foreman-Peck *et al*. refer to road races and endurance trials for cars, but present these as not minor shows or entertainments, but as important means by which technological advantages could be displayed (Foreman-Peck *et al*., 1995: 15). Although they admit that steam-powered cars out-performed petrol cars on-and-off in contests of speed, they argue that the wider demand was not for the fastest cars but for those that were reliable and convenient, factors which endurance tests were a better proxy for (Foreman-Peck *et al*., 1995: 15-6). Indeed Foreman-Peck *et al*. suggest that the superiority in reliability and convenience of petroleum cars was clearly apparent prior to their mass production, with some notable manufacturers abandoning steam for petrol before this point. They thus challenge the view that the dominance of the petroleum-fuelled car was a product of the chance reinforcement of one power source rather than another through institutional lock-in.

Other writers, such as Motavalli (2000), and Cowan and Hultén (1996), do not challenge the 'lock-in' thesis as a partial means to explain the dominance of petroleum-powered cars over steam- and, particularly, electric-powered cars during the 20th century. Nevertheless, this does not mean that they attribute the dominance of petroleum-powered vehicles to chance, accidental events. For one thing, these writers are clear that there were only three plausible sources of power for cars at the end of the 19th and beginning of the 20th century—steam, electricity and petroleum. Even were it to be established that petroleum had no intrinsic merits over the others, its status as a serious contender at the time does not seem to be challenged. Thus we already have a narrowing of the field of possibility that was not the result of chance or accident per se. Further than this, both Cowan and Hultén and Motavalli acknowledge that at the time the mass-production of cars was undertaken, petroleum-based cars had clear advantages over their competitors. In their analysis, Cowan and Hultén list the problems faced by cars using each of the three power sources at around the turn of the century, and point out that for the petroleum-powered car, all of them had been solved by 1912, except their noisiness. By contrast, both the steam car and electric car seemed to have serious remaining problems—the steam car's consumption of 'immense amounts of water' and the electric car's limited range and low top speed (Cowan and Hultén, 1996: 68). Motavalli, who is extremely sympathetic to alternative, non-petroleum-based ways of powering cars, also acknowledges the limitations of alternative forms of power at the time the petroleum-powered cars were being mass-produced (Motavalli, 2000: 9).

Summarizing these points, I would suggest that the empirical analyses considered here throw Urry's analysis into question. Even if we only accept what is agreed by all of these writers, it does not make sense to say that the dominance of petroleum-fuelled motor cars is due to minor, accidental, factors. Petroleum was one of only three plausible means to power cars at the end of the 19th century, and the major problems with petroleum-powered cars were resolved before those of steam- and electricity- powered cars. As such, Urry's claim that this is an example of a nonlinear complex phenomenon, in which accidental causes lead to hugely significant outcomes, is thrown into question.

My second overall point about Urry's use of this example is that even if he were to challenge other accounts and provide the empirical evidence to establish that the dominance of the petroleum car was the result of small-scale, accidental causes, there would need to be a degree of refinement in how such a case is talked about. Both Urry and other commentators refer to the idea that in some cases *a small cause has large effects* (e.g. Urry, 2005: 6; Hayles, 1991: 11). However, this is problematic because it misdirects attention by focusing only on one of the causes that leads to the final outcome in any given case. To see this, let us start with the classic 'chaos theory' example of a putatively small cause that has large effects, the butterfly effect. As we saw above, this involves the claim that the tiny amount of turbulence generated by a butterfly flapping its wings can lead to a vastly different outcome, say a storm occurring in a far-away city instead of the sunny weather that would have occurred if the butterfly had decided to have a rest. Is the butterfly's wing-flapping a small cause that generates a large effect? I would argue that this is not the case. After all, the small cause of the butterfly's wing-flapping does not cause, all by itself, the large effect of a storm occurring. Rather, the butterfly's wing-flapping is one of a range of atmospheric causal influences that interact and ramify over time, resulting in a storm. It is thus more accurate to say that two systems whose end-states are the results of many causes, may end up in markedly different end-states even if their starting conditions differ only by one small causal influence.[4] This is not merely a pedantic point, in my view, because it emphasizes the importance of attending to the various other causes that contribute to any outcome. If one believes that small causes have large effects, one does not need to seriously consider the other causes that contributed to these effects.

To be fair to Urry, his discussion of the domination of the petroleum-car does point towards the kinds of processes which reinforce the early advantages that the petroleum car might have had, insofar as there are learning processes in both constructing and using petroleum cars and their support systems which make it less effective for organizations to shift to other modes once they 'back' the petroleum car (Urry, 2003: 55). However, stronger emphasis needs to be given to these processes in my view, and further examination. Even if the complexity-based explanation was correct, Urry would need to accept that the dominance of the petrol car, as an 'effect', would not be anything like sufficiently explained by reference to the 'cause' of it completing a race in Chicago in 1896. Rather, it would be explained by a variety of causal factors including initial causes, but also including the institutional processes that created and supported the large-scale dominance of the petrol car against competitors, as well as those social and natural features that allowed the petrol car to continue to be viable (the existence of vast reserves of oil in the world, for example). These other causal features may well turn out to be somewhat distinctive, or at least only one class of phenomena amongst others, insofar as not all technological fields are dominated by one form of technology.[5] As such, I would suggest that we should dispose of the summary claim that a small cause can have large effects, and offer more precise formulations that encourage attention to be given to all of the relevant causal influences at work.[6]

Unpredictability

The other aspect of complexity discourse that I would like to examine here is the argument that complex phenomena are intrinsically unpredictable in character. Complexity discourse shares an emphasis on unpredictability with chaos theory, although arguably not to the same extent. Chaos theorists often argue that the trajectories of chaotic systems are inherently unpredictable in the sense that it is not possible to precisely predict where the system will be in state-space at any particular point in time.[7] Complex orders are frequently held to have a degree of stability, but to be periodically subject to unpredictable developments in which self-organizing processes will reformulate the system and its structure (Harvey & Reed, 1994). Thus, key writers such as Prigogine have tended to place an emphasis on the ultimate unpredictability of complex systems, their tendency to bifurcate such that they develop in one of two possible directions, and the impossibility of predicting which direction will be taken (Prigogine & Stengers, 1984: 160-170).[8] Proponents of complexity theory in the social science have been particularly enthusiastic to embrace this emphasis on unpredictability chiming in as it does with social theoretical concerns after positivism which have variously emphasized the role of agency, contingency and unintended consequences in social life (see for example Giddens, 1984; Bhaskar, 1979).

Urry is certainly keen to find unpredictable phenomena in the social world (see for example Urry, 2003: 59-72, 95, 105), and here I want to consider how analytically robust two of Urry's examples are. The first example makes a link between the nonlinear character of a phenomenon and its unpredictability, dealing with the ecological relationships between settlers and the environment in Malibu, United States. This is a case that Urry draws from Mike Davis' fascinating book *Ecology of Fear: Los Angeles and the Imagination of Disaster* (2000). Urry presents the example as follows (Urry, 2003: 34-5). Malibu can be considered the 'wildfire capital of North America' in that it suffers incredibly intense fires with relative frequency. The most significant factor in causing this is 'nonlinear relationship between the age structure of vegetation and the intensity of fires that are generated' (Urry, 2003: 34). For example, 50 year old vegetation of the kind present in Malibu does not burn 2 ½ times as intensely as 20 year old vegetation, but 50 times as intensely. Instead of being burnt off when it is young, however, the vegetation is often given the chance to age significantly because of the policy of 'total fire suppression'. The result of this is that when blazes do get started they are extreme in character, frequently burning down numerous properties as well as putting the lives of residents, fire-fighters and others in the locale at risk. Summing up his discussion, Urry states:

> *This example shows that certain kinds of cause can generate huge and unpredictable change while other examples would show that external causes could generate almost no significant effects (Urry, 2003: 35).*

In response to Urry's analysis, I want to suggest that it is seriously unhelpful to describe the presence of intense forest fires in Malibu as unpredictable. Certainly, an emphasis on their unpredictability is quite out of the spirit of Davis's analysis (see Ch. 3 of Davis, 2000). For one thing, Davis presents the move to 'develop' Malibu in the 20th century as misguided from the start, because of the:

> *…uncanny alignment of its coastal canyons with the annual "fire winds" from the north: the notorious Santa Anas… The San Fernando Valley acts as a giant bellows, sometimes fanning the Santa Anas to hurricane velocity as they roar seaward… Add a spark to the dense, dry vegetation on such an occasion and the hillsides will explode into uncontrollable wildfire (Davis, 2000: 100).*

Davis's argument is that the combination of geography and climate of the region makes it predictable that there will be fires, particularly at certain times of year.

As Urry reports, Davis further argues that the combination of a 'total fire suppression' policy and the nonlinear increase in burning intensity leads to particularly severe fires in the Malibu area. However, unlike Urry, Davis makes no connection between nonlinearity and unpredictability. Rather, Davis' analysis is based on the fact that knowledge of the nonlinear increase in burning intensity actually makes outcomes predictable in some sense. In other words, given the nonlinearity in question, it is precisely predictable that a policy of total fire suppression will lead to hugely intense fires.[9] Furthermore, these intense fires could be largely avoided if there were controlled burn-offs every few years, as these would prevents the vegetation getting to an age where it predictably burns with high intensity (Davis, 2000: 144).[10] Another predictable outcome that can be generated from

Davis' analysis is that comparable environments, such as parts of Baja California, will fare differently to Malibu because of their differing policies on fires. In the uplands of Baja California, where there is no attempt at fire suppression, there are more fires but they are 'patchier' and less intense, because high intensity vegetation does not build up (Davis, 2000: 103). The overall point I want to make here is that Urry misdescribes this kind of example because of his belief that nonlinearity is intrinsically linked to unpredictability. The intensity of Malibu wildfires is not unpredictable given the relevant knowledge.

It is important to clarify that in claiming certain aspects of Malibu wildfires to be predictable, I am not suggesting that we can be *absolutely certain* about the timing and placement of their occurrence and their precise characteristics (exact temperature and so on). However, if this level of detail and precision is necessary to be able to deem some phenomenon to be predictable then this would exclude many if not all social scientific cases, as well as many natural scientific ones. Indeed, absolute certainty of this kind is not even present in natural scientific laboratory controlled situations, given that scientists can never be certain that they know all of the possible confounding conditions including local contingent ones (See for example Collins, 1985).[11] Rather than placing all social scientific phenomena in the 'unpredictable' category, I would suggest that there are some cases in which we can have confidence that, under specified conditions, certain outcomes are likely to occur within a given time and space range. We can thus distinguish relatively predictable events, such as the intense character of Malibu wildfires, from completely unpredictable events, such as the numbers in the next lottery draw, rather than putting these together in the same category—'not predictable with certainty'.

The other putative example of unpredictability that I would like to consider here arises from Urry's discussion of networks and fluids that are global in character. Urry introduces his analysis of networks and fluids as part of his exploration of new ways to analyze global spaces. Urry's intention is to analyze the emergent properties of the global order without placing old-fashioned and outmoded concepts like 'nation' and 'region' at the centre of the analysis (Urry, 2003: 42-6). According to Urry, globally integrated networks are not nationally bounded, but operate transnationally 'across multiple and distant spaces and times' (Urry, 2003: 57). These networks make fairly stable and predictable links between people and objects around the globe, and are employed by organizations like Microsoft, McDonalds and Greenpeace to allow efficient communications, successful delivery of products and profits, and so on.

Global fluids are, according to Urry, less stable than globally integrated networks, and interact with other fluids, networks and organizations in unpredictable ways. These fluids include travelling peoples, global brands, social movements, the world's oceans, environmental and health hazards and automobility (Urry, 2003: 59-74). Urry argues that fluids are the result of many local actions which can interact and ramify to create 'global waves'. Urry states:

> *The "particles" of people, information, objects, money, images, risks and networks move within and across diverse regions forming heterogeneous, uneven, unpredictable and often unplanned waves… Such waves demonstrate no clear point of departure, deterritorialized movement, at certain speeds and at different levels of viscosity with no necessary end state or purpose (Urry, 2003: 60).*

Thus global fluids are seen by Urry as unpredictable, and as often lacking a clear start and end point.

Urry is aware that earlier writers within the sociological tradition, such as Durkheim, have argued against modes of representation that attempt to capture the 'flux' of life without abstracting from this to arrive at more stable, fixed conceptualizations (Urry, 2003: 59). However, Urry defends his approach in a broadly realist fashion by arguing that concepts of flow, liquidity, waves and so on are necessary to accurately describe what global society is like. The defence of these concepts is, for Urry, simply a matter of representational adequacy.

Whilst I would accept that, in principle, fluids, flows and unpredictable waves may exist, it is important to remember that identification of phenomena as having these characteristics is potentially fallible. Furthermore, as John Holmwood has argued, accounts of particular phenomena that emphasize their indeterminacy—unpredictability, unevenness, no clear point of departure, no necessary end state or purpose—have similar characteristics to confused accounts of determinate phenomena (Holmwood, 1996; see also Thompson, 2004). Putting this another way, a process that appears to be erratic and unpredictable in its operation may actually be a relatively determinate process that is currently being misunderstood. The relevance of Holmwood's contention to the present case is backed up, in my view, by the lack of analytical rigour present in Urry's discussions of some examples of global fluids. By categorizing all 'travelling peoples' as one fluid, for example, it is hardly surprising that this fluid seems unpredictable, heterogeneous and so on. However, a more careful separation of some of the constituents of that fluid might allow a more coherent picture to emerge, insofar as, for example, the 'flows' of refugees and transnational capitalists are likely to have different characteristics and logics (see McLennan, 2003: 555). In another of his examples, that of automobility (the car system), Urry himself undermines the notion that he is analyzing an unpredictable fluid with no clear starting point. For one thing, Urry clearly attributes the beginnings of the system of automobility to North America, implying that there is a straightforward, identifiable point of departure of this transport wave. For another thing, Urry goes on to suggest that societies become 'locked in' to the system of automobility in a way that seems 'impossible to break from' (Urry, 2003: 69). These phrases are hardly redolent with notions of unpredictability and fluidity. As such, some of Urry's claims about the unpredictability of 'global fluids' seem rather overstated. Furthermore, in some cases it may be that it is analytical and conceptual problems which gives social phenomena the appearance of indeterminacy, rather than this being inherent in the phenomena themselves.

Conclusion

The purpose of this piece has been to critically reflect on the concepts of nonlinearity and unpredictability, concepts which are commonly invoked by complexity theorists. The aim has been not to debunk complexity theory, or the work of John Urry which I have focused upon, but to use Urry's stimulating discussion of global complexity as a basis for thinking about how complexity theory might become more analytically precise in its use of notions like nonlinearity and unpredictability. This has involved both challenging the standard formulation that 'small causes have large effects' and arguing that there is a range of levels of predictability, rather than a simple contrast between completely predictable and totally unpredictable. As well as this, I have been raising the question of the empirical justification of claims about nonlinearity and unpredictability as constitutive features of the social and natural worlds. Two dimensions are important here. The more narrowly empirical issue is that claims about the nonlinearity and/or unpredictability of certain phenomena, such as the dominance of the petrol engine or the spread of wildfires in Malibu, need to be carefully justified by empirical evidence, rather than being accepted too swiftly by those enthusiastic to find complexity everywhere in the social and natural worlds. The wider conceptual component of the argument is that the apparent indeterminacy and unpredictability of phenomena may be signs of the limitations of existing concepts and theories rather than indicators of the true nature of the phenomena in question. I say *may* be signs here because it cannot be demonstrated that all phenomena can be rendered determinate and predictable, and complexity theorists have made interesting arguments which account for why certain phenomena may not have this character (see for example Cilliers, 1998). However, there is a danger in always operating with an assumption that the social world and its constituents *must be* intrinsically fluid and unpredictable. Those who are convinced that the social world has this form may fail to examine whether the apparently intrinsically confusing and unpredictable nature of the phenomena they study is actually an artefact of their own limited categories. For these reasons, a careful analytical and empirical approach to issues of nonlinearity and unpredictability is essential.

Notes

1. Presumably the shock was somewhat diminished for those who had already accepted indeterminism within the quantum domain.

2. Critical realism could be argued to share complexity theory's emphasis on continuity between the natural and social domains, although some hiatus between the two is often implied (see for example Archer, 1998).

3. McLennan argues that the terms linear and nonlinear are misused by Urry (and complexity theorists more generally) because their strict mathematical meanings are not adhered to (McLennan, 2006: 145). Whilst I have some sympathy for this argument, the problem I am concerned with here is not the appropriation of the aura of the 'hard' sciences by social scientific complexity theories, so much as evidential and conceptual issues in the usage of 'nonlinear' that actually has been adopted by social sciences, that relating to the proportionality of causes.

4. Perhaps some of the confusion here arises from cases where there is just one iterating cause, where the state of a system depends on just one cause repeated over time. A simple system of this kind would be one in which the key variable capturing the system state was X, and where for every time interval that elapsed a cause operated such that the value of X was squared. The equation for such a case would be $X_{(T+1)}=X_{(T)}^2$. This equation means that the value of X at some particular time (Time 'T+1') is the square of the value of X at the time interval before (Time 'T'). As Harvey and Reed point out, in situations of this kind, slight initial differences in the value of X will lead down the line to much larger differences because of the repeated squaring of the value (Harvey & Reed, 1994: 379). It is correct to say that it might only take a very small causal input to alter the initial state of the system, captured in the initial value of 'X'. It is also correct to say that there will be a marked difference in how the system develops over time even if this initial value was altered only minutely, for example the difference between $X_{initial}=2$ and $X_{initial}=2.0001$. However, this does not mean that the small cause altering the start value of X has had a large effect. Rather, the difference between the system that results depending on the two different values of $X_{initial}$ is the result of the *combination* of the small cause altering $X_{initial}$, and the many instances of the cause that produces a squaring of the value of X.

5. There do seem to be technological fields where there are multiple viable competitors at any one time. One example from the past few years is that of music in which CDs, vinyl records, tapes and MP3-files coexist as formats. This is not to say that all of these forms will persist, but that they have meaningfully persisted for a certain period of time along with one another. Contrasts between this kind of case and the apparently almost utter dominance of the petroleum car need to be examined. Of course, a further issue is where one draws the line with competitors; if aeroplanes, trains and boats are considered competitors then the petroleum car is not utterly dominant.

6. It is worth briefly noting that the causal processes involved in the 'butterfly effect' and 'lock-in' are somewhat different. In the case of the butterfly effect, the difference in outcomes is based on the vast number of causal interactions that take place in order to generate the weather. Weather systems with slightly different initial conditions may develop in quite strongly divergent ways. However, this divergence is not predictable given our current capacities of measurement and calculation. As such, it is not possible to predictably generate bad weather at some part of the globe by slightly altering the conditions at another point. In the case of 'lock in', the divergence of outcomes is based on the way in which one development path or another is reinforced over time by surrounding developments. Insofar as the lock-in theory is correct, it should be possible to manipulate initial conditions in order to predictably generate the preferred path of development.

7. For an argument about the limitations of unpredictability in chaotic systems see Brown (1996).

8. It is worth noting that other complexity theorists, while acknowledging the lack of strict predictability of complex systems, argue that there are attractors in the state-space of complex systems which draw them towards a limited set of possible configurations (see for example Kauffman, 1993).

9. Of course, those who set up the total fire suppression policy may not have known about the nonlinear relationship between vegetation age and burning intensity. But this limitation to their knowledge does not make the situation unpredictable in principle, just unpredicted at that point in time.

10. **Davis does note that the small possibility of** break-away fires would expose the fire departments to potential lawsuits (Davis, 2000: 144). However, if there was a general policy of controlled burn-offs, the material would not be there for fires of high intensity.

11. **One does not have to agree with every aspect** of Collins' analysis to accept his point that local contingencies of experimental set-up can have an impact upon any particular attempt to run an experiment.

Acknowledgements

I would like to thank the British Academy for providing an Overseas Conference Travel Grant to the World Sociology Congress 2006, at which a version of this paper was presented. Thanks also to Angus Bancroft for his comments on this paper.

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