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Content, Context, Fungibility and Disproof

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1 Introduction

(1) I am a biologist. While teaching biology, especially reproductive and evolutionary biology, at the University of Birmingham I promoted, and taught, a Philosophy of Science course: Bacon, Popper, Kuhn, Lakatos with asides to Waddington, Lysenko, and warnings about naive DNA preformationism. Our best students enjoyed even this amateur approach, but were unfamiliar with many of the classical Physics examples like Michelson-Morley or even Newton v. Einstein. These classical examples do not translate into Biology at all well; the quasi-biological ones are worse: black-versus-white swans becomes a simple problem of taxonomy, not an issue of disproof. The interesting issues were, I thought, common to biological science and physical science, and I felt that my teaching (for Popper, mostly from *Conjectures and Refutations*, 1963*a*) was inadequately based because the students didn't seem to take the physics examples into their biology. Now I believe that there are real problems within this transfer; further, I believe that the biological arguments must spread back into physics and raise questions about the classical physics examples themselves, about naive disproof arguments in science generally.

2 Components of the Problem

(2) Ian Stewart and I raised these questions in different contexts in three papers and a popular-science book (Cohen & Stewart 1991*a*, Cohen & Stewart 1991*b*, Cohen & Stewart 1994, Stewart & Cohen 1994); the enthusiastic reception of the book, *The Collapse of Chaos*, gives me some authority to raise these issues here. A more sophisticated version of the argument is in our next book (*Figments of Reality*, 1997).

(3) There are, I think, four components of the problem:

1. The adequacy of any theories at all, an issue addressed well by Popper (e.g. 1963*b*—but not as naively as I wish to). Stewart and I are impressed that most of the Science we "know" is revealed as "Lies-to-Children" (Stewart & Cohen 1994, Stewart & Cohen 1997) as soon as it is examined closely (by this we meant that these matters are simplified to the point where they're not in any sense "true", but are the standard myth for educational purposes—they fit with other myths, perhaps). Examples: rainbows produced by raindrops acting as tiny prisms (all exactly pointing in the right direction for your eyes? And for your friend 20 metres to your left?); the Bernoulli story of lower pressure over the

curved upper surface of airplane wings "pulling upwards" (so that if airplanes try to fly upside down they dive into the ground?); the anatomy of female genital tracts as portrayed in diagrams (always shown as having large cavities in uterus and vagina - the cavities are nominal, 10 microlitres in uterus, perhaps 10x that in vagina! See Cohen 1996). The intellectual context of testing any theory must be suspect, therefore, if it is tested against these educational constructs rather than against deeper appreciations. As Editor-in-Chief of *Speculations in Science and Technology* I receive many submissions which have precisely this structure: a speculation among fairy-tale (text-book) suppositions. Then I find that many of my own speculations, as well as those I find in *Nature* and *Science*, also have this naive setting. Rejecting most initial hypotheses because a disproof is "obvious" sounds persuasive, but not if the touchstone is itself bogus.

2. Is there one physics? This relates to post-Modern stances, to questions about the "Theory of Everything" and its utility if invented (found?). If there are many equally-congruent webs of theory at the physical "level" (perhaps even non-overlapping) then our parochial starting-with-the-electron one, with its successively deeper levels of fundamental particles, is digging its roots down into a progressively exclusive view, not generalizing but limiting our ability to extend intellectual frontiers. As we extend the zoo of particles, our commitment to this classification is reinforced (and the balancing of mathematical equations, rather than disproof, is the task at hand). This is a biological stance, perhaps inappropriate to the mathematically-tied theories of physics (what Stewart and I called the "Sherlock Holmes Stories", each self-consistent but not universal in application).

3. Tactics of disproof commonly have a reductionist basis: internal-to-the-theory predictions are set up for test. But Stewart and I showed that much, perhaps most, of apparently reductionist scientific theory depends for its argument on contextual elements (for example, the Gas Laws depend on Volume, Pressure, Temperature, all contextually determined; gas molecules don't know what volume they're in). *Ceteris paribus* arguments, on which most disproofs depend, specifically factor out context. Arguments in molecular genetics, as well as most classical physics and chemistry, are susceptible to this criticism. Results can only be interpreted within the *ceteris paribus* experimental design (this is close to, but more parochial than, Kuhn's paradigm argument and dangerously close to Feyerabend's criticism of scientific argument in general). "Normal science" must be blind to context, but context is vital for locating the problems under consideration, and for describing the phase space of possibilities in which an understanding of the problem is

sought. A theoretical physicist can only find (invent) a new particle, to match the equations with new particle-collision results, if (s)he knows the limits of possible properties of particles within present paradigm(s); an evolutionary biologist can only invent (discover) a palaeontological phylogeny if (s)he has in mind all the ways in which the group might have evolved. I can only claim to understand the evolution of life on Earth if I can begin to answer "What might, and might not, happen if we ran the system again on Earth—or what might we find on another aqueous planet?" (Cohen 1991, Cohen 1993, Stewart & Cohen 1997).

4. The converse of 3 above: emergent properties frequently show independence of antecedent causes or more "basic" components. Stewart and I borrowed the word "fungibility" from the legal lexicon to describe such cases: a bridge can be made from rope, from steel girders, or from concrete and can exhibit the same special properties, so the components are fungible. And the reductionist proposition that the properties of the bridge can be argued from—depend on—the properties of its components and their interactions simply does not apply. The emergent properties are contextual, the rest of the Universe sees the bridge as a link between the island and the mainland whatever it's made of (even a tunnel, "made of nothing", might be equivalent). Popper got involved directly in this problem. Natural selection as a basis for organic evolution had a strange, perhaps un-disprovable status for him, at least partly because it could not be linked inexorably to nice reductionist DNA molecular biology. Predictions, particularly disprovable within the same theories, could not be made because whatever the genetic substrate, evolution would still occur in much the same way: on Antares 3, aquatic carnivores would be streamlined like pike, dolphins or ichthyosaurs; terrestrial carnivores would have their eyes in front and herbivores would have them at the sides (cf. Popper 1978, though he did not put it in this way)

3 Reductionist Nightmares, Game Trees and Ant Country

(4) There is another aspect of this last problem, which exhibits as "conservation of complexity" assumptions. We like to believe that the results of an experiment, or the aggregate of a set of processes, are at the same level of complexity as we started with (Cohen & Stewart 1991*a*, Cohen & Stewart 1991*b*). We find it difficult to handle, in general, the fact that one growing feather is, by any measure of complexity, more complex than the group of cells on

the yolk which produced the whole chicken. Stewart and I have reformed the emergence/conservation-of-complexity problem as "Ant Country", the unresolvable complexity which is located between top-down and bottom-up explanations in nearly all cases (Stewart & Cohen 1994, Stewart & Cohen 1997). We think that Dawkins' hierarchical reductionism (Dawkins 1989), which uses only two "levels" of emergence to explain scientific problems, addresses this same problem in a more usable, but less theoretically satisfying, way.

(5) There are many theoretical assumptions made by those physicists who are committed to Theory-of-Everything models of the universe. They are committed, firstly, to that reductionist rhetoric which claims that explanations converge, so that "deep" cosmology and "deep" atomic-structure-chemistry are both explained by quantum physics equations. The ultimate convergence they dream of is one final equation which lies at the root of the Universe, the Thought in the Creator's Mind. So these theoretical physicists always have more fundamental levels to test their theories against.

(6) Stewart and I have allegiance to the opposite view, which is that explanations diverge (Cohen & Stewart 1994, Stewart & Cohen 1994, Stewart & Cohen 1997); real reductionist explanations of each property of the cell must require many chemical experts, then many physicists to explain each chemical property. We called this the Reductionist Nightmare. We agree that, working upwards from quantum theory, it is (just) possible to predict/explain/justify a few of the many chemical properties, but we believe that the properties to be explained multiply upwards too. Because of the emergence of many properties at the chemical "level", and because of the fungibility of much of the substructure, the Theory of Everything cannot actually explain anything. It is as useless as an explanatory device at the bottom as is the concept of God at the top (often used by the same physicists...). It certainly cannot serve, in our view, as the ultimate touchstone (for Popperian disproofs).

(7) Biology, unlike physics (and much chemistry) cannot be tested by appeal to either Higher or More Fundamental laws. We cannot test theories about the behaviour of ants (especially Langton's Ant, see below) by appealing to predictions from Theory of Everything, or by appealing to general Laws of Behaviour. Predictions must be restricted to our experimental or observational level, and it is at this level that they must be disproved. Contrast a theory that dried fruits have more calories per fruit than fresh fruits, which can be disproved—theoretically—by appeal to the Law of Conservation of Energy—whether or not they provide more calories when eaten, this is the wrong touchstone theory. As a biologist, I am unhappy about this technique of disproof by appeal to what is known. But very many of the physicists' theories seem to be disproved by incongruence with another theory or accepted argument (or, more usually, accepted because of congruence with such theo-

ries). Popper clearly approved of this—indeed, in some senses it is obviously necessary to have some touchstone theories for any disproof strategy to be applied. It seems to me that this can only be satisfactory in a tiny subclass of well-understood, essentially Laplacian, universes. Yet most, of even such simple universes will have an Ant Country, just as our much messier system does, and simple theory-touchstones won't exist.

(8) Langton's Ant is a beautiful examplar here (Gale 1993, Cohen & Stewart 1994). Its universe is a simple cellular automaton, a square grid of black or white cells with simple rules. In this case there are just two rules:

1. The Ant reverses the colour of any cell it visits.
2. When the Ant visits a white square it turns left; when it visits a black square it turns right.

We do know the Theory-of-Everything in this case, so it looks like a great candidate for the Laplacian universe appealed to above.

(9) Now it turns out that there is a simple repetitive behaviour that the Ant consistently "finds" after tens of thousands of seemingly chaotic moves. This behaviour consists of a sequence of 102 moves which brings the Ant back almost to where it started, but one square up and one square to the right (or one square down and to the left). This then creates a characteristic diagonal "highway".

(10) All moves of the Ant are deterministically specified by the rules above. This includes the initial "disorganised" phase before the "highway" behaviour is discovered. In this sense, all behaviour in the system is "microscopically" predictable (given the details of the initial arrangement of black and white squares on the playground, and the initial position and orientation of the Ant). But despite this "perfect" knowledge, there is, to date, no mathematical proof that the Ant will always find a highway. It just always has.

(11) In this Universe we know the initial conditions, and the rules (its Theory-of-Everything, indeed), yet we are unable to predict even very simple things. So even for Langton's Ant, unknowable-in-generality Ant Country intervenes between our top-down and our bottom-up arguments. How much more this must be true for real ants! But I think that it is true for electrons, too.

(12) In our new book (Stewart & Cohen 1997), we replace the argument about phase spaces with which we attempted to handle some of these problems in *Collapse*, by the similar concept "game trees". These are the whole sets of possible moves in any game (for example chess or snakes-and-ladders or, in principle, football). Any actual game marks out a path within this space, a tiny fraction of all possible moves. The developing chicken is, in a sense, playing its own life game, and it can be thought of as interacting with many other players in its environment. Some of these are "rules" about viscosity, about fats being hydrophobic, about necessary properties of dividing cells, others are contributions like yolk, or the egg-shell, with which it must overtly interact. Embryology navigates much the same path—a similar chess game—in the development of each chicken (just as flames reproduce without heredity, by accessing rules about hot air rising, about radiation and about oxidative chemistry), producing a similar morphology in each generation of chickens (or flames). And the complex feather is the result of many interactive "moves" in the game of development. The chicken is genuinely much more complicated than the egg, and is a new complex entity in the world, not simply an unfolded, previously cryptic code. It is certainly not the fowl DNA code made flesh, which is the model purveyed by those who espouse "conservation of complexity" philosophies, and whose science does not contain emergence. So theories of feather development—on which I did my Ph.D.—cannot be checked according to the Popperian doctrine of disproof (Cohen & 'Espinasse 1961). My thesis had the right form, with complex unlikely predictions confirmed, never disconfirmed by technically-difficult experimental results. But the result was bogus Popper, as it so often is in written-up biology. Popper himself was very taken with emergence, but did not, so far as I recall, feel that it posed problems for theory-testing. I believe that it does, and that the Popper-concerned scientist must play a different game of her own.

4 How to Play the Game of Emergent Science

(13) Many, perhaps most scientists do not play this game, do not address the problems I have listed here. They understand their subject well, they perceive holes in our understanding of it, and they try to patch these holes, confident that the result will be a "whole cloth" (in the Perry Mason sense, a complete fabricated story with all the sub-stories tying in). I have addressed this question for biochemistry/molecular biology (Cohen & Rice 1996), suggesting that integration should not be only on one "level" but should permit integration with nearby theory in other disciplines. However, the work for that paper convinced me that Popper's unease with the "explanations"—usually adaptive theories—in much of biology was to be expected. So, rather than lauding the structure of physical theories while deploring that of biology, I take the reverse position.

(14) I believe the unsatisfactory, "bogus" nature of disproof in biology is

(almost) equally applicable to chemistry and all but the most arid physics. The properties of the vast array of inorganic compounds sit so clumsily upon the physical-chemistry theories meant to explain—predict—them that they must be dealt with essentially as a Natural History (with organic chemistry's contribution being somewhat more ordered—like physiology or comparative anatomy, perhaps, in the life sciences). The zoo of "fundamental" particles and constants is clearly awaiting a Linnaeus rather than a Dirac or even a Mendeleev. The theories of physics which so beautifully exemplify Popper-in-action are, in fact, the most theoretical, the most mathematical, the most "invented". Brahe, Copernicus, Newton all took the theoretical road, invented a new mathematical form to apply to the recalcitrant data. But Newton was not successful because gravity fitted the data any better—epicycles had been pretty good; he invented a new way of looking at the paths of the planets. And the theorists of this kind have always been the heroes of our physics/astronomy stories. The descriptive stuff (Europa has an ice sheet 10–20 km deep over a sea of perhaps 50 km depth, Pluto/Charon are an unlikely pair, etc) needs another kind of mind to organise—perhaps a planetary Linnaeus, again. Popper was a good philosopher, and philosophical physics fitted his ideas well. But I am basically a "bench" biologist, and my distrust of disproof strategies extends from my own expertise outwards to bench chemists and physicists. Our "normal science" is indeed theory-laden and theory-led; but its theories are tested by a great swathe of logical and experimental methods, only rarely by the pure light of reason.

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