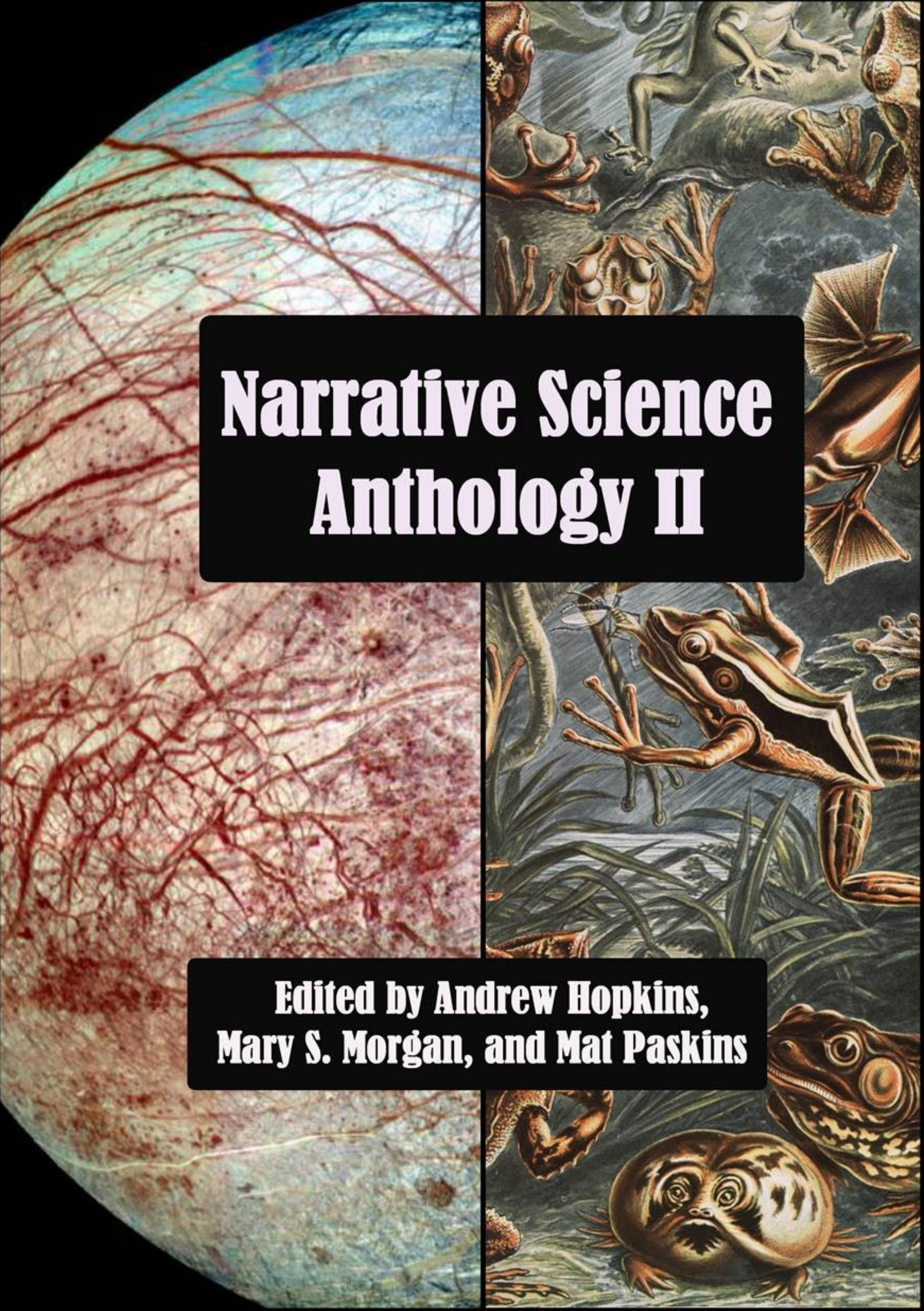


Narrative Science Anthology II

Edited by Andrew Hopkins,
Mary S. Morgan, and Mat Paskins

Cover Images

Left: Europa’s Frozen Surface—enhanced-colour image taken from NASA's Galileo spacecraft (image credit: NASA/JPL/University of Arizona).

Right: Plate from Ernst Haeckel's 1899 ''Kunstformen der Natur'', depicting frogs classified as Batrachia.

This work was hosted by the London School of Economics and Political Science and was funded by the European Research Council under the European Union's Horizon 2020 research and innovation programme (grant agreement No. 694732). www.narrativescience.org

NARRATIVE SCIENCE ANTHOLOGY

II

Edited by:

Andrew Hopkins, Mary S. Morgan and Mat Paskins

www.narrativescience.org

CONTENTS

PREFACE

The Preface to the predecessor of this online volume, An Anthology of Narrative Science (also known as Anthology I), predicted that a second collection of case studies complied as part of our Narrative Science Project would be forthcoming "later in 2019". A series of minor delays (probably par for the course), took the task of completing this sophomore volume into the early part of 2020, at which point, the Project and it activities began to fall foul of the intensifying Covid-19 pandemic. The editors started working from home, as did many of the contributors, and in the ensuing disruption, the two-way email traffic carrying edits, corrections, questions and suggestions, slowed to a crawl, and even ground to a halt in some cases.

As the pandemic and its effects continued into the following year, our Narrative Science parent Project at the London School of Economics duly completed its allotted lifespan at the end of March 2021; its members moved on to the next stages in their careers. At that point, Anthology II was still far from complete.

Since then, the editors have shoehorned the outstanding tasks into the gaps in their calendars, and now, more than three years on from the originally mooted completion, this second collection of narrative science cases is finally ready to see the light of day. Sincere thanks go to the authors of each case study for their imaginative contributions, and to those who kindly helped with translation. Special thanks are due to those who continued to engage with the editing process in the challenging working environment engendered by the pandemic. Those contributors whose pieces were done and dusted even before we had heard of Covid-19, are particularly thanked for their patience.

The Editors: Andrew Hopkins, Mary S. Morgan and Mat Paskins

May 2023

INTRODUCTION

What is in this book, and how can I use it? This is a collection of thirty primary source excerpts from the history of science in the past two hundred years, each of which is accompanied by an introductory commentary. All of the sources contain narratives: each narrative brings together heterogeneous materials and makes sense out of them by creating an ordering indicating the relations among them. In some of these narratives, those drawn from historical sciences such as evolutionary biology and geology, these connections are temporal. In others, the ordering relies on a repeating pattern which is said to underlie disparate phenomena, such as the cycles described by economists from the 18th to the 20th centuries. In others, causal relations between elements dominate, such as in the anecdotes about the behaviour of animals, or the processes of reaction between chemicals. These are all ‘narratives of Nature’, where Nature refers equivalently to the domains of the sciences, whether human, natural, physical or social. In contrast, other narratives report scientists’ processes of research, or reflect their understanding of their own work. This useful distinction between ‘narratives of nature’ and ‘research narratives’ was given to us by one of our project members, Robert Meunier and reported in our project book: *Narrative Science: Reasoning, Representing and Knowing since 1800* (2022), which provides a parallel set of analyses to the commentaries in this Anthology. The case narratives here were gathered by the three editors from both the immediate members of our project team and from the network of scholars who became associated with our Narrative Science Project at the London School of Economics during its years of research 2016–21.

To understand what these narratives are, and what they can do, it is helpful by starting to think about what they are not, and what we do not do. These are not the narratives about science given by historians or philosophers or literary critics: rather, they are narratives employed by scientists and natural philosophers themselves, as part of their own work. We have not sought to go behind the scenes to reveal the assumptions or ideologies of those who created them. Rather we engage with the epistemic role of narratives for those scientists who created them: namely how they used narratives to identify and solve their scientific problems. We are less interested in critiquing the narratives qua narratives than in examining how they work, identifying their connections with other modes of scientific understanding and explanation, and thinking about how the stories told within scientists' own practices might relate to the narratives of other fields.

The term narrative can cover a wealth of different forms of knowledge and communication, and you may find your instinct is to reject some of our texts as narratives. The commentaries for each text will explain why we have taken each of them as narratives; but we also suggest that by reading these texts with comparisons in mind, their narrative qualities will emerge. For example, narrative II is a limpid tale of crocodiles in eighteenth century Florida, while narrative XXVI is an account of some chemical syntheses whose authors have made a point of mentioning experimental missteps and paths not taken. The former is more obviously narrative in form, and accessible to a wide audience, and indeed had a considerable influence on poets as well as natural historians who read it. The latter is much more technical, and contains terms which are inaccessible to non-chemists (even if its larger gist is clear), but still it is a narrative explaining both a process of research work and how chemical reactions happen. The inclusion of these rather different kinds of narrative will, we hope, prompt reflection and debate about how we have identified narratives, and their characteristics in the sciences. While we are not primarily concerned with narrative as a mode of communication, or its role in pedagogical practice—we recognise that both of these are extremely important, and have informed some of the commentators' discussions of the narrative features of their texts. These features include, for example, the point of view from which the narrative is told, or role of the narrator, including on some occasions the narrator's self-conscious insertion into the narrative; the presentation of counterfactuals or alternative outcomes as part of a scientific explanation; the construction of sequences through narrative form; and the use of historical narrative reasoning in scientific contexts.

Inevitably, the selection—which is presented in chronological order—tells its own story, through the juxtaposition of different perspectives and materials. It opens with enlightenment hopes, fears, and wonderment at the prospect of progress, the dangers of dearth, the knowability of the cosmos, and the abundance of the non-human world; and it culminates in twenty-first century accounts of deep history, the putative contributions of human activity to a new geological epoch, and the role of history and memory in responding to extensive ecological loss. From the collection as a whole, while it is clear that scientific narratives have adopted different forms in different periods, and addressed different problems, it is also clear that the human, natural, physical and social sciences have not become less narrative overall.

We have sought to be selective, rather than comprehensive, in ways which enable us to revel in the variety of forms and functions narrative plays in the sciences. Most of the narratives are in English; in general, those which are not are first presented in their original languages of publication and then accompanied by a translation. And while some of the narratives here may be familiar to many readers already, such as the nineteenth-century writings by Charles Darwin on evolution through natural selection and Charles Lyell on the historical focus of the science of geology, we have not been bound by the contours of a conventional history of science. So there is discussion of organic chemistry and ecology, but not of molecular biology or quantum physics; there is discussion of economics and sociology, but little on medicine (hitherto the focus of narrative in science). We have chosen the examples in this Anthology II (and in its predecessor, Anthology I)* to develop and broaden the scope of conventional thinking about what narrative looks like, and what it does, in the sciences. We hope that presentation of less well-known examples from the history of science, and those where the workings of narrative are less obvious, offer provocations to think about more well-studied fields. A different big picture, and different connections, would doubtless emerge from other sequences of narratives—we would be delighted if this collection prompts the appreciation, or even study, of other examples in other scientific fields.

Although not its primary aim, the collection does provide teachable materials, and pathways could be selected through the texts and commentary to focus, for example, on: how the narrative form is used to put together heterogeneous elements; how narrative captures particular moments of scientific experience; how and where time plays a dominant role in the narrative and where it does not; how narrative functions in deciding research pathways in a field; and so forth.

We hope all readers and users of this Anthology II enjoy its highly varied examples of scientific narratives, whether those excerpts we have presented as narratives are in conventional written form, or more controversially in various visual forms.

The Editors: Andrew Hopkins, Mary S. Morgan and Mat Paskins

* https://www.narrative-science.org/resources-narrative-science-project.html

CONTRIBUTORS

Debjani Bhattacharyya, *University of Zürich,* VIII.

Martin Böhnert, *University of Kassel,* XIII.

Ross Brooks, *Oxford Brookes University,* XII.

Federico D'Onofrio, *University of Vienna,* I.

Silvia Di Bianchi, *State University of Milan,* V.

Geoffrey Cantor, *University of Leeds,* VII.

Kim M. Hajek, *University of Groningen,* X.

Helena Hammond, *University of Roehampton,* XIV.

Andrew Hopkins, *University College London,* VI., XXII., XXVII., XXVIII.

Colin McSwiggen, *New York University,* XXIII.

Felicity Mellor, *Imperial College London,* XXIX.

Robert Meunier, *University of Luebeck,* XIII., XVIII.

Mary S. Morgan, *London School of Economics,* III., XVII., XX.

Mat Paskins, *University College London,* II., IV., XIX., XX., XXI., XXIV., XXV., XXVI., XXX.

Jahnavi Phalkey, *ICTS Bengaluru,* XV.

Greg Priest, *Stanford University,* XI.

Jim Scown, *Cardiff University,* IX.

Keith Tribe, *University of Tartu,* XVI.

I.

Jacques-François de Chastenet, Marquis de Puységur (1656–1743), was one of the most influential among Louis XIV's generals. From 1690, he was responsible for the logistics of the army and was a specialist in reconnaissance. He served in different campaigns across Europe and was nominated Maréchal de France by Louis XV in 1734. His son published the posthumous *Art de la guerre, par principes et par règles* in two volumes (1748 and 1749).

Persuaded of the importance of theory for a correct understanding of warfare experience, Puységur insisted on the need for the application of geometry to the art of war in field campaigns, follow-ing the example of Maréchal Vauban's development of siege warfare (which involved perfecting the development of fortress design following the emergence of star forts in the Renaissance, and by developing a method of attacking fortresses). Vauban's method was essentially geometric: political, psychological and even financial aspects (so crucial to previous military thinkers) had very little place in it.

Puységur claimed that he too was actually rediscovering an ancient geometrical art of war that had been lost in the ruin of the ancient world just like Greek mathematics. He studied the ancient narratives of battles by the historians and generals of antiquity, searching for information on such lost knowledge. A large part of his two-volume book consisted of commentaries on long quotes from narratives (*récits*) of the battles of the past (including Caesar's campaigns). *Récits* were therefore crucial for Puységur's art of war: as a source of heuristics, as evidence, and as teaching material. Army units were arrayed and moved "according to all the rules of geometry" (Puységur 1748, II, 73) and illustrated in expensive prints of different fighting and marching orders of growing complexity. Array and movement in space were eminently geometrical concepts, but battles happened in time as well as space, and the characteristics and respective effectiveness of different orders were only made intelligible by narratives that connected the moving configurations in a sequence of battle and put together the actions of the two opposing armies in the field.

The importance of narratives for Puységur is demonstrated by his resort to the narrative of an imaginary campaign around Paris. The chosen text is particularly curious for it narrates the possible evolution of a battle that is supposed to take place in the western outskirts of Paris as if the clashing armies had approximately the size and position of the Imperial and French army in Freiburg on 3 August 1644. That actual battle had ended with a costly victory for the French while the Imperial General de Mercy withdrew from Freiburg undisturbed. Pusységur scrupulously combines three reports of that battle: two by contemporaries and the third by an anonymous historian, but transfers all the events to the countryside between Paris and Versailles, the Val de Meudon, the Moulin d'Issy and Sèvres. So this short piece combines Puységur's constant reference to historical narratives and his imaginary *exempla ficta* to provide materials that yielded abstract principles and geometrical rules that could be transferred to other contexts.

Federico D'Onofrio

[See page 185 for reference and further reading.]

1749: JACQUES-FRANÇOIS DE CHASTENET DE PUYSÉGUR, *Art de la guerre, par principes et par règles. Tome Second* (Paris: Quai Des Augustins)

[EXTRACT: pp. 171–175]

Quand on est sur le chemin de Paris à Versailles, et que l'on a passé *le Point du jour* pour arriver au pont de Sèvres, on a devant soi une montagne laquelle commence au chemin qui entre dans Sèvres; après quelques élévations, il y a une espèce de plateau d'où elle s’élève davantage et s'étend jusqu'au-delà de Meudon.

Supposons que depuis cette montagne de Sèvres jusqu'à S. Denis, ce ne soit qu'une plaine, qui ne soit coupée ni par aucune montagne, ni par aucune rivière, mais que le terrain qu'occupe la rivière soit le grand chemin qui va de S. Denis à Paris, que S. Denis soit Brisack, et que Paris soit Fribourg; que le terrain pour entrer dans le camp de M. de Mercy ne soit pas plus grand que celui depuis la montagne de Sèvres, jusqu'à la maison de Billancourt, où commenceroit le marais qui s'étendroit du côté du bois de Boulogne, tel qu'étoit celui qui fermoit la droite du poste de M. de Mercy.

Supposons encore que le revers de la montagne de Sèvres par où se feroit l'attaque de M. le Prince fut cultivé, comme le côté qui se présente à nos yeux, à qui nous ôtons toute grande clôture de murs; comme il y en a à la montagne de Sèvres; que le retranchement qui commenceroit à la maison de Billancourt, traversant le grand chemin, et ayant gagné la montagne, vînt au plateau dont nous avons parlé, que ce plateau fût l’endroit où M. de Mercy auroit placé son fort; que de-là, le retranchement passât le long de la cime de la montagne; que pour la défendre M. de Mercy y eût employé trois ou quatre régimens d'infanterie, ainsi que le dit M. de Turenne, tandis que sa cavalerie seroit en bataille dans la plaine le long du grand chemin.

Supposons outre cela, qu'en approchant de Meudon il y ait un bois, que le revers de la montagne par où il faudroit attaquer fut escarpé, tel que celui qui se présente à l'attaque de M. le Duc d'Enghien, de sorte que l'on ne pût y monter, et que pour faire diversion des forces de M. de Mercy, et l'attaquer entre Paris et la gauche de son camp, on fût obligé de le faire par le Val de Meudon où est une ravine qui sépare cette montagne de celle où est placé le moulin à vent d'Issy que nous regardons comme celui de S. Georges dont il sera parlé; que pour conduire l'armée de M. de Turenne à cette ravine, il fallût faire un grand tour et passer par Versailles, que tout le pays pour y arriver fût tout bois, dont on eût mal, ou point du tout reconnu les approches; que la montagne où est le moulin d'Issy fût cette seconde dont parle M. de la Moussaye, où il ne s'est fait aucune attaque et au pied de laquelle passe le grand chemin de Brisack à Fribourg; que cette seconde montagne du côté de la plaine fût élevée et cultivée comme celle que défendoit M. de Mercy, mais que le revers au lieu d'être cultivé, comme l'endroit par où attaqua M. le Duc d'Enghien, fût rempli de bois de haute futaye. La ravine du Val de Meudon est encore supposée aussi étroite que celle par où M. de Turenne nous marque avoir attaqué.

Dans la situation que M. de Turenne, de même que M. de la Moussaye, représente nos deux armées quand elles attaquent, nous voyons qu'elles n'ont aucune communication, et que leurs forces sont entièrement séparées, qu'elles ne pourront se communiquer que quand elles auront forcé et seront entrées dans la plaine; au lieu que M. de Mercy durant les attaques peut s'aider de toutes ses troupes, pour renforcer celles qui en auroient besoin.

M. de Turenne dit que son armée, quand il passa le Rhin à Brisack pour s'approcher de Fribourg, étoit de cinq mille chevaux et de quatre à cinq mille hommes de pied; que celle de M. le Duc d'Enghien étoit de six mille hommes de pied et de trois mille chevaux.

Il faut donc nous représenter l'armée de M. de Turenne dans le ravin du Val de Meudon, voulant forcer le passage pour entrer dans la plaine; une partie de l'infanterie ennemie entrée dans le défilé pour en défendre l'issue, tandis que l'autre partie est en bataille dans la plaine; il faut se représenter aussi derrière elle sa seconde ligne de cavalerie qui la soutient, dans la vue que si le grand corps d'infanterie de M. de Turenne, fort supérieur au sien, le rendoit maître du défilé, quand il y auroit réussi, sa cavalerie ne pût entrer dans la plaine, se former, ni s'étendre, étant obligé de défiler dans cette ravine par un cavalier ou deux de front, pour se former à l'entrée de la plaine. En effet, quoiqu'appuyé de son infanterie qui a le dos au défilé, il ne peut comme il le dit lui-même, faire passer dans la plaine qu'un escadron; n'y ayant pas d'espace pour s'étendre.

Figurons-nous dans le ravin du Val de Meudon 4 à 5000 hommes de pied qui ne peuvent occuper qu'un très-petit front, et derrière eux cinq mille chevaux peut-être marchans un à un, ou deux ou trois de front. Quelle file, ou quelle étendue de troupes, les unes derrière les autres ! Ainsi quoique l'ennemi soit beaucoup plus foible, le peu de troupes qu'il a se présentant tout de front en bataille pour vous attaquer en tête et en flanc; si après avoir forcé le passage vous tentez d'entrer plus avant dans la plaine, comme par cette situation il fait combattre à la fois plus de troupes que vous qui en avez le double, mais dont vous ne pouvez faire usage, il a tout l'avantage sur vous.

Ainsi, pour avoir mal reconnu les lieux par où l'armée de M. de Turenne devoit marcher, et ceux où elle devoit faire son attaque; son armée a été mise au hasard de ne pas y arriver de jour pour attaquer en même-tems que celle de M. le Duc d’Enghien, qui de son côté attaquoit par un grand front, mais n'avoit pas assez de troupes pour tout embrasser, tandis que M. de Turenne qui attaquoit par un très-petit front en avoit trop pour les pouvoir toutes faire agir. Par-là, nous avons perdu notre supériorité en nombre, et acheté cher l'avantage que nous avons eu en obligeant M. de Mercy de quitter son premier poste, pour en prendre un autre: voici ce qui en est arrivé.

Le lendemain de notre première attaque, nos troupes par la fatigue et la perte des hommes, se sont trouvées hors d'état de pouvoir l'attaquer ce jour-là dans son second poste, et ayant été contraintes de remettre l'attaque au jour suivant, Mercy qui sçavoit profiter du tems pour se retrancher promptement, l'ayant fait malgré le courage de nos troupes, et la capacité des chefs, lorsque nous l’avons attaqué, nous y avons été repoussés et obligés de nous retirer, et de le laisser tranquille dans son camp, jusqu'à ce que pressé d'ailleurs par la disette de fourage (quoique cette raison-là n'ait pas été donnée pour la cause de sa retraite), il a pris le parti de marcher dans le pays de Wirtemberg, mais par le bon ordre qu'il a sçû mettre dans sa marche, le corps de cavalerie qui avoit la tête de notre armée l'ayant voulu attaquer près de l'Abbaye de S. Pierre, a été repoussé, et sa marche n'a pû être interrompue par tous les efforts de notre armée [...].

[TRANSLATION]

1749: JACQUES-FRANÇOIS DE CHASTENET DE PUYSÉGUR, *The art of war, its principles and rules. Volume Two* (Paris: Quai Des Augustins)

When you are on the way from Paris to Versailles, after you cross the *Point du Jour* to reach the Pont de Sèvres, you have before you a mountain whose slope begins at the path that enters Sèvres; after a few elevations, there is a kind of plateau from which it rises further and extends beyond Meudon.

Let us suppose that, after this mountain, a vast plain extends itself from Sèvres to St. Denis, uninterrupted by any mountain or river, and that the ground that the river occupies [near Freiburg] is, instead, the great road that goes from St. Denis to Paris, that St. Denis is Breisach, and that Paris is Freiburg; [let us suppose] that the ground to enter the camp of M. de Mercy is no larger than that from the mountain of Sèvres, to the house of Billancourt, where the marshes that extend on the side of the Bois de Boulogne would begin, in analogy with the marshes that closed the right of M. de Mercy's camp.

Let us further suppose that the back of the mountain of Sèvres, where M. le Prince planned to attack, was cultivated just like the front side, and [let us imagine] removing all high fences and gates that exist on the mountain of Sèvres. [Let us suppose] that the entrenchment which began at the house of Billancourt, after crossing the highway, reached the mountain, and extended to the plateau that we described above. This plateau would then be the place where M. de Mercy placed his fort. [Let us suppose] that from there the entrenchment would run along the top of the mountain and that, in order to defend it, M. de Mercy had to employ three or four regiments of infantry there, as M. de Turenne says, while the cavalry would fight in formation on the plain along the main road.

Suppose further that, in the direction of Meudon there were a forest, that the flank of the mountain from which it would be necessary to attack were as steep, as that encountered by M. le Duc d'Enghien when he attacked, so that one could not go up there. [Let us assume] that, in order to distract the forces of M. de Mercy, and to attack him between Paris and the left of his camp, one was obliged to go by the Valley of Meudon where a ravine separates the above-mentioned mountain from that where the windmill of Issy stands (which we liken to that of St. Georges [in the battle of Freiburg]). [Let us suppose] that in order to lead the army of M. de Turenne to this ravine, the army was obliged to make a great turn and pass by Versailles and that the army had to cross a territory entirely covered by woods, whose surroundings had been only badly or not at all explored. [Let us suppose] that the mountain where the Issy mill stands was the second mountain of which M. de la Moussaye speaks [in his account of the Battle of Freiburg], where no attack took place and at the foot of which passes the great road from Breisach to Freiburg. [Let us suppose] that this fertile mountain was cultivated on the side of the plain, just like the elevation defended by M. de Mercy, but that the other side was entirely forested, instead of being cultivated like the place where the Duke d'Enghien attacked.

The Val de Meudon ravine is supposed to be as narrow as the one through which M. de Turenne decided to lead his attack. M. de Turenne, as well as M. de la Moussaye, represents our two armies as having no communication and being entirely separate when they attacked, until they broke the enemy lines and entered the plain; instead [the two authors show] that M. de Mercy during the fight could rely on his entire army to reinforce any weak point.

M. de Turenne says that his army, when it crossed the Rhine at Breisach to approach Freiburg, was five thousand horses and four to five-thousand foot men strong; that of M. le Duc d'Enghien was six thousand foot and three thousand horses strong.

We must therefore depict to ourselves the army of M. de Turenne in the ravine of the Val de Meudon, trying to force its way and enter the plain, while part of the enemy infantry entered the narrow pass [of the Val de Meudon] to defend its exit and the other part was fighting in the plain. We have to represent ourselves a second line of cavalry that supported [Imperial] troops from behind, in the expectation that, if the large body of infantry of M. de Turenne (much stronger than Mercy's infantry) managed to conquer the exit of the valley, [Turenne's] cavalry could not enter the plain, arrange themselves in formation or deploy, since they were forced to march through this ravine in a column no wider than a rider or two, and could only be brought into array once they entered the plain. Actually, although supported by his infantry, with its back to the narrow pass, [Turenne] could not, as he himself acknowledged, send a squadron into the plain, since there was not enough space to deploy it.

Let us imagine in the ravine of the Val de Meudon 4 to 5000 foot men who can only march in a very narrow line, and behind them five thousand horses perhaps walking one by one or two or three in line. What a line, or what a range of troops, one behind the other! So although the enemy is much weaker, the few troops he has present themselves deployed in array, ready to attack you in front and in flank. If you forced your way and you try to penetrate further into the plain, the enemy can engage many more troops than you who have twice as many soldiers that you cannot make use of. Therefore, the enemy has a considerable advantage over you.

Thus, from having badly explored the places that the troops of M. de Turenne had to march through and those where they were to make their assault, his army ran the risk of not attacking at the same time as the army of the Duke d'Enghien. While the latter attacked on a large front, but did not have enough troops to cover its entirety, M. de Turenne, who attacked with a very small front, had too many troops to be able to deploy all of them. In this way, we [the French] lost our superiority in numbers, and bought dearly the advantage we had in forcing M. de Mercy to abandon his first position to take up another: here is what happened.

The day after our first attack, our troops, due to fatigue and the loss of men, found themselves unable to attack Mercy in his new positions, and were forced to put off the attack to the following day. Mercy knew how to take advantage of the weather to reinforce his defensive posture and did so. Despite the courage of our troops, and the ability of the commanders, when we attacked him, we were pushed back and forced to withdraw and leave him alone in his camp, until he decided to move to the territory of Wirtemberg, due to a shortage of fodder (although this reason was never given as the cause of his retreat). Due to the good order of his march, when the corps of cavalry that formed the avant-garde of our army tried to attack [the Imperial army] near the Abbey of St. Pierre, it was repelled and Mercy's march could not be interrupted despite all the efforts of our army [...].

II.

William Bartram (1739–1823) travelled through the South-eastern United States in the 1770s, collecting plants and animals and writing an account of his travels. He was the son of the colonial naturalist John Bartram, and in his early life was an apprentice in botanical study and sketching. In this passage, he describes his encounter with alligators in Florida.

It was long believed that Bartram had gone too far in his description of Florida alligators. A reviewer for *The Monthly Review* in 1793 objected to his "somewhat too luxuriant and poetical language…in his extraordinary account of the crocodile, or alligator, as he indiscriminately terms that horrid animal". Scientific writers, too, took exception to Bartram's dramatic depiction: a late nineteenth century expert on fisheries, F.W. True, called it "most evident hyperbole". But for others the bellowing and the fight which Bartram depicted became phenomena which could be corroborated by further observation.

Bartram's narrative also attracted scepticism over the sheer number of alligators which he claimed to have observed. However, Karl Schmidt, assistant curator of reptiles and amphibians at the Field Museum of Natural History, Chicago, explained that the species had later been severely depleted by collectors of eggs and young alligators, and by hunters who took advantage of the trade in hides which was established after Bartram's travels (Lee 1972, p. 125). The reception of Bartram's natural historical narrative can thus be considered as an example of 'shifting baseline syndrome', the adjustment of ecological expectations in the face of unremembered loss (See **XXIV** and **XXX**).

The vividness with which Bartram described the animals he observed proved suggestive for many literary writers. As Michael Branch (2004, p. 184) remarks, "in an age in which literary capital generally flowed westward across the Atlantic, Bartram was widely read by Europeans—including, notably, William Wordsworth, Samuel Taylor Coleridge, Thomas Carlyle, and Francois-René, Vicomte de Chateaubriand—whose imaginative conceptions of nature were informed by Bartram's romantic descriptions of American wilderness."

The suggestiveness of Bartram's writing for literary authors should not distract from his own artfulness, however, or from the manner in which he 'focalised' his account on the interactions between himself and the alligators. In an article for *Early American Literature* from 1972, Bertha Lee showed that Bartram developed his narrative from an earlier report, in which "Bartram is not alone, there is no mention of the fight between the two male alligators, the reptiles do not emit smoke [sic] from their nostrils, there is no encounter in the middle of the lagoon with deafening jaw-clapping alligators, and there is simply not a single bear". Lee concluded, however, that Bartram did not simply invent his details and that his "usual method of literary intensification [was] to combine a series of observations into one striking scene". Bartram "used considerable narrative power to create such verisimilitude that readers for nearly two hundred years have believed firmly in the truth of every word he wrote", or at least they were willing to see his observations as refutable rather than simply fiction (Lee 1972, pp. 127–128).

Mat Paskins

[See page 185 for references.]

1791: WILLIAM BARTRAM, *Travels through North and South Carolina, Georgia, East and West Florida, the Cherokee country, the extensive territories of the Muscogulges, or Creek confederacy, and the country of the Chactaws; containing an account of the soil and natural productions of those regions; together with observations on the manners of the Indians* (Philadelphia: James and Johnson)

[EXTRACT: pp. 115–121]

The evening was temperately cool and calm. The crocodiles began to roar and appear in uncommon numbers along the shores and in the river. I fixed my camp in an open plain, near the umost projection of the promontory, under the shelter of a large Live Oak, which stood on the highest part of the ground and but a few yards from my boat. From this open, high situation I had a free prospect of the river, which was a matter of no trivial consideration to me, having good reason to dread the subtle attacks of the alligators, who were crowding about my harbour. Having collected a good quantity of wood for the purpose of keeping up a light and smoke during the night, I began to think of preparing my supper, when, upon examining my stores, I found but a scanty provision, I thereupon determined, as the most expeditious way of supplying my necessities, to take my bob and try for some trout. About one hundred yards above my harbour, began a cove or bay of the river, out of which opened a large lagoon. The mouth or entrance from the river to it was narrow, but the waters soon after spread and formed a little lake, extending into the marshes, its entrances and sores within I observed to be verged with floating lawns of the pistia and nymphea and other aquatic plants; these I knew were excellent haunts for trout.

The verges and islets of the lagoon were elegantly embellished with flowering plants and shrubs; the laughing coots with wings half spread were tripping over the little coves and hiding themselves in the tufts of grass; young broods of the painted summer teal, skimming the still surface of the waters, and following the watchful parent unconscious of danger, were frequently surprised by the voracious trout, and he in turn, as often by the subtle, greedy alligator. Behold him rushing forth from the flags and reeds. His enormous body swells. His plaited tail brandished high, floats upon the lake. The waters like a cataract descend from his opening jaws. Clouds of smoke issue from his dilated nostrils. The earth trembles with his thunder. When immediately from the opposite coast of the lagoon, emerges from the deep his rival champion. They suddenly dart upon each other. The boiling surface of the lake marks their rapid course, and a terrific conflict commences. They now sink to the bottom folded together in horrid wreaths. The water becomes thick and discoloured. Again they rise, their jaws clap together, re-echoing through the deep surrounding forests. Again they sink, when the contest ends at the muddy bottom of the lake, and the vanquished makes a hazardous escape, hiding himself in the muddy turbulent waters and sedge on a distant shore. The proud victor exulting returns to the place of action. The shores and forests resound his dreadful roar, together with the triumphing shouts of the plaited tribes around, witnesses of the horrid combat.

My apprehensions were highly alarmed after being a spectator of so dreadful a battle; it was obvious that every delay would but tend to increase my dangers and difficulties, as the sun was near setting, and the alligators gathered around my harbour from all quarters; from these considerations I concluded to be expeditious in my trip to the lagoon, in order to take some fish. Not thinking it prudent to take my fusee with me, lest I might lose it overboard in case of a battle, which I had every reason to dread before my return, I therefore furnished myself with a club for my defence, went on board, and penetrating the first line of those which surrounded my harbour, they gave way; but being pursued by several very large ones, I kept strictly on the watch, and paddled with all my might towards the entrance of the lagoon, hoping to be sheltered there from the multitude of my assailants; but ere I had half-way reached the place, I was attacked on all sides, several endeavouring to overset the canoe. My situation now became precarious to the last degree: two very large ones attacked me closely, at the same instant, rushing up with their heads and part of their bodies above the water, roaring terribly and belching floods of water over me. They struck their jaws together so close to my ears, as almost to stun me, and I expected every moment to be dragged out of the boat and instantly devoured, but I applied my weapons so effectually about me, though at random, that I was so successful as to beat them off a little; when, finding that they designed to renew the battle, I made for the shore, as the only means left me for my preservation, for, by keeping close to it, I should have my enemies on one side of me only, whereas I was before surrounded by them, and there was a probability, if pushed to the last extremity, of saving myself, by jumping out of the canoe on shore, as it is easy to outwalk them on land, although comparatively as swift as lighting in the water. I found this last expedient alone could fully answer my expectations, for as soon as I gained the shore they drew off and kept aloof. This was a happy relief, as my confidence was, in some degree, recovered by it. On recollecting myself, I discovered that I had almost reached the entrance of the lagoon, and determined to venture in, if possible to take a few fish and then return to my harbour, while day-light continued; for I could now, with caution and resolution, make my way with safety along shore, and indeed there was no other way to regain my camp, without leaving my boat and making my retreat through the marshes and reeds, which, if I could even effect, would have been in a manner throwing myself away, for then there would have been no hopes of ever recovering my bark, and returning in safety to the settlements of men. I accordingly proceeded and made good my entrance into the lagoon, though not without opposition from the alligators, who formed a line across the entrance, but did not pursue me into it, nor was I molested by any there, though there were some very large ones in a cove at the upper end. I soon caught more trout than I had present occasion for, and the air was too hot and sultry to admit of their being kept for many hours, even though salted or barbecued. I now prepared for my return to camp, which I succeeded in with but little trouble, by keeping close to the shore;

[…]

It was by this time dusk, and the alligators had nearly ceased their roar, when I was again alarmed by a tumultuous noise that seemed to be in my harbour, and therefore engaged my immediate attention. Returning to my camp I found it undisturbed, and then continued on to the extreme point of the promontory, where I saw a scene, new and surprising, which at first threw my senses into such a tumult, that I was some time before I could comprehend what was the matter; however, I soon accounted for the prodigious assemblage of crocodiles at this place, which exceeded every thing of the kind I had ever heard of.

How shall I express myself so as to convey an adequate idea of it to the reader, and at the same time avoid raising suspicions of my want of veracity. Should I say, that the river (in this place) from shore to shore, and perhaps near half a mile above and below me, appeared to be one solid bank of fish, of various kinds, pushing through this narrow pass of St. Juan's into the little lake, on their return down the river, and that the alligators were in such incredible numbers, and so close together from shore to shore, that it would have been easy to have walked across on their heads, had the animals been harmless. What expressions can sufficiently declare the shocking scene that for some minutes continued, whilst this mighty army of fish were forcing the pass? During this attempt, thousands, I may say hundreds of thousands of them were caught and swallowed by the devouring alligators. I have seen an alligator take up out of the water several great fish at a time, and just squeeze them betwixt his jaws, while the tails of the great trout flapped about his eyes and lips, ere he had swallowed them. The horrid noise of their closing jaws, their plunging amidst the broken banks of fish, and rising with their prey some feet upright above the water, the floods of water and blood rushing out of their mouths, and the clouds of vapour issuing from their wide nostrils, were truly frightful. This scene continued at intervals during the night, as the fish came to the pass. After this sight, shocking and tremendous as it was, I found myself somewhat easier and more reconciled to my situation, being convinced that their extraordinary assemblage here, was owing to this annual feast of fish, and that they were so well employed in their own element, that I had little occasion to fear their paying me a visit.

III.

Thomas Malthus (1776–1834), in one of the most influential books in political economy, argued that populations will always grow faster than the supply of food over the long term, thus exposing man to the dual “checks of misery and vice”. This unhappy picture led Thomas Carlyle to label economics “the dismal science”, a moniker of the nineteenth century that noted not only the grip that Malthus’s predictions gained over the popular imagination (with cartoons of a Malthusian monster devouring children), but the miserable prognostications of ‘the stationary state’ (when economic growth ran out).

Malthus’s tract was written in a period of scientific discovery, utopian dreams about the social system, and romantic ideas about the future of man. He argued against these latter views because he could not see a way out of the population problem revealed by his analysis. He did however share the vigorous mode of argument of that period, celebrating the “blazing comet” of the French revolution, and he used his rhetorical powers to contrast his own reasonable scientific argument with the wonderful speculation that “man will ultimately become an ostrich”. (Rhetorical ‘speculations’ play a similar role for Lyell, see **VI.**) Malthus’s turn for words became a salient re-source for later evolutionary theorists, with Darwin picking up his phrase “the struggle for existence”.

His “postulata” can hardly be argued with, but the two statements that follow needed justification. It is often thought that he picked out of thin air his claim that population—unchecked—grows in geometrical ratio. But he justifies this by referring to the contemporary demography of the USA. For evidence on the other point: the arithmetic growth of food output, Malthus was living through the agricultural revolution in Britain, with experimental farming, fertiliser application, ‘spade husbandry’, and the cultivation of unused lands (see **II.** in Anthology I on Ricardo). But these activities had limits. His nicely framed arithmetic argument *proves* the greatness of the problem, and thus the “law of necessity” which checks population growth.

For the economist, the clever part is his explanation of the process by which this law works: it creates oscillations between experiences of “severe distress” and “tolerable comfort” as the population grows above or below growth in the output of food. Revealing the interaction between demographic and economic processes requires a ‘narrative argument’ of the kind that economists love to make that show how their laws work out in the activities of the real (or imagined) world. Such arguments are always complicated because the dynamic sequence is never linear or simple—it has to be narrative-ised to make sense. We don’t ‘see’ those regular oscillations, not only because there are many other relevant “interrupting causes”. Rather, the key—in technical economic terms—lies in the difference between the “nominal and real prices of labour” which hides the oscillations not only for us as observers but even to those actors in the economy on whose reactions all this narrative argument depends.

Mary S. Morgan

[See page 185 for further reading.]

1798: THOMAS ROBERT MALTHUS, *An Essay on the Principle of Population, as it affects the future improvement of society* (London: J. Johnson)

[EXTRACTS: pp. 1–34]

CHAPTER I.

The great and unlooked for discoveries that have taken place of late years in natural philosophy; the increasing diffusion of general knowledge from the extension of the art of printing; the ardent and unshackled spirit of inquiry that prevails throughout the lettered, and even unlettered world; the new and extraordinary lights that have been thrown on political subjects, which dazzle, and astonish the understanding; and particularly that tremendous phenomenon in the political horizon the French revolution, which, like a blazing comet, seems destined either to inspire with fresh life and vigour, or to scorch up and destroy the shrinking inhabitants of the earth, have all concurred to lead many able men into the opinion, that we were touching on a period big with the most important changes, changes that would in some measure be decisive of the future fate of mankind.

It has been said, that the great question is now at issue, whether man shall henceforth start forwards with accelerated velocity towards illimitable, and hitherto unconceived improvement; or be condemned to a perpetual oscillation between happiness and misery, and after every effort remain still at an immeasurable distance from the wished-for goal.

[…]

In entering upon the argument I must premise that I put out of the question, at present, all mere conjectures; that is, all suppositions, the probable realization of which cannot be inferred upon any just philosophical grounds. A writer may tell me that he thinks man will ultimately become an ostrich. I cannot properly contradict him. But before he can expect to bring any reasonable person over to his opinion, he ought to shew, that the necks of mankind have been gradually elongating; that the lips have grown harder, and more prominent; that the legs and feet are daily altering their shape; and that the hair is beginning to change into stubs of feathers. And till the probability of so wonderful a conversion can be shewn, it is surely lost time and lost eloquence to expatiate on the happiness of man in such a state; to describe his powers, both of running and flying; to paint him in a condition where all narrow luxuries would be contemned; where he would be employed only in collecting the necessaries of life; and where, consequently, each man's share of labour would be light, and his portion of leisure ample.

I think I may fairly make two postulata.

First, That food is necessary to the existence of man.

Secondly, That the passion between the sexes is necessary and will remain nearly in its present state.

These two laws, ever since we have had any knowledge of mankind, appear to have been fixed laws of our nature. [...]

[...]

Assuming then, my postulata as granted, I say, that the power of population is indefinitely greater than the power in the earth to produce subsistence for man.

Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio. A slight acquaintance with numbers will shew the immensity of the first power in comparison of the second.

By that law of our nature which makes food necessary to the life of man, the effects of these two unequal powers must be kept equal.

This implies a strong and constantly operating check on population from the difficulty of subsistence. [...]

[...]

And the race of man cannot, by any efforts of reason, escape from it. Among plants and animals its effects are waste of seed, sickness, and premature death. Among mankind, misery and vice. The former, misery, is an absolutely necessary consequence of it. Vice is a highly probable consequence, and we therefore see it abundantly prevail; but it ought not, perhaps, to be called an absolutely necessary consequence. The ordeal of virtue is to resist all temptation to evil.

[…]

CHAPTER II.

I said that population, when unchecked, increased in a geometrical ratio; and subsistence for man in an arithmetical ratio.

Let us examine whether this position be just.

[…]

In the United States of America, where the means of subsistence have been more ample, the manners of the people more pure, and consequently the checks to early marriages fewer, than in any of the modern states of Europe, the population has been found to double itself in twenty-five years.

This ratio of increase, though short of the utmost power of population, yet as the result of actual experience, we will take as our rule; and say,

That population, when unchecked, goes on doubling itself every twenty-five years, or increases in a geometrical ratio.

Let us now take any spot of earth, this Island for instance, and see in what ratio the subsistence it affords can be supposed to increase. We will begin with it under its present state of cultivation.

If I allow that by the best possible policy, by breaking up more land, and by great encouragements to agriculture, the produce of this Island may be doubled in the first twenty-five years, I think it will be allowing as much as any person can well demand.

In the next twenty-five years, it is impossible to suppose that the produce could be quadrupled. It would be contrary to all our knowledge of the qualities of land. The very utmost that we can conceive, is, that the increase in the second twenty-five years might equal the present produce. Let us then take this for our rule, [….] The most enthusiastic speculator cannot suppose a greater increase than this. In a few centuries it would make every acre of land in the Island like a garden.

Yet this ratio of increase is evidently arithmetical.

[…]

Let us now bring the effects of these two ratios together.

The population of the Island is computed to be about seven millions; and we will suppose the present produce equal to the support of such a number. In the first twenty-five years the population would be fourteen millions; and the food being also doubled, the means of subsistence would be equal to this increase. In the next twenty-five years the population would be twenty-eight millions; and the means of subsistence only equal to the support of twenty-one millions. In the next period, the population would be fifty-six millions, and the means of subsistence just sufficient for half that number. And at the conclusion of the first century, the population would be one hundred and twelve millions, and the means of subsistence only equal to the support of thirty-five millions; which would leave a population of seventy-seven millions totally unprovided for.

[…]

No limits whatever are placed to the productions of the earth; they may increase for ever and be greater than any assignable quantity; yet still the power of population being a power of a superior order, the increase of the human species can only be kept commensurate to the increase of the means of subsistence, by the constant operation of the strong law of necessity acting as a check upon the greater power.

[…]

The way in which these effects [checks] are produced seems to be this.

We will suppose the means of subsistence in any country just equal to the easy support of its inhabitants. The constant effort towards population, which is found to act even in the most vicious societies, increases the number of people before the means of subsistence are increased. The food therefore which before supported seven millions, must now be divided among seven millions and a half or eight millions. The poor consequently must live worse, and many of them be reduced to severe distress. The number of labourers also being above the proportion of the work in the market, the price of labour must tend toward a decrease; while the price of provisions would at the same time tend to rise. The labourer must therefore work harder to earn the same as he did before. During this season of distress, the discouragements to marriage, and the difficulty of rearing a family are so great, that population is at a stand. In the mean time the cheapness of labour, the plenty of labourers, and the necessity of an increased industry amongst them, encourage cultivators to employ more labour upon their land; to turn up fresh soil, and to manure and improve more completely what is already in tillage; till ultimately the means of subsistence become in the same proportion to the population as at the period from which we set out. The situation of the labourer being then again tolerably comfortable, the restraints to population are in some degree loosened; and the same retrograde and progressive movements with respect to happiness are repeated.

This sort of oscillation will not be remarked by superficial observers; and it may be difficult even for the most penetrating mind to calculate its periods. […]

[…]

One principal reason is, that the histories of mankind that we possess, are histories only of the higher classes. We have but few accounts that can be depended upon of the manner and customs of that part of mankind, where these retrograde and progressive movements chiefly take place. A satisfactory history of this kind, of one people, and of one period, would require the constant and minute attention of an observing mind during a long life. […]

Such a history would tend greatly to elucidate the manner in which the constant check upon population acts; and would probably prove the existence of the retrograde and progressive movements that have been mentioned; though the times of their vibration must necessarily be rendered irregular, from the operation of many interrupting causes; such as, the introduction or failure of certain manufactures: a greater or less prevalent spirit of agricultural enterprize: years of plenty, or years of scarcity: wars and pestilence: poor laws: the invention of processes for shortening labour without the proportional extension of the market for commodity: and, particularly, the difference between the nominal and real prices of labour; a circumstance which has perhaps more than any other, contributed to conceal this oscillation from common view.

IV.

In this passage from his book *Natural Theology* in 1802, Paley (1743–1805) draws together evidence from anatomy to describe the circulation of the blood and the purposes which it serves for an organism. The circulation, Paley notes, "compose a system, and testify a contrivance, perhaps the best under-stood of any part of the animal frame". He draws an analogy between the body and the water pipes of a town, but indicates that the circulatory system of the blood serves a purpose beyond those of civil engineering, and are well-adapted to play this role.

At the time Paley was writing, it was common for authors to praise simple machines and to disparage more complex ones for their trickiness, intricacy and the likelihood that they would break down. Paley does not adopt such an analytic, but rather the opposite. In presenting the circulation to us, he is building up a case using narrative means to explore and expose the complexity of the machine, rather than deploying the narrative equivalent of the anatomist's knife to make it simpler than it is. He follows the flow of the blood out to the extremities of the body through the arteries and back to the heart via the veins; his narrative leads the reader to follow the whole process, including how and where the arteries are kept safe. He keeps inviting the reader to pause, and to wonder at the details which he presents. In the closing parts of this excerpt, he writes: "Consider what an affair this is, when we come to very large animals.... Hear Dr. Hunter's account of the dissection of a whale...".

More recently, Paley, an English clergyman, is regarded as having provided a 'teleological' argument about the role of design in creation, offering a narrative of origins by reasoning backwards from the complexity of natural phenomena, via an analogy to machines such as a watch, and thus appealing to a designer, and hence to God. Paley's argument was (according to the popular view) then superseded by Charles Darwin's explanations of evolution through natural selection. That is, Darwin took such teleological notions and gave them a convincing rationale, which he developed through historical narrative. But, as Adam Shapiro has observed, this view of Paley as giving such teleological explanations of scientific phenomena was only articulated during the 1980s and 90s, by Richard Dawkins in his popular book *The Blind Watchmaker,* and then by Michael Behe in *Darwin's Black Box,* his defence of so-called "intelligent design" (Shapiro 2014, pp.115–116).

Thus, historians have argued that the generic framework within which Paley should be read decides how his claims can be evaluated. Was he writing popular science, as Dawkins suggested? Or, was he rather writing popular theology, intended to excite devotion in the reader by providing narratives pointing to the wonders of nature?

But for a reader who comes to this passage unaware of both Paley's early and later reputations, then it might communicate a sense of intricacy and awesome complexity without, it seems, referring to any larger claims about design. In terms of how narratives are read, Paley's text makes the rather neat point that even accounts that might be seen by some as teleological do not provide one unified design by which they are to be read, but are reshaped according to their readers' expectations.

Mat Paskins

[See page 185 for reference.]

1802: WILLIAM PALEY, *Natural Theology or, Evidences of the Existence and Attributes of the Deity, Collected from the Appearances of Nature* (London: R. Faulder)

[EXTRACT: pp. 106–110]

The circulation of the *blood*, through the bodies of men and quadrupeds, and the apparatus by which it is carried on, compose a system, and testify a contrivance, perhaps the best understood of any part of the animal frame. The lymphatic vessels, or the nervous system, may be more subtile and intricate; nay, it is possible that in their structure they may be even more artificial than the sanguiferous; but we do not know so much about them.

The utility of the circulation of the blood, I assume as an acknowledged point. One grand purpose is plainly answered by it; the distributing to every part, every extremity, every nook and corner, of the body, the nourishment which is received into it by one aperture. What enters at the mouth, finds its way to the fingers' ends. A more difficult mechanical problem could hardly I think be proposed, than to discover a method of constantly repairing the waste, and of supplying an accession of substance to every part, of a complicated machine at the same time.

This system presents itself under two views: first, the disposition of the blood vessels, i.e. the laying of the pipes; and, secondly, the construction of the engine at the centre, viz. the heart, for driving the blood through them.

I. The disposition of the blood-vessels, as far as regards the supply of the body, is like that of the water pipes in a city, viz. large and main trunks branching off by smaller pipes (and these again by still narrower tubes) in every direction, and towards every part, in which the fluid, which they convey, can be wanted. So far the water pipes, which serve a town, may represent the vessels, which carry the blood from the heart. But there is another thing necessary to the blood, which is not wanted for the water;

and that is, the carrying of it back again to its source. For this office a reversed system of vessels is prepared, which, uniting at their extremities with the extremities of the first system, collects the divided and subdivided streamlets, first by capillary ramifications into larger branches, secondly by these branches into trunks; and thus returns the blood (almost exactly inverting the order in which it went out) to the fountain from whence its motion proceeded. All which is evident mechanism.

The body, therefore, contains two systems of blood-vessels, arteries and veins. Between the constitution of the systems there are also two differences, suited to the functions which the systems have to execute. The blood, in going out, passing always from wider into narrower tubes; and, in coming back, from narrower into wider; it is evident, that the impulse and pressure upon the sides of the blood-vessels, will be much greater in one case than the other. Accordingly, the arteries which carry out the blood, are formed with much tougher and stronger coats, than the veins which bring it back. That is one difference: the other is still more artificial, or, if I may so speak, indicates, still more clearly, the care and anxiety of the artificer. Forasmuch as in the arteries, by reason of the greater force with which the blood is urged along them, a wound or rupture would be more dangerous, than in the veins, these vessels are defended from injury, not only by their texture, but by their situation; and by every advantage of situation which can be given to them. They are buried in sinuses, or they creep along grooves, made for them, in the bones; for instance, the under edge of the ribs is sloped and furrowed solely for the passage of these vessels. Sometimes they proceed in channels, protected by stout parapets on each side; which last description is remarkable in the bones of the fingers, these being hollowed out, on the under side, like a scoop, and with such a concavity that the finger may be cut across to the bone without hurting the artery which runs along it. At other times, the arteries pass in canals wrought in the substance, and in the very middle of the substance, of the bone: this takes place in the lower jaw; and is found where there would, otherwise, be danger of compression by sudden curvature. All this care is wonderful, yet not more than what the importance of the case required. To those, who venture their lives in a ship, it has been often said, that there is only an inch board between them and death; but in the body itself, especially in the arterial system, there is, in many parts, only a membrane, a skin, a thread. For which reason, this system lies deep under the integuments; whereas the veins, in which the mischief that ensues from injuring the coats is much less, lie in general above the arteries, come nearer to the surface, are more exposed.

It may be further observed concerning the two systems taken together, that, though the arterial, with its trunk and branches and small twigs, may be imagined to issue or proceed, in other words to *grow* from the heart, like a plant from its root, or the fibres of a leaf from its foot-stalk (which however, were it so, would be only to resolve one mechanism into another), yet the venal, the returning system, can never be formed in this manner. The arteries might go on shooting out from their extremities, i. e. lengthening and subdividing indefinitely; but an inverted system, continually uniting its streams, instead of dividing, and thus carrying back what the other system carried out, could not be referred to the same process.

II. The next thing to be considered is the engine which works this machinery, viz. the *heart*. For our purpose it is unnecessary to ascertain the principle upon which the heart acts. Whether it be irritation excited by the contact of the blood, by the influx of the nervous fluid, or whatever else be the cause of its motion, it is something, which is capable of producing, in a living muscular fibre, reciprocal contraction and relaxation. This is the power we have to work with: and the inquiry is, how this power is applied in the instance before us. There is provided in the central part of the body a hollow muscle, invested with spiral fibres, running in both directions, the layers intersecting one another; in some animals, however, appearing to be semi-circular rather than spiral. By the contraction of these fibres, the sides of the muscular cavities are necessarily squeezed together, so as to force out from them any fluid which they may at that time contain: by the relaxation of the same fibres, the cavities are in their turn dilated; and, of course, prepared to admit every fluid which may be poured into them. Into these cavities are inserted the great trunks, both of the arteries which carry out the blood, and of the veins which bring it back. This is a general account of the apparatus: and the simplest idea of its action is, that, by each contraction, a portion of blood is forced as by a syringe into the arteries; and, at each dilatation, an equal portion is received from the veins. This produces, at each pulse, a motion and change in the mass of blood, to the amount of what the cavity contains, which in a full grown human heart, I understand, is about an ounce, or two table-spoons full. How quickly these changes succeed one another, and by this succession how sufficient they are to support a stream or circulation throughout the system, may be understood by the following computation, abridged from Keill's Anatomy, p. 117. ed. 3. "Each ventricle will at least contain one ounce of blood. The heart contracts four thousand times in one hour; from which it follows, that there passes through the heart, every hour, four thousand ounces, or three hundred and fifty pounds, of blood. Now the whole mass of blood is said to be about twenty-five pounds, so that a quantity of blood equal to the whole mass of blood passes through the heart fourteen times in one hour; which is about once every four minutes." Consider what an affair this is, when we come to very large animals. The aorta of a whale is larger in the bore than the main pipe of the waterworks at London Bridge; and the water roaring in its passage through that pipe, is inferior, in impetus and velocity, to the blood gushing from the whale's heart. Hear Dr. Hunter's account of the dissection of a whale. "The aorta measured a foot diameter. Ten or fifteen gallons of blood is thrown out of the heart at a stroke with an immense velocity, through a tube of a foot diameter. The whole idea fills the mind with wonder*."

* Dr. Hunter's account of the dissection of a whale. Phil. Trans.

V.

Johann Elert Bode (1747–1826) was an eminent figure in astronomy from the 1780s to the 1820s. He was Director of the Berlin Observatory from 1786 until 1825, and his name is associated with the Titius-Bode law and with the nautical almanac, *Astronomisches Jahrbuch*, which he edited for several years. He was aware of the most recent advances in astronomy made by William Herschel in England, and he contributed to the dissemination of Herschel's cosmology on the continent, being also a fellow of the Royal Society.

In this short excerpt from *Betrachtung des Weltgebäudes* (*Contemplating the Structure of the Universe*), republished in the 1844 collection of Bode's works edited by Carl Bremiker (the passage also occurs in earlier editions), Herschel's view of heavenly structures and the constitution of the universe is compared with the systems of Kant and Lambert, and Herschel is presented as the one who gave definitive affirmation of their hypotheses thanks to his observations. It is worth noting that this excerpt is useful in reconstructing the narrative that astronomers in Prussia used to attempt to gain prestige and their own role in the rivalry between France and England regarding the prediction of the new planet of Neptune. Bode presents a research narrative regarding the structure of the Milky Way in a rigorous manner, reporting both the observations and the chronological order of the theories and hypotheses made by different authors, and interjecting them with his own observations.

Then there is a second type of narrative regarding the future possibility of measuring the motions of fixed stars. Its function is to build up a heuristic tool for speculative questions. Interestingly, this second type of narrative appeals to the laws of physics (e.g. gravity), but also leaves open a path for speculations regarding the probability (*Wahrscheinlichkeit*) that posterity might or might not find "the science, number and measure to determine exactly in advance the motion of the fixed stars in the firmament of the earth dweller in the immeasurable spaces after several centuries of the universe". The possibility of finding such a science depends on our faculties and on our capacity to improve our observations, i.e. on empirical grounds. In fact, Bode's reasoning also leaves open the possibility that human beings will never be able to predict certain types of motions due to the limits of their intellectual capacities. This is somewhat surprising considering that the Titius-Bode law, for instance, was supposed to predict the existence of celestial bodies by calculating orbital motions on the basis of proportions and Kepler's law, being grounded therefore on a purely mathematical foundation. Nevertheless, Bode did not discard the possibility of attributing our eventual lack of a science predicting orbital motions or relative motions of fixed stars to our faculties, rather than to the lack of data or technology. However, in the final footnote of the excerpt, Bode tries to reinforce the idea that we can reach a deeper understanding of the universe by mentioning the recent discoveries of binary systems that he himself reported in the *Astronomisches Jahrbuch* in 1815.

Silvia De Bianchi

1816: JOHAN ELERT BODE, Die Betrachtung des Weltgebäudes. In Bremiker C. (ed.) *Anleitung zur Kenntniß des gestirnten Himmels* (Berlin: Nicolai, 1844)

[EXTRACT: pp. 555–559]

[…]

Merkwürdig, höchst merkwürdig ist nämlich jener lichtschimmernde Bogen, der das ganze Firmament fast um die Mitte der Himmels kugel, zwar in ungleicher Breite, aber doch in ununterbrochenem Zusammenhange, umgiebt. Ich meine die sogenannte Milchstraße. Diese prachtvolle Zone ist der würdigste Gegenstand der Bewunderung und der Aufmerksamkeit*.

„So sehen wir", schreibt Lambert, „auf der Erde den Regenbogen uns in unzählbaren Tropfen das Bild der Sonne vorstellen;" so scheint „der große Schöpfer die Tropfen des Lichts, in welchem er wohnt, um den Himmel herum ausgebreitet zu haben." Man könnte fragen: Woher ist in dieser prachtvollen Zone die Menge der Sterne gegen die in allen andern Gegenden auf einmal so zahllos? Hieraus haben schon Kant, Lambert und nun auch Herschel gefolgert, daß höchst wahrscheinlich die Sterne der Milchstraße in Vergleichung mit den übrigen nicht wirklich näher beisammen stehen, wie es das Ansehn hat, sondern in den unergründlichen Tiefen des Himmels in unermeßlichen Reihen und Schichten hinter einander liegen, und nur daher dort hinaus mehr angehäuft oder gedrängter beisammen erscheinen, als in den übrigen Gegenden des Weltraums, wo wir sie von der Seite oder der Fläche nach sehen**. Hiernach wäre also das ganzeHeer dieser Weltsysteme nicht kugelähnlich, sondern in einer flachen, gleichsam linsenförmigen oder schichtenähnlichen Gestalt aufgestellt, und mitten unter diesen gesammten Systemen und Sternenheeren der Milchstraße glänzt auch unsere Sonne als ein Stern. Daher werden alle Sterne, die wir senkrechter oder längs der größten Durchschnittsebene dieser Schicht nach allen Seiten im Kreise herum sehen, unsere sogenannte Milchstraße ausmachen, die übrigen seitwärts herum stehenden aber am ganzen Himmel zerstreut erscheinen. Wir scheinen ferner mit unserm Sonnensysteme etwas seitwärts außer der der Länge nach mitten hindurch gehenden Ebene dieses allgemeinen Fixsternen- oder Milchstraßensystems zu liegen, weil die scheinbare Figur der Milchstraße nicht völlig ein größter Kreis der Himmelskugel ist, indem sie dem Südpol um 10 Grad näher als dem Nordpol vorbei geht. Endlich scheinen wir nicht im Mittelpunkte jener Kreisebene, sondern beträchtlich davon entfernt zu liegen, und zwar nach derjenigen Seite der Milchstraße hin, wo wir den Schwan, Fuchs mit der Gans, Pfeil, Adler &c. sehen, weil hier dieselbe viel breiter und heller sich zeigt, auch ihre Sterne zerstreuter erscheinen, als dieser Gegend gerade gegenüber, wo der Fuhrmann, die Zwillinge, Orion, der große Hund &c. glänzen. Nach dieser Voraussetzung hätten im Allgemeinen alle Systeme der Fixsterne in ihrer Stellung gegen einander auf ähnliche Art eine Beziehung auf die Milchstraße, wie die Planeten unsers Sonnensystems auf den Thierkreis***.

Es ist sonderbar, daß die Astronomen nicht schon längst aus der an unserm Firmaments so äußerst merkwurdig erscheinenden Gestalt und Lage der Milchstraße, die beide von einem ungefähren Zufall sehr unterscheidende Merkmale mit sich führen, dergleichen Folgerungen über die Austheilung der Fixsterne im Weltraume gewagt haben, die zugleich den Beweis aufstellen, daß auch auf dem unermeßlichen Schauplatze des Universums sich die deutlichsten Spuren ordnungsvoller Plane eines weisen Schöpfers offenbaren. Wer will es auch dem Bewohner der Erde als eine unerlaubte Kühnheit anrechnen, über die Lage der gesammten Fixsternensysteme nachzudenken und Schlüsse zu wagen, da ihm selbst der sinnliche Anblick des Sterngewölbes hierzu Veranlassung darbietet?

Die neuere Sternkunde lehrt ferner, daß die Fixsterne, welche man sonst für unbeweglich gehalten, wirklich eine eigene Bewegung haben, die wir aber, ihrer ungeheuern Entfernung wegen, erst nach einer langen Reihe von Jahren bemerken können****. Das gesammte Heer der Fixsterne oder Sonnen, welches, nach der obigen Vorstellung, unsere Milchstraße bildet, wird sich also vermuthlich gemeinschaftlich in Kreisen um eine im

Mittelpunkte derselben liegende große Sonne bewegen. Dieser majestätische Centralkörper muß, zufolge der vorigen Erklärung, nach der Seite hinaus anzutreffen sein, wo uns die Milchstraße am schmalsten und im schwächsten Lichtschimmer erscheint, und, weil wir nicht genau in der größten Ebene derselben liegen, am Firmamente etwas außerhalb der Milchstraße stehen. Da nun beides gerade beim Sirius zutrifft, bei welchem nahe ostwärts die Milchstraße in ihrer fast geringsten Breite und nur in einem schwach dämmernden Lichte vorbei geht, so sind einige Sternkundige veranlaßt worden, diesem schönsten Fixstern am Himmel einen solchen Rang zuzugestehen. Auf diese Art würde, in Betreff der Stellung der einzelnen Weltkörper, das ganze Fixsternensystem der Milchstraße im Großen etwa das sein, was unser und alle andere Sonnensysteme dagegen im Kleinen sind. Welche Vorstellung haben wir uns von dieser Centralsonne zu machen, auf welche eine zahllose Menge Sonnen mit ihrem Gefolge von Planeten eine Beziehung haben! Muß nicht ihre Masse und Größe ihrer weiten Alleinherrschaft angemessen sein? Und verräth nicht vielleicht die uns in die Augen fallende vorzügliche Pracht des Sirius seine hohe Würde?

Welche Veränderungen gehen nun hiernach im Raume des Weltalls vor, wenn nicht allein Monde um ihre Planeten laufen, Planeten und Kometen um Sonnen sich fortwälzen, sondern wenn ganze Weltsysteme wieder die Herrschaft noch größerer Körpermaßen erkennen und in den unendlichen Gefilden des Weltraums auf weit ausgedehnten Laufbahnen dahereilen. Die sich hieraus ergebende allgemeine Schlußfolge, daß keine Kugel der Schöpfung in einer absoluten Ruhe sei, sondern vielmehr das ganze Heer derselben mit einander in Verbindung stehe und beständig fortwandere, ließ sich auch schon zum Theil im Voraus als richtig annehmen. Die Bewegung ist eine wesentlich nothwendige Eigenschaft der Körperwelt: ohne sie würde dieselbe einer abgenutzten Maschine, einer unwirksamen und todten Masse gleichen, und der weisheitsvolle Plan der Schöpfung, welcher beständig neue Scenen, Veränderungen, Mannigfaltigkeiten und Abwechselungen fordert, nicht erfüllt werden. Kennen wir gleich jetzt noch nicht die Gesetze, nach welchen sich ganze Sonnensysteme verrücken, und fehlt uns die Wissenschaft, Zahl und Maaß genau im Voraus zu bestimmen, wie viel nach Ablauf mehrerer Jahrhunderte die Bewegung der Fixsterne am Firmamente des Erdbewohners und in den unermeßlichen Räumen des Weltalls austragen wird, so ist es doch höchst wahrscheinlich, daß unsere Nachwelt durch mehrere und genauere Beobachtungen sich dieser Kenntniß stufenweise nähern wird*****.

Allein was erhält nun auch jene unzählbaren Sonnensysteme in ihrer unverrückten Harmonie und Ordnung? Welches mächtige Band verbindet sie alle gleichsam als gemeinschaftliche Glieder jener großen Naturkette, die alles, was vorhanden ist, umschlingt? Wir kennen kein anderes, als die mit der ursprünglichen Wurfbewegung stets vergesellschaftete Schwere oder Anziehungskraft.

---

* Der Name Milchstraße ist abgeschmackt und fabelhaft, und daher diesem über alle unsere Begriffe erhabenen Gegenstände bei weitem nicht angemessen. Lichttöne, Sternengürtel, Sternendiadem könnten etwa in menschlicher Sprache würdigere Benennungen sein, Klopstock nennt sie in seiner Ode „Dem Unendlichen" die Straße voll Glanz.

** Eben so, wie wir in einem Walde die in langen Reihen hinter einander stehenden Bäume gedrängter sehen, als diejenigen, welche wir zur Seite, um und neben uns haben.

*** Kant und Lambert haben über diese und andere damit verwandte Materien, mit den tiefsten philosophischen und astronomischen Einsichten, Gedanken niedergeschrieben und Muthmaßungen gewagt, welche dem erhabenen Gegenstande, den sie abhandeln, angemessen sind, und von Allen, die sich vom Weltbau würdige Begriffe machen wollen, recht sehr verdienen, gelesen und überdacht zu werden. Von Kants allgemeiner Naturgeschichte und Theorie des Himmels ist 1798 zu Königsberg eine neue Ausgabe, mit des Verfassers eigenen Berichtigungen und mit Anmerkungen bereichert, erschienen, und von Lamberts kosmologischen Briefen über die Einrichtung des Weltbaues hat der Baron v. Utenhove zu Utrecht eine französische Uebersttzung mit vielen erläuternden Anmerkungen im Jahre 1801 zu Amsterdam herausgegeben. Endlich trägt der berühmte Herschel in seinen Abhandlungen „Ueber den Bau des Himmels“ die 1791 zu Königsberg, aus dem Englischen übersetzt, herausgekommen, und die auch auszugsweise mit Erläuterungen in meinen astronomischen Jahrbüchern für 1788 und 1794 stehen, ganz ähnliche Erklärungen über die Austheilung und Lage der Fixsternen-Systeme der Milchstraße vor, und beschäftigt glücklich manche Ideen und Muthmaßungen jener beiden Philosophen durch Beobachtungen.

**** Herschel, Prevost und Andere haben gezeigt, daß ein Theil der scheinbaren Bewegung, welche man an verschiedenen Fixsternen bisher bemerkt, dadurch sich erklären lasse, daß unsere Sonne selbst (als ein Fixstern) mit ihren, ganzen Gefolge ihren Ort im Weltraume ändere, und zwar nach der Gegend hin fortrücke, wo wir das Gestirn des Herkules sehen. Siehe mein astronom. Jahrbuch für 1786 Seite 259, 1787 Seite 224, und 1805 Seite 113 u. folg.

***** Man hat seit wenigen Jahren die äußerst wichtige Entdeckung gemacht, daß mehrere Doppelsterne ihre Stellung langsam gegen einander verändern, und entweder der kleine um den größeren fortschreitet, oder beide um ihren gemeinsamen Schwerpunkt sich drehen. Das ist eine ganz neue Ansicht; so laufen also sich benachbarte Sonnen um einander! (Siehe astronom. Jahrb. für 1815, 17, 19, 24, 25.)

[TRANSLATION]

1816: JOHAN ELERT BODE, Contemplating the Structure of the Universe. In Bremiker, C. (ed.) *A Guide to the Knowledge of the Starry Sky* (Berlin: Nicolai, 1844)

[…]

Even more remarkable is that light-shimmering arch that surrounds the whole firmament almost around the middle of the sky, in uneven width, but in an uninterrupted fashion. I mean the so-called Milky Way. This magnificent zone is the worthiest object of admiration and attention*.

"So we see," writes Lambert, "on earth the rainbow presents us with the innumerable drops of the image of the sun;" thus "the great Creator seems to have spread the drops of light in which he lives around the sky." One might ask: why in this splendid zone is the quantity of stars against those in all other regions suddenly so numerous? From this, Kant, Lambert and now Herschel have already concluded that the stars of the Milky Way are most likely not really closer together in comparison with the others, as is generally seen, but rather in the unfathomable depths of the sky in huge rows and layers one behind the other and therefore only appear more clustered or crowded there than in the other regions of space, where we can see them from one side or from the surface**. According to this, the whole mass of these world systems would not be spherical, but would be set up in a flat, lenticular or layer-like shape, and in the midst of these entire systems and host of stars in the Milky Way our sun shines as a star. Therefore, all of the stars that we see in circles in all directions, perpendicular or along the largest average level of this layer, will form our so-called Milky Way, but the remaining ones standing sideways will appear scattered throughout the sky. We also seem to be lying a bit obliquely with regard to our solar system, apart from the plane of this general fixed-star or Milky Way system that runs through the middle, because the apparent figure of the Milky Way is not entirely a large circle of the celestial globe, in that it passes 10 degrees closer to the South Pole than to the North Pole. At least we don't seem to be at the centre of that circular plane, but rather considerably away from it, towards the side of the Milky Way where we see Cygnus, Vulpecula, Lyra, Sagitta, Aquila, because here it is much wider and brighter, their stars also appear more scattered than just in front of this area, where Auriga, Gemini, Orion, Canis Major shine. According to this assumption, each row of fixed stars in their relative positions would have a relationship with the Milky Way in a similar way as the planets of our solar system with the zodiac***.

It is strange that astronomers have not long since dared to draw such conclusions about the distribution of the fixed stars in space from the shape and position of the Milky Way, which appears to be extremely unusual in our firmament, both are features that are very different from an approximate coincidence and at the same time the proof that even on the immense scene of the universe the clearest traces of orderly plans of a wise creator are revealed. Who wants to credit the inhabitants of the earth with an unauthorized audacity to think about the location of the entire fixed star systems and to dare to conclude, since even the sensual view of the star vault gives him reason to do so?

Recent astronomy also teaches that the fixed stars, which are otherwise thought to be immovable, in fact really have their own movement, which, however, because of their immense distance, we can only notice after a long series of years****. The entire mass of fixed stars or suns, which, according to the above idea, forms our Milky Way, will probably move together in circles around a large sun in the centre of it. According to the previous explanation, this majestic central body must be found on the side where the Milky Way appears to us most narrow and in the faintest glow of light, and, because we are not exactly on the thickest level of it, it must stand a little outside the Milky Way in the firmament. Since both conditions apply to Sirius, where the Milky Way passes almost eastwards in its narrowest width and only in a very dim light, some astronomers have been asked to give such a rank to this most beautiful fixed star in the sky. In this way, in terms of the position of the individual world bodies, the entire fixed star system would be for the Milky Way on a large scale roughly what our and all other solar systems are on the small scale for it. What idea do we have of this central sun, to which a multitude of suns have a relationship with their entourage of planets! Doesn't their mass and size have to be commensurate with their wide rule? And perhaps the exquisite splendour of Sirius, which catches our eye, betrays his dignity?

What changes now take place in space, when not only moons run around their planets, planets and comets around the sun, but when entire world systems recognize the domination of even larger bodies and in the infinite realm of space speed around on vast trajectories? The general conclusion that follows from this, that no sphere of creation is in absolute rest, but rather that the whole mass of them is connected to one another and continually migrates, can already be partially accepted as correct in advance.

Motion is an essential property of the physical world: without it, it would be like a damaged machine, an ineffective and dead mass, and the wise plan of creation, which constantly calls for new scenes, changes, manifolds and alternations, would not be fulfilled. We don't know yet the laws according to which entire solar systems move, and we lack the science, number and measure to determine exactly in advance the motion of the fixed stars in the firmament of the earth dweller in the immeasurable spaces after several centuries of the universe; it is highly probable that our posterity will gradually approach this knowledge through several and more precise observations*****.

But what holds those innumerable solar systems in their undamaged harmony and order? Which mighty bond connects them all as if they were common links in that great natural chain that wraps around everything that is there? We know of no other than the gravity or attraction that is always associated with the original throwing motion. But perhaps the world creator has put other forces into the nature of the world bodies, which the restricted human mind will never ponder.

---

* The name Milky Way is vulgar and fabulous, and therefore far from being appropriate for this object above all of our terms. Light tones, star belts, star diadem could be more worthy terms in human language, in his ode The Infinite, Klopstock calls it the street full of splendour.

** Just as when we see trees standing in long rows one behind the other in a forest and consider them to be more than those we have on one side, around and next to us.

*** Kant and Lambert have written down thoughts and other assumptions about these and other related matters, with the deepest philosophical and astronomical insights, which are appropriate to the sublime subject that they deal with, and by everyone who wants to make concepts worthy of world architecture, very much deserve to be read and reconsidered. A new edition of Kant's Universal Natural History and Theory of Heavens was published in Königsberg in 1798, enriched with the author's own corrections and annotations, and Baron v. Utenhove of Utrecht published a French translation with many explanatory notes in Amsterdam in 1801. Most recently the famous Herschel in his treatises On the Construction of Heavens, which came out translated from English in Königsberg in 1791, and which I consider in my astronomical yearbooks for 1788 and 1794, contains very similar explanations about the distribution and location of the fixed-star systems of the Milky Way, and happily confirms some ideas and presumptions of these two philosophers through observations.

**** Herschel, Prevost and others have shown that a part of the apparent movement, which has so far been observed in various fixed stars, can be explained by the fact that our sun itself (as a fixed star) with its entire retinue changes its location in space, namely towards the area where we see the star of Hercules. See my Astronomer's Yearbook for 1786 page 259, 1787 page 224, and 1805 page 113 and following.

***** A very important discovery has been made for a few years that several binary stars are slowly changing their positions relative to one another, and that either the small one progresses around the larger one, or both revolve around their common centre of gravity. This is a completely new perspective; "I run neighbouring suns around each other!" (See Astronom. Yearb. for 1815, 17, 19, 24, 25.)

VI.

Dissatisfied with his initial choice of career in the London legal profession, Charles Lyell (1797–1875) turned his ambitions to the burgeoning science of geology where he soon found his true vocation, and by the early 1820s he had established himself in geology's social and institutional structures. His *Principles of Geology*, initially published in three volumes between 1830 and 1833, made an immediate impact and was instrumental in the development of geology (and biology) in the nineteenth century. It went through eleven editions (the last in 1872), and its influence has continued into the twenty-first century.

In this passage, and indeed throughout the first volume of *Principles*, Lyell's preoccupation was to convince his readers that the geological processes that had operated in the past were no different "in kind or degree" (ibid p.75) from those presently observable, and that geological time was very much longer than was generally assumed. He noted that geologists of earlier generations had assumed that the physical features ("monuments") of the surface of the Earth such as mountain chains must have been formed by processes that were "far more energetic" than those that were presently observable. One of these "first cultivators of geology", John Woodward (1665–1728), had ascribed the appearance of the surface of the Earth entirely to the catastrophic effects of the biblical deluge acting over "the course of a few months". In contrast, Lyell conceived of the Earth in former times as a place of slow-moving change, and what may once have been assumed to be extraordinary events were actually part of "the regular course of events", so major features like mountains were formed by the cumulative effect of small, incremental earthquake movements that may have only occurred once a century. This cumulative process of change was a difficult concept to grasp because of humankind's limited ability to apprehend the big picture both spatially and temporally. The implication was that if only we were able to view all the Earth's workings in their entirety, then we would understand that everything could be explained by processes operating over great spans of time.

Lyell's account of Earth history is not universally regarded as a narrative (e.g. Secord 2007; Buckland 2010). But while it is true that he did not emphasise a beginning or an ending, and his account had no discernible trajectory, Lyell's version of Earth history did include numerous elements typical of narrative including metaphor, analogy, rhetoric, and anthropomorphism; it also posited causal connections between events—a further characteristic of narratives (and like Malthus in **III.**, Lyell contrasted his sober reasoning against others' speculations, in this case about Egypt's mummies—another kind of monument). As such, Lyell's uniformitarianism has functioned as a grand narrative which has guided the practice of geological interpretation since the *Principles* first appeared. However, because Lyell sought to invoke as geological explanations only processes and events no different "in kind or degree" from those presently observable, he imposed an *a priori* gradualistic assumption on the past. This resulted in extreme catastrophic events which had never been observed (such as meteorite impacts) being precluded from the range of potential geological explanations, a flaw which has only been addressed in recent decades.

Andrew Hopkins

[See page 186 for references.]

1830: CHARLES LYELL, *Principles of Geology Volume 1* (London: John Murray)

[EXTRACT: pp. 75–79]

CHAPTER V.

> Review of the causes which have retarded the progress of Geology [...]

We have seen that, during the progress of geology, there have been great fluctuations of opinion respecting the nature of the causes to which all former changes of the earth's surface are referrible. The first observers conceived that the monuments which the geologist endeavours to decipher, relate to a period when the physical constitution of the earth differed entirely from the present, and that, even after the creation of living beings, there have been causes in action distinct in kind or degree from those now forming part of the economy of nature. These views have been gradually modified, and some of them entirely abandoned in proportion as observations have been multiplied, and the signs of former mutations more skilfully interpreted. Many appearances, which for a long time were regarded as indicating mysterious and extraordinary agency, are finally recognized as the necessary result of the laws now governing the material world; and the discovery of this un-looked for conformity has induced some geologists to infer that there has never been any interruption to the same uniform order of physical events. The same assemblage of general causes, they conceive, may have been sufficient to produce, by their various combinations, the endless diversity of effects, of which the shell of the earth has preserved the memorials, and, consistently with these principles, the recurrence of analogous changes is expected by them in time to come.

Whether we coincide or not in this doctrine, we must admit that the gradual progress of opinion concerning the succession of phenomena in remote eras, resembles in a singular manner that which accompanies the growing intelligence of every people, in regard to the economy of nature in modern times. In an early stage of advancement, when a great number of natural appearances are unintelligible, an eclipse, an earthquake, a flood, or the approach of a comet, with many other occurrences afterwards found to belong to the regular course of events, are regarded as prodigies. The same delusion prevails as to moral phenomena, and many of these are ascribed to the intervention of demons, ghosts, witches, and other immaterial and supernatural agents. By degrees, many of the enigmas of the moral and physical world are explained, and, instead of being due to extrinsic and irregular causes, they are found to depend on fixed and invariable laws. The philosopher at last becomes convinced of the undeviating uniformity of secondary causes, and, guided by his faith in this principle, he determines the probability of accounts transmitted to him of former occurrences, and often rejects the fabulous tales of former ages, on the ground of their being irreconcilable with the experience of more enlightened ages.

As a belief in want of conformity in the physical constitution of the earth, in ancient and modern times, was for a long time universally prevalent, and that too amongst men who were convinced that the order of nature is now uniform, and has continued so for several thousand years; every circumstance which could have influenced their minds and given an undue bias to their opinions deserves particular attention. Now the reader may easily satisfy himself, that, however undeviating the course of nature may have been from the earliest epochs, it was impossible for the first cultivators of geology to come to such a conclusion, so long as they were under a delusion as to the age of the world, and the date of the first creation of animate beings. However fantastical some theories of the sixteenth century may now appear to us,—however unworthy of men of great talent and sound judgment, we may rest assured that, if the same misconceptions now prevailed in regard to the memorials of human transactions, it would give rise to a similar train of absurdities. Let us imagine, for example, that Champollion, and the French and Tuscan literati now engaged in exploring the antiquities of Egypt, had visited that country with a firm belief that the banks of the Nile were never peopled by the human race before the beginning of the nineteenth century, and that their faith in this dogma was as difficult to shake as the opinion of our ancestors, that the earth was never the abode of living beings until the creation of the present continents, and of the species now existing,—it is easy to perceive what extravagant systems they would frame, while under the influence of this delusion, to account for the monuments discovered in Egypt. The sight of the pyramids, obelisks, colossal statues, and ruined temples, would fill them with such astonishment, that for a time they would be as men spell-bound—wholly incapacitated to reason with sobriety. They might incline at first to refer the construction of such stupendous works to some superhuman powers of a primeval world. A system might be invented resembling that so gravely advanced by Manetho, who relates that a dynasty of gods originally ruled in Egypt, of whom Vulcan, the first monarch, reigned nine thousand years. After them came Hercules and other demi-gods, who were at last succeeded by human kings. When some fanciful speculations of this kind had amused the imagination for a time, some vast repository of mummies would be discovered and would immediately undeceive those antiquaries who enjoyed an opportunity of personally examining them, but the prejudices of others at a distance, who were not eye-witnesses of the whole phenomena, would not be so easily overcome. The concurrent report of many travellers would indeed render it necessary for them to accommodate ancient theories to some of the new facts, and much wit and ingenuity would be required to modify and defend their old positions. Each new invention would violate a greater number of known analogies; for if a theory be required to embrace some false principle, it becomes more visionary in proportion as facts are multiplied, as would be the case if geometers were now required to form an astronomical system on the assumption of the immobility of the earth.

Amongst other fanciful conjectures concerning the history of Egypt, we may suppose some of the following to be started. “As the banks of the Nile have been so recently colonized, the curious substances called mummies could never in reality have belonged to men. They may have been generated by some *plastic* *virtue* residing in the interior of the earth, or they may be abortions of nature produced by her incipient efforts in the work of creation. For if deformed beings are sometimes born even now, when the scheme of the universe is fully developed, many more may have been "sent before their time, scarce half made up," when the planet itself was in the embryo state. But if these notions appear to derogate from the perfection of the Divine attributes, and if these mummies be in all their parts true representations of the human form, may we not refer them to the future rather than the past? May we not be looking into the womb of nature, and not her grave? May not these images be like the shades of the unborn in Virgil's Elysium—the archetypes of men not yet called into existence?"

These speculations, if advocated by eloquent writers, would not fail to attract many zealous votaries, for they would relieve men from the painful necessity of renouncing preconceived opinions. Incredible as such scepticism may appear, it would be rivalled by many systems of the sixteenth and seventeenth centuries, and among others by that of the learned Falloppio, who regarded the tusks of fossil elephants as earthy concretions, and the vases of Monte Testaceo, near Rome, as works of nature, and not of art. But when one generation had passed away, and another not compromised to the support of antiquated dogmas had succeeded, they would review the evidence afforded by mummies more impartially, and would no longer controvert the preliminary question, that human beings had lived in Egypt before the nineteenth century: so that when a hundred years perhaps had been lost, the industry and talents of the philosopher would be at last directed to the elucidation of points of real historical importance.

But we have adverted to one only of many prejudices with which the earlier geologists had to contend. Even when they conceded that the earth had been peopled with animate beings at an earlier period than was at first supposed, they had no conception that the quantity of time bore so great a proportion to the historical era as is now generally conceded. How fatal every error as to the quantity of time must prove to the introduction of rational views concerning the state of things in former ages, may be conceived by supposing that the annals of the civil and military transactions of a great nation were perused under the impression that they occurred in a period of one hundred instead of two thousand years. Such a portion of history would immediately assume the air of a romance; the events would seem devoid of credibility, and inconsistent with the present course of human affairs. A crowd of incidents would follow each other in thick succession. Armies and fleets would appear to be assembled only to be destroyed, and cities built merely to fall in ruins. There would be the most violent transitions from foreign or intestine war to periods of profound peace, and the works effected during the years of disorder or tranquillity would be alike superhuman in magnitude.

VII.

The son of a blacksmith, Michael Faraday (1791–1867) had little formal schooling and was apprenticed to a bookbinder at the age of 13. Being fascinated by science, he read copiously and in 1812 attended a course of lectures by the eminent chemist Humphry Davy at the Royal Institution (RI) in London. Davy was greatly impressed by Faraday's notes of those lectures, and in the following year Faraday became Davy's assistant. In the early 1820s Faraday established himself as an innovative experimentalist, his first major discovery being electromagnetic rotations.

The reader of this excerpt is first struck by the numbers at the start of each paragraph. The text clearly forms part of a much longer narrative, being preceded by 1160 other paragraphs and followed by a further 2266 paragraphs. We are, then, in the midst of an extensive research project conducted by Faraday to understand the relationship between electricity and matter which, as para. 1161 makes clear, he considered of great scientific significance. Faraday contributed 30 papers or "series" to the *Philosophical Transactions of the Royal Society of London* between 1832 and 1856. Except for "series 30", they were reprinted in 3 volumes with the title *Experimental Researches in Electricity* (hereafter *ERE*). Faraday's published account of his cage experiment was based on the research he pursued on 15th January 1836 and recorded in his *Experimental Notebook* (hereafter *EN*), paras. 2808–59.

ERE constituted a grand unified project that occupied much of Faraday's working life. By numbering his paragraphs he imposed order on his narrative and provided a ready way of cross-referencing topics discussed elsewhere in the sequence. He also used numbered paragraphs in *EN*, the final entry being para. 16041.

Para. 1173 begins with a description of the cage, which is very similar to the opening sentences of para. 2808 in *EN*, which also includes the sketch in the image included here on p 48.

Para. 1174 contains Faraday's account of what is now known as Faraday's Cage, in which the cube was connected to a powerful electrostatic generator con-sisting of stationery cushions rubbing against a revolving glass plate "fifty inches in diameter". He entered the highly charged cage and, using a sensitive electrometer, found no charge inside it (cf. para. 2852 in *EN*). For Faraday, this was a dramatic confirmation of his hypothesis that there was "no absolute and independent charge" inside a non-conductor.

David Gooding (1990) discussed the complex interactions between theorising and experimentation in Faraday's work. Underpinning the cage experiment were a number of theoretical concerns. The most important was Faraday's dissention from the prevalent view among contemporaries that a charged body influences other bodies at a distance by an electrical force. This force (like Newton's gravitational force) acts in straight lines, its intensity decreasing as the square of the distance—the intervening space being empty. Instead, drawing on his earlier electrochemical researches, Faraday rejected the notion of empty space and envisaged that the particles comprising any medium are electrically polarised—para. 1165. A line of force would thus consist of a string of spatially extended particles, each having a positive charge at one end and an equal negative charge at the other; the positive charge on one particle being adjacent to the equal negative charge of its neighbour. Thus Faraday's theory—unlike the opposing theory—excluded the possibility of "absolute and independent charge[s]" inside the cage—which is just what he observed. The cage experiment described in para. 1174 was a crucial experiment that impressively confirmed Faraday's theory of electrical action.

Geoffrey Cantor

[See page 186 for reference.]

1838: MICHAEL FARADAY, Experimental Researches in Electricity, Eleventh Series *Philosophical Transactions of the Royal Society of London* vol. 128

[EXTRACT: pp. 1–5]

§18.On Induction. [...]

Received [by the Royal Society of London] November 30,—Read December 21, 1837

i. Induction an action of contiguous particles.

1161. The science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its overruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1165. [...] At present I believe ordinary induction in all cases to be an action of contiguous particles consisting of a species of polarity, instead of being an action of either particles or masses at a sensible distance; and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electrical forces. [...]

ii. On the absolute charge of matter.

1169. Can matter either conducting or non-conducting be charged with one electric force independently of the other in the least degree either in a sensible or latent state?

1170. [Cites evidence that "prove that conductors cannot be bodily charged".] [...]

1171. With regard to electrics or non-conductors the conclusion does not at first seem so clear. [...]

[Discusses experiments using oil of turpentine, which confirmed the absence of absolute charge.]

[...]

1173. I carried these experiments [with non-conductors] on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connection with the wires, and supplied in every direction with bands of tin foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution [...].

[Faraday then carried out an experiment with the cage and concluded that] Every attempt to charge air bodily and independently with the least portion of either electricity failed.]

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an outside communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least signs of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface.

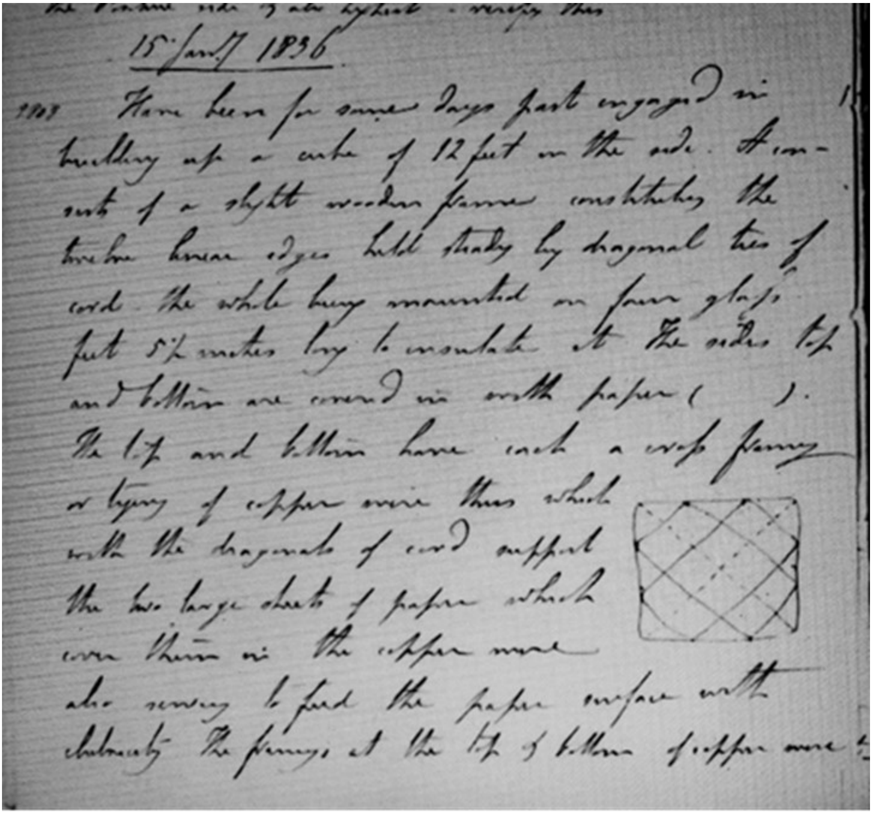
The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

[The following excerpts and image are taken from Faraday's Experimental Notebook, RI MS F2 E, f. 596.]

15 January 1836

2808. Have been for some days past engaged in building up a cube of 12 feet in the side. It consists of a slight wooden frame constituting the twelve linear edges held steady by diagonal ties of cord; the whole being mounted on four glass feet, 5½ inches long, to insulate it. The sides, top and bottom are covered in with paper. The top and bottom have each a cross framing or tying of copper wire thus which with the diagonals of cord support the two large sheets of paper which cover them in, the copper wire also serving to feed the paper surface with electricity.

2852. I now went inside the cube, standing on the stool and Anderson [his assistant] worked the [electrical] machine until the cube was fully charged & he continued working the machine. I could by no appearance find any traces of electricity in myself or the surrounding objects. I could not affect the gold leaf electrometer within. But if I brushed it with a flannel it was excited in the usual way.



15 Jan^y 1836.

2008 Have been for some days past engaged in building up a cube of 12 feet in the side. It consists of a slight wooden frame constituting the twelve linear edges held steady by diagonal ties of cord the whole being mounted on four glass feet 5½ inches long to insulate it. The sides top and bottom are covered in with paper ( ). The top and bottom have each a cross framing or tying of copper wire thus which with the diagonals of cord support the two large sheets of paper which cover them in the copper wire also serving to feed the paper surface with electricity. The framings at the top & bottom of copper wire

VIII.

Henry Piddington (1797–1858) became famous for his work on tropical storms in the Bay of Bengal. He was the first to coin the term ‘cyclone’ to describe the storms in the Bay, based on their resemblance to “the coil of a snake” Piddington 1848:8).

Piddington was a British sailor who docked his ship in Bengal in 1824 and remained there till his death in 1858, serving as the Foreign Secretary to the Agricultural Society of India, a secretary to the Asiatic Society of Bengal, curator to the Museum of Economic Geology, and President of the Marine Court of Enquiry at Calcutta. His legal work in the Marine Court left an imprint on his scientific work on storms and he developed a genre of writing which he named ‘cyclone memoirs’, publishing 23 of these between 1839 and 1851, along with three important books, most notably, *The Sailor’s Hornbook for the Law of Storms* (1848).

In Piddington’s cyclone memoirs, such as we see in this extract, he sought to develop a way of condensing wind movements into storm-life narratives. For each individual storm, he organized logs, witness depositions about ships wrecked by storms and gales, descriptions of local conditions on the coasts, along with pressure charts and sailing charts, into the narrative compounds which constituted his cyclone memoirs. In these memoirs he collated wind, pressure and tide data, collected at specific times before and after a storm, to follow the minutiae of changes in the atmosphere into a life narrative of storm formations in the Bay of Bengal. The narrativising of his observations on the genesis, form and destructive impact of storms was driven by his intention to find a pattern or law which governed the movements of oceanic winds in the Bay of Bengal. His idea was that such narratives of individual storms, when stitched together through inductive logic, produced practical knowledge about cyclones which could enable the “plainest shipmaster” to prevent shipwrecks but would also constitute scientific law-based knowledge—and he called this new science ‘cyclonology’. He claimed that while the writings of his predecessors (namely Colonels Reid and Redfield) on the whirlwinds in the West Indian Ocean merely offered a theory of storms, his own work was decidedly focused on making an account of the law of storms.

These narratives about the lives of cyclones at the same time as enabling him to form an account of the law of storms allowed him to develop a utilitarian scientific handbook, along with diagrammatic reckoners (called ‘storm cards’) explaining how to avoid and profit from cyclonic winds and thus act as a form of insurance and protection against wreckage. This dual ambition fit both his scientific persona seeking scientific laws, and his legal responsibilities to arbitrate in the Marine Court.

Debjani Bhattacharyya

[See page 186 for reference.]

1839: HENRY PIDDINGTON, Researches on the Gale and Hurricane in the Bay of Bengal on the 3rd, 4th, and 5th of June, 1839; being a First Memoir with reference to the Theory of the Law of Storms in India. Parts I and II *Journal of the Asiatic Society of Bengal* vol. VIII

[EXTRACTS: pp. 559–589 & 631–650]

PART I

[…]

So far, I trust, we shall be able to embody all the information which can be obtained here, and perhaps furnish a valuable supplement to Col. Reid's book; but it is evident that our work will not be complete without the statements to be obtained from the logs of the homeward bound ships from hence;

[…]

To exhibit the foregoing Logs in a collected view, for ready reference, I have arranged all the principal facts in the following series of Tables from the 1st to the 5th June, exhibiting thus at one view the weather experienced by the different ships, and their positions at noon on the same day. No account has been taken of the small difference of apparent noon occasioned by the difference of longitude, as there is nothing which requires this degree of exactness. It will be remarked that throughout the difference between the Easterly and Westerly winds occurs about lat. 19° 30'.

[…]

I have next delineated the whole of the tracks with the winds at noon upon the general Chart, and from these are deduced the centres [of the hurricanes], which last I have marked by a single circle or two for each day, and from the centres I estimate the course of the hurricane. To render the whole more distinct, three diagrams are also given, to half scale, upon which I have a few remarks to make. In considering these diagrams and tables, the reader will be struck with some few anomalies; that is, he will observe that the arrows do not always show the wind as blowing in exact circles, and that in one or two instances, they are altogether different from the others, though not absolutely contradictory. I take these few discrepancies mostly to arise from some one of the following causes:–

I. The carelessness of many in noting the direction of the wind, or the not noting it at the time.

II. Their erroneous estimation of its direction when looking at a weather-cock or dog-vane, and, if a ship is going fast, the not allowing for the effect of her motion upon it.

III. On shore, local circumstances, such as houses, hills, rivers, and the like, which may often produce differences.

IV. At sea the vicinity of the land, ranges of mountains, &c. which when the gale or hurricane strikes them, occasion a re-action altering the direction of the wind.

V. As it has been necessary to fix upon one instant of time at which to compare the wind and weather experienced by different vessels, noon has of course been chosen; but when the winds are varying, it may occur that the one marked about noon is a little more unfavourable to the appearance of the diagram than that which perhaps was the predominant one throughout the day; as, however, it would have appeared like accommodating the facts to the hypothesis, I have preferred allowing them to stand as marked, taking a mean point where the limits of the variation of the wind are expressed, such as SE when the words "between South and East," are used.

VI. The positions of the vessels are rarely accurately ascertained in a severe gale.

Let us consider these causes separately. The careless habits of seamen are well known, and that these should extend to what is apparently the unimportant matter of noting the exact direction of the wind is not surprising, and is well known to every intelligent man, who has commanded a vessel. In severe weather too when a vessel is lying with her yard arms in the water, boats and booms washing away, and sails blowing from the yards, those on whom the responsibility rests have far other matters to engage their attention than the exact direction of the wind; and in many vessels, where perhaps the captain and chief mate are the only persons who can take charge of the deck in such weather, the log is rarely marked till the gale ceases, and it is written up perhaps at a still later period. "You must not look for very great exactness in my log, Sir, for to tell the truth, every word of it was written from memory after the gale was over; myself and the mate had something else besides writing to do while the gale lasted," was literally said to me by one commander; and no doubt this is necessarily true of many, as those who know the severe fatigue of body, and excessive anxiety of mind which the masters of small vessels must undergo in bad weather will readily allow.

[…]

PART II

[…]

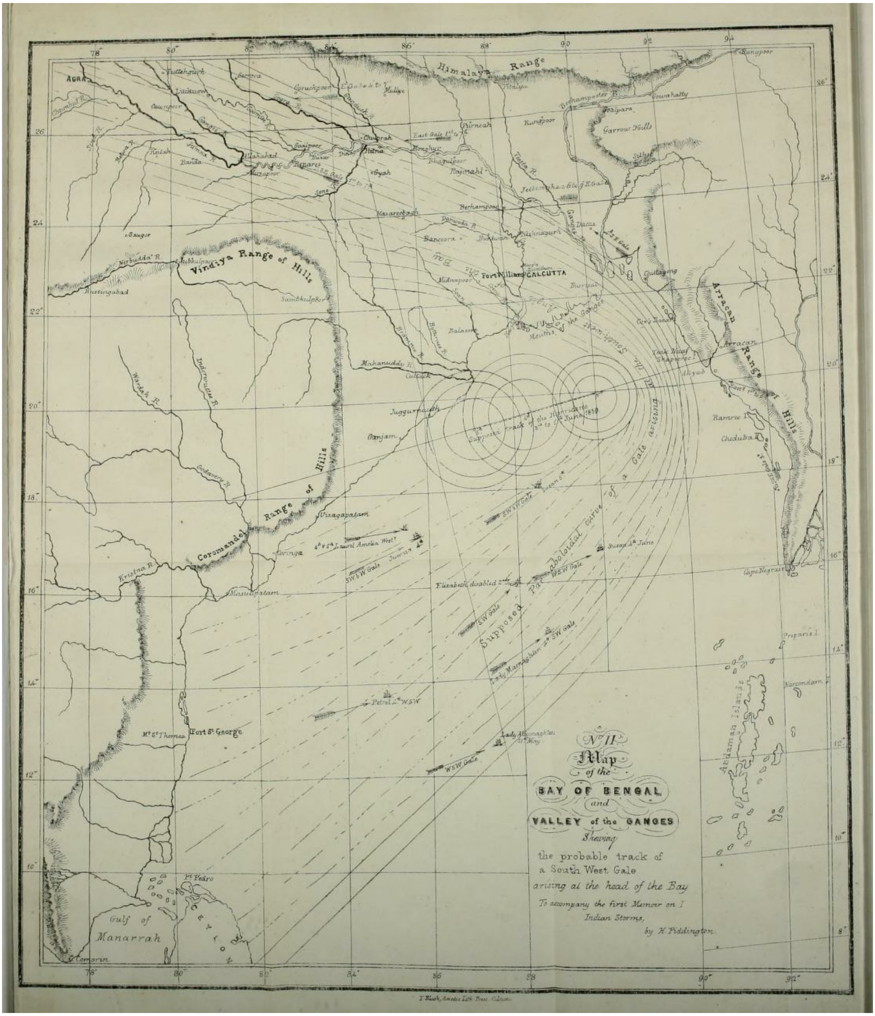
The object […] is to adduce evidence, which shews that it was at the same time both a gale, i.e. a strong wind blowing in with tolerable steadiness from one quarter of the compass; and a hurricane, namely, a violent wind blowing in a circle or vortex of greater or less diameter. At present too it seems probable, from the dates, that the gale produced the hurricane. We may consider that this storm was one of those which usually occur at the change of the monsoon from NE. to SW., which in various parts of the Bay may be said to take place between the 15th May and 15th June. It is from the lst to the 15th June that we look for the rains in Calcutta, though sometimes, as in this year, they may be said to have begun in April. It will be borne in mind then, that whatever follows, whether facts or hypotheses, relates only to the beginning of the SW. monsoon. Future observations will inform us, whether the October Gales as they are called,—though they sometimes occur in November,—are subject to the same or different laws.

[…]

If we look at the Bay of Bengal, Map No. II [opposite], we shall be struck with the fact, that while it is bounded on the East by the mountain range which stretches from the Malay peninsula to Bootan [present day Myanmar], often approaching very near the shores, and rising to the height of from 3000 to perhaps 5000 feet on the Arracan coast; it is also bounded, on the West, by the Coromandel range, which supports the Eastern side of the elevated table lands of the Deccan. At the valley of the Mahanuddee (the river of Cuttack) however, at its junction with the Vindiya range, it turns suddenly to the North-Westward and Westward, leaving thus between it and the mountains of Arracan, the wide opening from Point Palmiras to Chittagong, which, to use an orientalism, is the gate to the plains of Bengal.

The salient angle, formed by the corner where the Vindiya and Coromandel ranges meet, and the entering one, where the Bootan, or Himalaya, and Arracan and Cachar ranges join (leaving however the valley of Assam as an opening for the great Burrumpooter [Brahmaputra River] to flow through,) thus form, as it were, an angular channel; through which all the lower strata of the current of the SW. monsoon may be supposed to find their way over the plains of Bengal and up the valley of the Ganges; and this is their natural course. But we may suppose that the SW. monsoon when urged to any great force at the mouth of the Bay, about Ceylon, must strike against the mountain ranges of Arracan in about from lat. 16°, which is that of Cape Negrais, to lat. 20° or 21°; or about that of Arracan; and, being deflected thence, must turn off in a paraboloidal line towards the great opening offered by the low lands at the head of the Bay, and thence proceed up the valley of the Ganges as before.

But when the head of the Gale is thus deflected, it may meet also with that portion of the monsoon which has blown along the Coromandel range and coast—called the "long-shore wind," by the old navigators—which has a much shorter distance to travel; and there occasion an eddy of variable winds, whirlwind or hurricane, according to the force of the first impulse—and this again influenced too, doubtless, by many causes to which we are yet strangers.



If this theory be true for these tempests, we should look to find points, about the meeting of the two currents, varying in position according to their respective forces, at which, during these gales, it should be comparatively calm, or blowing but moderately; and it is curious that at Balasore, in latitude 21° 28, and at the Black Pagoda in 19° 62' N. this comparative calm is found to have existed.

IX.

In this article, James Johnston (1796–1855), a Scottish chemist and geologist, sketches a providential and cyclical vision of matter typical of the period. For Johnston, circulating elements offer a narrative ‘history’ for the chemist to trace. His article presents a circular narrative, or rather a series of circular narratives that allow scientist and reader to move between the familiar and the unfamiliar, the particular and the universal, the living and the dead.

The extract outlines a carbon cycle. Johnston’s narrative is at its simplest in the schema that appears roughly halfway through the passage; the plant produces that which sustains the animal body, which in turn produces exactly that carbonaceous matter the plant requires to grow (for ‘carbonic acid’ here read ‘carbon dioxide’). This formulation and reformulation of certain compounds (such as starch and water) by the same circulating elements (here carbon, oxygen and hydrogen) presents a narrative of continual change that holds true for all matter. Johnston’s article also details hydrological, nitrogen, and inorganic mineral cycles, all held together and made comparable by this same circulatory narrative.

This narrative is facilitated by other narrative tropes. Johnston begins by describing the “familiar” gas in a pint of beer before moving to the unfamiliar action of carbon in the interior of a plant and the workings of a volcano. As the familiar becomes unfamiliar, so the insignificant and “minute” become of central importance—the bubbles in beer also underwriting the “functions of life”.

While following this narrative trajectory facilitates the communication of Johnston’s science, it also bears on its conception. It allows the chemist to move between scales. As carbon circulates through plant and animal bodies, a portion is also locked within the earth’s crust as these bodies decay after death. As volcanoes erupt, so the earth in turn “breathes” across geologic timescales to renew the atmosphere with carbon. In the narrative movement from bodily to planetary cycles, the particular examples Johnston details allow him to identify universal laws that hold true across the natural world.

These laws are “instructive” of God’s wisdom. Chemist and reader gain a reverence, through them, for the apparently providentially-ordained workings of the material world. The whole “created” carbon here forms an unchanging quantity in “repeated circulation”. Sitting neatly within a providential narrative, whereby everything has its place and purpose, chemistry’s theories are legitimised under established worldviews.

Johnston’s chemical economy also sustains, and is itself sustained by, narratives from political economy. With an exemplary Protestant work ethic, carbon never rests—it is always “ready to begin anew the same endless revolution”. While carbon cycles through soil, plant, animal and air, “never really the property of any”, it nevertheless has its role in nature. This is no utopian dream, but a conservative vision of matter, like the worker, knowing and never questioning its place.

Johnston’s chemistry is conceptualised through established narrative tropes. Forming a circulatory narrative of matter transformation, it in turn offers other narratives that may themselves be put to various “instructive” purposes.

Jim Scown

1853: JAMES F. W. JOHNSTON, The Circulation of Matter *Blackwood's Edinburgh Magazine* 73 pp. 550–560

[EXTRACT: pp. 551–553]

[…]

A second and more special form of the circulation of matter – one more exclusively connected with the necessities and functions of life – is presented in the history of the carbonic acid of the atmosphere. This gas is familiar to every one as that sparkling air which, rising in countless bubbles, gives life to the creaming tankard, to the tempting champagne, and to the more innocent soda-water. Of this gas there exists in the atmosphere only a minute proportion, but its presence is essential to the constitution of our air, and is necessary to the very existence of vegetable life. At the level of the sea, and at moderate elevations, this proportion averages about one gallon in every two thousand five hundred of air. When we ascend to eight or ten thousand feet, the quantity becomes doubled. Upon this minute proportion, as we have said, all vegetable existence depends.

This dependence appears more remarkable, however, the more precise our ideas are of the absolute quantity of this substance which the entire air contains. The whole weight of the carbonic acid of the atmosphere which presses on a square inch of the earth's surface, weighs less that seventy grains, and the carbon it contains less than twenty grains; and it has been calculated that, were the whole surface of the earth dry land and under cultivation, crops such as we ordinarily raise would extract and fix the whole of this carbon in the form of vegetable matter in the short space of fourteen years. But this is prevented by the constant restoration of carbonic acid to the air, and chiefly through the repeated circulation of the same quantity of matter.

The trees of the forest yearly shed their leaves, and these leaves, through the influence of the weather, decay and disappear, restoring to the atmosphere, in the form of carbonic acid, a portion of the same carbon which living leaves had previously extracted from it, during the period of growth. The yearly ripening herbage, and every plant that naturally withers on plane and hill – the grass of the burning prairie, and the timber of inflamed forests, with all that man consumes for fuel and other uses; – every form of vegetable matter, in short, returns more or less quickly to the state of carbonic acid, and disappears in the invisible air. Thus, what is yearly removed by living plants, is so far restored again by those which naturally perish, or are destroyed by the intervention of man.

But man himself, and other animals, assist in the same conversion. They consume vegetable food with the same final result as when it perishes by actual decay, or is destroyed by the agency of fire. It is conveyed into the stomach in the form in which the plant yields it; it is breathed out again, from the lungs and the skin, in the form of carbonic acid and water. We can follow out this operation, more closely, and it will be both interesting and instructive to do so.

The leaf of the living plant sucks in carbonic acid from the air, and gives off the oxygen contained in this gas. It retains only the carbon. The roots drink in water from the soil, and out of this carbon and water the plant forms starch, sugar, fat, and other substances. The animal introduces this starch, sugar, or fat into its stomach, and draws in oxygen from the atmosphere by its lungs; and with these materials it undoes the previous labours of the living plant, delivering back again, from the lungs and the skin, both the starch and the oxygen in the form of the carbonic acid and water. The process is clearly represented in the following scheme:–

	Takes in	Produces
THE PLANT,	*Carbonic acid* by its leaves; *Water* by its roots.	*Oxygen* from its leaves; *Starch, &c.,* in its solid substance.
THE ANIMAL,	*Starch and fat* into the stomach; *Oxygen* into the lungs.	*Carbonic acid and water* from the skin and the lungs.

The circle begins with carbonic acid and water, and ends with the same substances. The same material – the same carbon, for example – circulates over and over again, now floating in the invisible air, now forming the substance of the growing plant, now of the moving animal, and now again dissolving into the air, ready to begin anew the same endless revolution. It forms part of a vegetable to-day—it may be built into the body of a man tomorrow; and, a week hence, it may have passed into another animal. What is mine this week is yours the next. There is, in truth, no private property in ever-moving matter.

Yet all the carbonic acid which is removed from the air by the agency of plants is not immediately restored by the circulation above described—a larger wheel revolves to make up the deficiency.

It has been shown that when plants die and decay, are burned in air, or are eaten by animals, the carbon they contain is delivered back again to the atmosphere in the form of carbonic acid; but all the plants produced yearly over the whole earth are not so resolved into gaseous substances in any given time. In all parts of the world some portions of vegetable matter escape for a time this total destruction and are buried beneath the surface of the earth, to be preserved in the solid form for an indefinite period. With such comparatively indestructible forms of vegetable matter we are familiar in the peat-bogs of Scotland and Ireland, sometimes from 50 to 100 feet deep, and in the submarine forests which are seen on so many parts of our insular coasts. What is thus collected and gradually buried would necessarily cause a constant diminution in the small quantity of carbonic acid contained in the air, were there no natural means in operation for making up the deficiency.

Nor is this all. Within the bosom of the great seas tiny insects are at work, upon which nature has imposed, in addition to the quest for food and the care of their offspring, the perpetual labour of building new houses. For defence as well as for shelter, the shell-fish toils continually, repairing, enlarging, and renewing his own dwelling-place; and, dying at last, he leaves it as a contribution to the growing thickness of shelly limestone. For thousands of miles, in more southern seas, still humbler insects erect their massive coral walls, which, now skirting long coast-lines, and now encircling solitary islands, bid defiance to the angriest waters; and as they die, generation after generation, they leave, in rocky beds of coralline limestone, an imperishable memorial of their exhaustless labours. These rocks contain, chained down in a seemingly everlasting imprisonment, two-fifths of their weight of carbonic acid. This has been derived either directly or indirectly from the atmosphere, and thus the sea must ever be drinking in carbonic acid from the air. The labours of marine animals, therefore, like the burying of vegetable matter, should cause a yearly diminution of the absolute quantity of this gas contained in the atmosphere, were no other natural operation to compensate for the constant removal.

But the earth herself breathes for this purpose. From cracks and fissures in the crust, which occur in vast numbers over the surface of the earth, carbonic acid gas issues in large quantities, and daily mingles itself with the ambient air. It sparkles in the springs of Carlsbad, rushes as from subterranean bellows on the table-land of Paderborn; chinks in the pockets of the Prince of Nassau; astonishes innocent travellers in the Grotto del Cane; interests the chemical geologist in the cave of Pyrmont; and is terrible to man and beast in the fatal "Valley of Death," the most wonderful of the wonders of Java. And besides, it doubtless issues still more abundantly from the unknown bottom of the expanded waters which occupy so large a proportion of the surface of the globe. From these many sources, continually flowing into the air or rising into the sea, carbonic acid is daily supplied in place of that which is daily withdrawn, to be buried in the solid crust. Did we know after what lapse of time the earth would again breathe out what is thus daily entombed, we should be able to express in words how long this slowly resolving secular wheel requires fully to perform one of its immense gyrations.

Thus, like the watery vapour of the atmosphere, its carbonic acid also is continually circulating. While that which floats in the air, during one generation, circles many times, it may be, from the atmosphere to the plant, from the plant to the animal, and from the animal to the air again—never really the property of any, and never lingering long in one stay—the whole created carbon is slowly moving in a greater circle between earth and air. It rises from the earth at one end of the curve in the state of an elastic gas, it amuses itself by the way in assuming for brief intervals many successive varieties of plant-form and animal-form, till it is finally buried in the earth again, at the other end of the curve, in the state of solid limestone and fossil plants.

X.

In this 1857 communication to the Society of Biology, Claude Bernard (1813–78) presented one of his most significant findings in the field of physiology, in a career which would see him embody the scientific spirit through his later (1865) *Introduction* to experimental methods in medicine. Here, the focus is Bernard's research on the production of sugars by the liver, namely how he isolated and characterised the substance responsible for the formation of sugar—now called glycogen.

His communication thus forms part of a larger narrative, as is clear from the title, which promises "new research" rather than announcing directly Bernard's isolation of the "glycogenic substance". But Bernard does not frame his efforts as an epic quest. The narrative begins rather with his personal research journey, as indicated by frequent first-person pronouns, and use of the perfect past tense. This implies that actions in the past have some connection to the present, unlike tenses used for distant historical narratives. Having concluded that some substance in the liver could produce sugar through various (chemical) transformations, Bernard's task was to isolate this substance. His account emphasises the 'careful' nature of repeated experimentation in the face of frequent discouragement.

As Bernard begins to present his new experiments to the Society, the narrative of his personal research efforts gives way to description and argument about the physiological properties of the newly isolated substance. At this point, the informed reader could reconstruct Bernard's position in the scholarly debates surrounding liver function. This remains latent in his paper however, compared to the main research narrative which begins: "Here follow the methods which allowed me to extract this substance".

This narrative portrays an active experimenter, though some processes appear in a passive form. Since the actions take the present tense, we can deduce that this is not the account of one singular experiment, but rather an exemplary narrative detailing the key steps needed to isolate the glycogenic substance from a dog's liver. Curiously, as the experiment proceeds, the pronoun used to designate the experimenter shifts abruptly from "je" ("I") to "on". This shift opens up the identity of the experimenting subject from the singular to a general "you"—anyone who performs this experiment—and thereby reveals this research narrative for what it is: a convention, in which the experimenter-character acts out what is primarily an experimental protocol, not the story of any one experiment he actually performed.

In a final narrative shift, Bernard moves from describing how he obtained the substance to giving a dense, impersonal characterisation of its physical and chemical properties. Complex forms of synthetic narrative reasoning lie behind this list-like description, notably branching comparisons, which allow Bernard to begin to classify the substance. Despite his careful theorisation of the experimental method, Bernard had surprisingly little to say about the function of language in reporting scientific results. Here, the narrative contrast between process and characteristics seems to indicate that his priority is to engage his audience in following (and thus adopting) his method for isolating the substance. On a narrative level, research processes—both personal and exemplary—are clearly delineated from "facts" about glycogen.

Kim Hajek

1857: CLAUDE BERNARD, Nouvelles recherches expérimentales sur les phénomènes glycogéniques du foie. *Comptes rendus des séances et Mémoires de la Société de biologie* IV (année 1857) *Mémoires lus à la Société de biologie pendant l'année 1857 (1858)*

[EXTRACT: pp. 3–5]

[...]

A mon avis, rien de moins acceptable que cette idée souvent formulée et généralement admise, que l'on peut établir une équation dans l'un des membres de laquelle on placerait tous les éléments du sang artériel qui arrive à la glande, et dans l'autre membre, le sang qui sort et le liquide sécrété. Guidé par quelques données expérimentales, j'ai été porté à admettre la préexistence d'une matière propre au foie, qui subit une transformation et produit le sucre.

Une des expériences principales qui m'ont conduit à cette notion est celle que j'ai faite depuis longtemps, consistant à laver un foie avec soin et à lui enlever tout le sang et tout le sucre qu'il contient, et à l'exposer ensuite à une température tiède. Le sucre dont on ne trouvait plus de traces apparaît d'une manière manifeste.

Je me suis appliqué à rechercher et à isoler la matière soupçonnée comme préexistant au sucre, et après de nombreux tâtonnements, après avoir plusieurs fois abandonné cette recherche, je suis arrivé, dans des essais tout récents, à l'isoler et à déterminer ses caractères essentiels.

Les expériences dont on va connaître les résultats ont été faites sur des chiens nourris exclusivement avec de la viande.

La matière que je présente isolée est sécrétée par le foie; aucun autre organe ne la possède. Comme l'amidon, qui existe dans la graine et qui subit, sous l'influence de certaines conditions physiques et chimiques, sa transformation en sucre, cette matière, contenue dans le foie, se transforme en sucre, même après la mort, sous l'influence des mêmes conditions physiques et chimiques.

Dans la fonction glycogénique du foie, il faut nécessairement reconnaître deux ordres de phénomènes:

1° La création de cette matière, acte vital, dont l'origine essentielle est encore inconnue.

2° La transformation de cette matière en sucre, phénomène purement chimique.

J'admets donc que le foie ne sécrète pas le sucre, mais se nourrit du sang, et crée, par une évolution organique, cette matière spéciale, qui subit, sous l'influence du ferment que le sang lui fournit, sa transformation en sucre.

Voici les procédés qui m'ont servi à extraire cette matière:

L'animal étant tué par la section du bulbe rachidien, le foie est pris, coupé en tranches et jeté dans l'eau bouillante. Je ferai remarquer que si l'on jette le foie dans une eau froide, qu'on porte ensuite à l'ébullition, il se forme du sucre pendant cette élévation graduelle de température.

Au contraire, le foie étant plongé dans l'eau bouillante, le ferment est coagulé, et la matière reste dans l'eau en se dissolvant. J'exprime alors la masse, et la liqueur est additionnée de trois ou quatre fois son volume d'alcool à 40°. Je la recueille sur un filtre, la redissous dans l'eau, et la fais bouillir pendant une demiheure avec une solution concentrée de potasse; on la précipite de nouveau par l'alcool, et on la traite ensuite par l'acide acétique, pour transformer en acétate soluble le carbonate de potasse entraîné; on précipite une dernière fois par l'alcool, et on lave avec l'alcool au même titre.

Cette matière ainsi obtenue et desséchée est blanche, amorphe, insipide, soluble dans l'eau, à laquelle elle donne une teinte opaline; bouillie avec une solution de potasse, elle ne donne pas d'ammoniaque; calcinée avec la chaux sodée, elle ne donne pas non plus d'ammoniaque, caractères qui la rangent parmi les substances non azotées. Soumise à une température capable de la torréfier légèrement, elle produit de la dextrine et une petite quantité de sucre. Une solution de cette substance transformée en dextrine par l'action limitée de l'acide sulfurique, dévie à droite le plan de polarisation. Mise en contact avec l'eau iodée, elle prend une teinte violacée, analogue à celle que donne l'amidon qui se transforme en dextrine. Elle est précipitable par l'alcool. Elle ne réduit point le tartrate de cuivre et de potasse; elle se transforme en sucre sous l'influence des acides minéraux, mais avec lenteur: au contraire, elle subit rapidement cette transformation vers la température de 40°, sous l'influence de ferments tels que la salive, le tissu pancréatique, la diastase, et surtout sous l'influence du sang, lequel transforme aussi l'amidon végétal en sucre.

[TRANSLATION]

1857: CLAUDE BERNARD, New experimental research on glycogenic phenomena of the liver. *Minutes of the meetings and Memoirs of the Biological Society IV* (year 1857), *Memoirs read at the Biological Society during the year 1857 (1858)*

[...]

In my opinion, nothing is less acceptable than this frequently expressed and generally accepted idea, that it is possible to set up an equation on one side of which would be placed all the components of the arterial blood which arrive at the gland, and on the other side, the blood which leaves and the liquid secreted. Guided by some experimental data, I was brought to acknowledge the pre-existence of a substance specific to the liver, which undergoes a transformation and produces the sugar.

One of the principal experiments which led me to this notion is one I have practiced for a long time, consisting of carefully washing a liver, removing from it all the blood and sugar contained within, and then exposing it to a warm temperature. The sugar, of which there had been no trace, appeared quite manifestly.

I applied myself to seeking out and isolating the substance suspected of pre-existing sugar and, after a great deal of trial and error, after having abandoned the research numerous times, I managed, in quite recent tests, to isolate it and determine its essential characteristics.

The experiments of which we [*on*] will see the results were performed on dogs fed exclusively on meat.

The substance that I present here as an isolate is secreted by the liver; no other organ possesses it. Like starch, which exists in seeds and undergoes, under the influence of certain physical and chemical conditions, a transformation into sugar, this substance, contained in the liver, transforms into sugar, even after death, under the influence of the same physical and chemical conditions.

In the glycogenic function of the liver, it is necessary to recognize two orders of phenomena:

1. The creation of this substance, a vital act, of which the essential origin is still unknown.

2. The transformation of this substance into sugar, a purely chemical phenomenon.

I recognise therefore that the liver does not secrete sugar, but is nourished by the blood, and creates, by organic evolution, this specific substance which undergoes, under the influence of ferment provided by the blood, its transformation into sugar.

Here follow the methods which allowed me to extract this substance:

The animal, having been killed by incision of the rachidian bulb, the liver is removed, sliced and immersed in boiling water. I will point out that if the liver is placed in cold water, which is then brought to the boil, sugar is formed during this gradual increase in temperature.

On the contrary, on the liver being placed into boiling water, the ferment is coagulated and the substance remains dissolved in the water. I calculate the mass, and to the solution is added three or four times its volume of alcohol at 40 degrees. I collect it on a filter, redissolve it in water, and boil it for half an hour with a concentrated solution of potash; you [*on*] precipitate it again with alcohol, and next you treat it with acetic acid, in order to transform the potassium carbonate obtained into soluble acetate; you precipitate with alcohol one last time, and wash with alcohol in the same manner.

This substance thus obtained and dried is white, amorphous, insipid, soluble in water, to which it gives an opaline tint; boiled in a solution of potash, it does not produce ammonia; nor does it produce ammonia when carbonised with soda-lime, characteristics which place it among the non-nitrogenous substances. Exposed to a temperature capable of roasting it gently, it produces dextrin and a small amount of sugar. A solution of this substance, transformed into dextrin by the limited action of sulphuric acid, rotates the plane of polarisation to the right. Placed in contact with iodised water, it takes on a purplish-blue hue, analogous to that given by starch converted into dextrin. It is precipitable by alcohol. It does not reduce copper or potassium tartrates; it is converted into sugar under the influence of mineral acids, but slowly: contrarily, it undergoes this transformation rapidly around temperatures of 40 degrees under the influence of ferments such as saliva, pancreatic tissue, diastase, and particularly under the influence of blood, which also converts vegetable starch into sugar.

[Translation by Beatrice Fagan & Kim Hajek]

XI.

1859 marked the publication of Charles Darwin's *On the Origin of Species*. Darwin's other books were profusely illustrated, but the *Origin* featured only a single visual, an elaborate evolutionary tree diagram. Darwin (1809-1882) accompanied his diagram with eleven pages of textual exegesis, but the diagram takes centre stage. He returned to it several times over the course of the *Origin*, using it to illuminate not only the general process of evolution by natural selection, but also how that process could explain patterns and laws that naturalists had found to characterize the living world. He used the diagram to explain why taxonomists always classified organisms into groups within groups, in the same way that moons cluster around their planets, and planets around their stars. He used the diagram to explain the "law of geological succession," which held that fossil remains of extinct animals tend to be intermediate in character between living animals.

Evolutionary biology is often described as a "historical science," in which the laws and predictions that are supposed to characterize the physical sciences are set aside in favour of narratives of how particular lineages of organisms evolved, such narratives being uniquely adapted to evolutionary explanations. But Darwin's diagram gives the lie to such simplistic formulations (Priest, 2018).

The diagram has a narrative structure, but it does not represent how any particular lineage of organisms is supposed to have evolved. It is abstract, schematic, conjectural. It is a visual representation of the process by which evolution by natural selection tends to occur, and of the patterns and lawful regularities that are the natural outgrowths of that process. Moreover, Darwin's diagram does not reveal its meaning directly to a passive interpreter. Darwin demands that the user engage with the diagram dynamically to understand how it explains a particular pattern or law. He then demands that the reader interpret elements of the diagram differently, and then reengage with it to understand how it explains a second pattern or law.

Darwin asks us to follow the process by which a single species gives rise to two or more varieties by inviting us to interpret the space between two horizontal lines as representing a thousand generations and the distance between the superscripted letters on a given line as representing organisms having a relatively small, but still meaningful difference in structure and behaviour. He then asks us to reimagine the space between two horizontal lines as representing a million, or a hundred million, generations and the space between superscripted letters as representing a much larger difference in structure and behaviour. He asks us to extend in our imaginations the dotted lines at the bottom of the diagram down beyond the boundaries of the page. And so we can see how a single species could have given rise to many genera, families, orders, and higher taxa. He asks us to reimagine the horizontal lines as strata of the earth's crust and the superscripted letters as fossil remains of extinct species so that we may understand the law of geological succession.

Darwin's diagram is narrative, but it is not *a* narrative. It is a scaffold on which he, and we, together create narratives that enable us to understand evolutionary processes.

Greg Priest

[See page 186 for reference.]

1859: CHARLES DARWIN, *On the Origin of Species by Means of Natural Selection, or The Preservation of Favoured Races in the Struggle for Life* (London: John Murray)

[EXTRACT: pp. 116–125]

The accompanying diagram will aid us in understanding this rather perplexing subject. Let A to L represent the species of a genus large in its own country; these species are supposed to resemble each other in unequal degrees, as is so generally the case in nature, and as is represented in the diagram by the letters standing at unequal distances. […]

Let (A) be a common, widely-diffused, and varying species, belonging to a genus large in its own country. The little fan of diverging dotted lines of unequal lengths proceeding from (A), may represent its varying offspring. The variations are supposed to be extremely slight, but of the most diversified nature; they are not supposed all to appear simultaneously, but often after long intervals of time; nor are they all supposed to endure for equal periods. Only those variations which are in some way profitable will be preserved or naturally selected. […] When a dotted line reaches one of the horizontal lines, and is there marked by a small numbered letter, a sufficient amount of variation is supposed to have been accumulated to have formed a fairly well-marked variety, such as would be thought worthy of record in a systematic work.

The intervals between the horizontal lines in the diagram, may represent each a thousand generations; but it would have been better if each had represented ten thousand generations. After a thousand generations, species (A) is supposed to have produced two fairly well-marked varieties, namely a^1 and m^1…. If, then, these two varieties be variable, the most divergent of their variations will generally be preserved during the next thousand generations. And after this interval, variety a^1 is supposed in the diagram to have produced variety a^2, which will, owing to the principle of divergence, differ more from (A) than did variety a^1. Variety

m^1 is supposed to have produced two varieties, namely m^2 and s^2, differing from each other, and more considerably from their common parent (A). We may continue the process by similar steps for any length of time; some of the varieties, after each thousand generations, producing only a single variety, but in a more and more modified condition, some producing two or three varieties, and some failing to produce any. Thus the varieties or modified descendants, proceeding from the common parent (A), will generally go on increasing in number and diverging in character. In the diagram the process is represented up to the ten-thousandth generation, and under a condensed and simplified form up to the fourteen-thousandth generation.

But I must here remark that I do not suppose that the process ever goes on so regularly as is represented in the diagram, though in itself made somewhat irregular. I am far from thinking that the most divergent varieties will invariably prevail and multiply: a medium form may often long endure, and may or may not produce more than one modified descendant; for natural selection will always act according to the nature of the places which are either unoccupied or not perfectly occupied by other beings; and this will depend on infinitely complex relations. But as a general rule, the more diversified in structure the descendants from any one species can be rendered, the more places they will be enabled to seize on, and the more their modified progeny will be increased. In our diagram the line of succession is broken at regular intervals by small numbered letters marking the successive forms which have become sufficiently distinct to be recorded as varieties. But these breaks are imaginary, and might have been inserted anywhere, after intervals long enough to have allowed the accumulation of a considerable amount of divergent variation.

[...]

After ten thousand generations, species (A) is supposed to have produced three forms, a^{10}, f^{10}, and m^{10}, which, from having diverged in character during the successive generations, will have come to differ largely, but perhaps unequally, from each other and from their common parent. If we suppose the amount of change between each horizontal line in our diagram to be excessively small, these three forms may still be only well-marked varieties; or they may have arrived at the doubtful category of subspecies; but we have only to suppose the steps in the process of modification to be more numerous or greater in amount, to convert these three forms into well-defined species: thus the diagram illustrates the steps by which the small differences distinguishing varieties are increased into the larger differences distinguishing species. By continuing the same process for a greater number of generations (as shown in the diagram in a condensed and simplified manner), we get eight species, marked by the letters between a^{14} and m^{14}, all descended from (A). Thus, as I believe, species are multiplied and genera are formed.

[…]

But during the process of modification, represented in the diagram, another of our principles, namely that of extinction, will have played an important part. As in each fully stocked country natural selection necessarily acts by the selected form having some advantage in the struggle for life over other forms, there will be a constant tendency in the improved descendants of any one species to supplant and exterminate in each stage of descent their predecessors and their original parent. For it should be remembered that the competition will generally be most severe between those forms which are most nearly related to each other in habits, constitution, and structure. Hence all the intermediate forms between the earlier and later states, that is between the less and more improved state of a species, as well as the original parent-species itself, will generally tend to become extinct. So it probably will be with many whole collateral lines of descent, which will be conquered by later and improved lines of descent […].

If then our diagram be assumed to represent a considerable amount of modification, species (A) and all the earlier varieties will have become extinct, having been replaced by eight new species (a^{14} to m^{14}); and (I) will have been replaced by six (n^{14} to z^{14}) new species.

[…]

The new species in our diagram descended from the original eleven species, will now be fifteen in number. Owing to the divergent tendency of natural selection, the extreme amount of difference in character between species a^{14} and z^{14} will be much greater than that between the most different of the original eleven species. The new species, moreover, will be allied to each other in a widely different manner. Of the eight descendants from (A) the three marked a^{14}, q^{14}, p^{14}, will be nearly related from having recently branched off from a^{10}, b^{14} and f^{14}, from having diverged at an earlier period from a^{5}, will be in some degree distinct from the three first-named species; and lastly, o^{14}, e^{14}, and m^{14}, will be nearly related one to the other, but from having diverged at the first commencement of the process of modification, will be widely different from the other five species, and may constitute a subgenus or even a distinct genus.

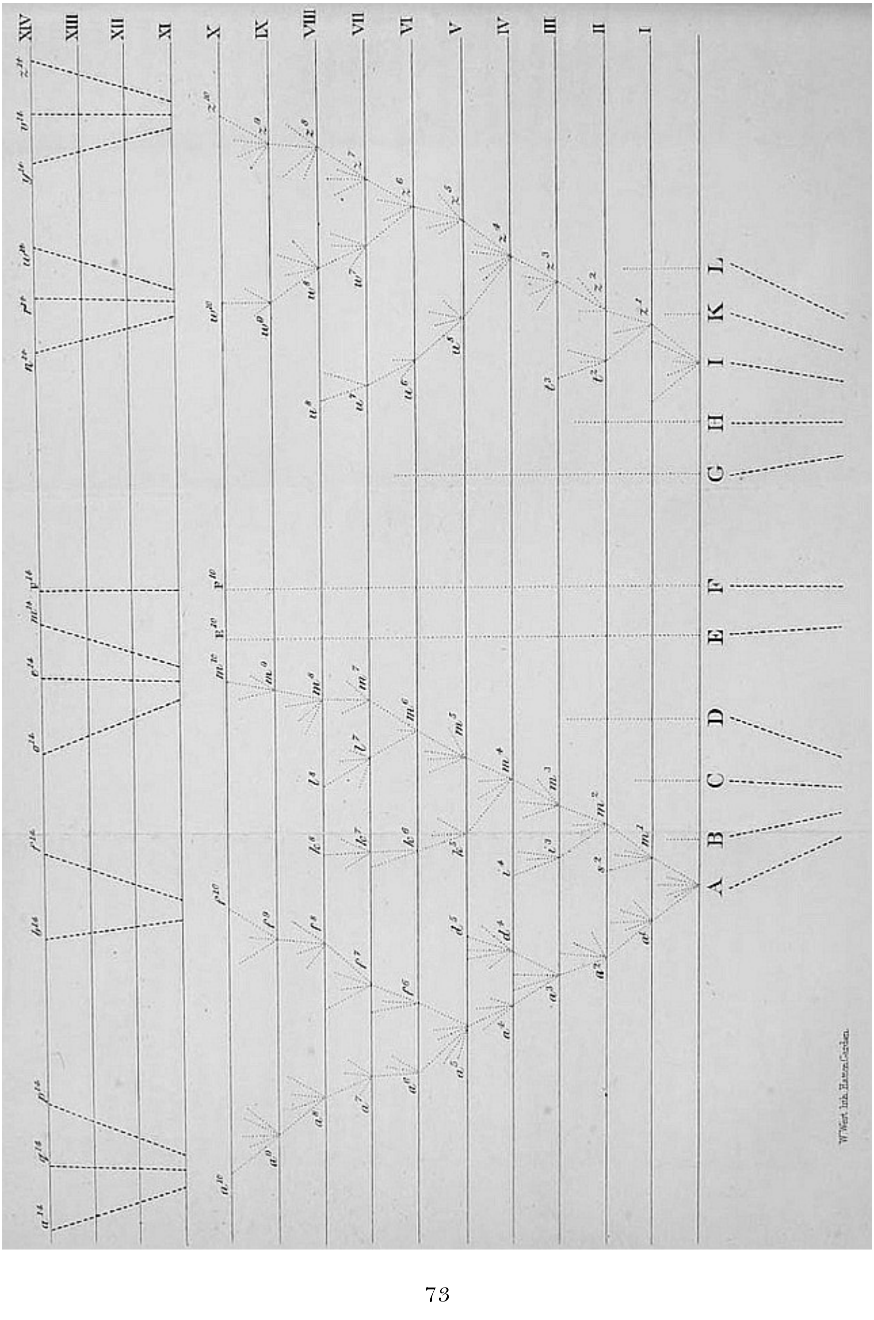
[…]

Thus it is, as I believe, that two or more genera are produced by descent, with modification, from two or more species of the same genus. And the two or more parent-species are supposed to have descended from some one species of an earlier genus. In our diagram, this is indicated by the broken lines, beneath the capital letters, converging in sub-branches downwards towards a single point; this point representing a single species, the supposed single parent of our several new subgenera and genera.

[…]

In the diagram, each horizontal line has hitherto been supposed to represent a thousand generations, but each may represent a million or hundred million generations […].

I see no reason to limit the process of modification, as now explained, to the formation of genera alone. If, in our diagram, we suppose the amount of change represented by each successive group of diverging dotted lines to be very great, the forms marked a^{14} to p^{14}, those marked b^{14} and f^{14}, and those marked o^{14} to m^{14}, will form three very distinct genera. We shall also have two very distinct genera descended from (I); and as these latter two genera, both from continued divergence of character and from inheritance from a different parent, will differ widely from the three genera descended from (A), the two little groups of genera will form two distinct families, or even orders, according to the amount of divergent modification supposed to be represented in the diagram. And the two new families, or orders, will have descended from two species of the original genus; and these two species are supposed to have descended from one species of a still more ancient and unknown genus.



XII.

The idea that humans might have evolved from a primordial dual-sexed being has a long history. Notions of hermaphrodite origins were used to provide a narrative framework for understanding and discussing sex variations including intersexualities, transformations of sex, and non-reproductive sexual behaviours by Charles Darwin (1809–1882), as he sought to accommodate puzzling sex-related phenomena within his developing evolutionary schema.

From the earliest stage of his evolutionary theorising, Darwin's acceptance of the concept of hermaphroditism helped him to frame sex variations in evolutionary terms and, most importantly, to explain the inheritance of sex-related characteristics. In his unpublished notebooks, penned between 1836 and 1844, Darwin grappled to situate a variety of sex variations within his developing evolutionism, including the rearing of a queen bee from the pupae of a worker, the assumption of male-typical plumage by female birds, and the occurrence of homologous sexual anatomical structures. 'Every animal surely is hermaphrodite' he wrote. However, Darwin cautiously refrained from making such explicit statements in his published works. Nonetheless, the perennial co-existence of female and male characteristics in every individual remained an important facet of his evolutionism, though Darwin insisted that characteristics of the opposite sex usually remained merely in a latent state. Only in exceptional circumstances he argued, were they expressed.

Examples of avian sex transformations formed the mainstay of Darwin's discussion of 'latent characters' in the second volume of *The Variation of Animals and Plants*. Only after establishing the anomalous nature of such occurrences however, did he outline a narrative of heredity whereby individuals passed on sex-related characters of the opposite sex to both male and female offspring without them normally developing those characters themselves.

Darwin extended his analysis in The *Descent of Man* by embracing a grand narrative of the evolution of sex, in which he suggested that hermaphroditism was the primordial condition of humanity's remotest ancestor. Such a deep and largely unspecified evolutionary narrative, implying an infinitely longer timespan than humans took to evolve from lower primates, was open to numerous interpretations, potentially raising questions of morality and respectability which Darwin was undoubtedly keen to avoid. Furthermore, Darwin sought to downplay the possibility that the suggestion of dual-sexed origins might be seen to imply a 'true' hermaphroditism in humans of the kind found in some lower animals, thereby avoiding awkward challenges to Victorian norms of gender and sexuality. He therefore guided his readers through a 'singular difficulty' which was raised by the proposition of hermaphrodite ancestry. Any notion that a hermaphroditic state had continued for so long that early mammals might be considered to have been true hermaphrodites was not, he argued, viable. Instead Darwin reiterated his commitment to a narrative in which sex-related characteristics acquired by one sex were transmitted in an imperfect state to the other, suggesting that this latent form of hermaphroditism was a very early evolutionary adaptation.

Ross Brooks

1868: CHARLES DARWIN, *The Variation of Animals and Plants Under Domestication* (London: John Murray)

[EXTRACT: pp. 51–52]

Latent Characters. — But I must explain what is meant by characters lying latent. The most obvious illustration is afforded by secondary sexual characters. In every female all the secondary male characters, and in every male all the secondary female characters, apparently exist in a latent state, ready to be evolved under certain conditions. It is well known that a large number of female birds, such as fowls, various pheasants, partridges, peahens, ducks, &c., when old or diseased, or when operated on, partly assume the secondary male characters of their species. In the case of the hen-pheasant this has been observed to occur far more frequently during certain seasons than during others. A duck ten years old has been known to assume both the perfect winter and summer plumage of the drake. Waterton a gives a curious case of a hen which had ceased laying, and had assumed the plumage, voice, spurs, and warlike disposition of the cock; when opposed to an enemy she would erect her hackles and show fight. Thus every character, even to the instinct and manner of fighting, must have lain dormant in this hen as long as her ovaria continued to act. The females of two kinds of deer, when old, have been known to acquire horns; and, as Hunter has remarked, we see something of an analogous nature in the human species. On the other hand, with male animals, it is notorious that the secondary sexual characters are more or less completely lost when they are subjected to castration. Thus, if the operation be performed on a young cock, he never, as Yarrell states, crows again; the comb, wattles, and spurs do not grow to their full size, and the hackles assume an intermediate appearance between true hackles and the feathers of the hen. Cases are recorded of confinement alone causing analogous results. But characters properly confined to the female are likewise acquired; the capon takes to sitting on eggs, and will bring up chickens; and what is more curious, the utterly sterile male hybrids from the pheasant and the fowl act in the same manner, "their delight being to watch when the hens leave their nests, and to take on themselves the office of a sitter." That admirable observer Reaumur asserts that a cock, by being long confined in solitude and darkness, can be taught to take charge of young chickens; he then utters a peculiar cry, and retains during his whole life this newly acquired maternal instinct. The many well-ascertained cases of various male mammals giving milk show that their rudimentary mammary glands retain this capacity in a latent condition. We thus see that in many, probably in all cases, the secondary characters of each sex lie dormant or latent in the opposite sex, ready to be evolved under peculiar circumstances. We can thus understand how, for instance, it is possible for a good milking cow to transmit her good qualities through her male offspring to future generations; for we may confidently believe that these qualities are present, though latent, in the males of each generation. So it is with the game-cock, who can transmit his superiority in courage and vigour through his female to his male offspring; and with man it is known that diseases, such as hydrocele, necessarily confined to the male sex, can be transmitted through the female to the grandson. Such cases as these offer, as was remarked at the commencement of this chapter, the simplest possible examples of reversion; and they are intelligible on the belief that characters common to the grandparent and grandchild of the same sex are present, though latent, in the intermediate parent of the opposite sex.

1871: CHARLES DARWIN, *The Descent of Man* (London: John Murray)

[EXTRACT: pp. 199–200]

There is one other point deserving a fuller notice. It has long been known that in the vertebrate kingdom one sex bears rudiments of various accessory parts, appertaining to the reproductive system, which properly belong to the opposite sex; and it has now been ascertained that at a very early embryonic period both sexes possess true male and female glands. Hence some extremely remote progenitor of the whole vertebrate kingdom appears to have been hermaphrodite or androgynous. But here we encounter a singular difficulty. In the mammalian class the males possess in their vesiculae prostatica rudiments of a uterus with the adjacent passage; they bear also rudiments of mammae, and some male marsupials have rudiments of a marsupial sack. Other analogous facts could be added. Are we, then, to suppose that some extremely ancient mammal possessed organs proper to both sexes, that is, continued androgynous after it had acquired the chief distinctions of its proper class, and therefore after it had diverged from the lower classes of the vertebrate kingdom? This seems improbable in the highest degree; for, had this been the case, we might have expected that some few members of the two lower classes, namely fishes and amphibians, would still have remained androgynous. We must, on the contrary, believe that when the five vertebrate classes diverged from their common progenitor the sexes had already become separated. To account, however, for male mammals possessing rudiments of the accessory female organs, and for female mammals possessing rudiments of the masculine organs, we need not suppose that their early progenitors were still androgynous after they had assumed their chief mammalian characters. It is quite possible that, as the one sex gradually acquired the accessory organs proper to it; some of the successive steps or modifications were transmitted to the opposite sex. When we treat of sexual selection, we shall meet with innumerable instances of this form of transmission—as in the case of the spurs, plumes, and brilliant colours, acquired by male birds for battle or ornament, and transferred to the females in an imperfect or rudimentary condition.

XIII.

George J. Romanes (1848–1894), a Canadian-Scottish physiologist, and close associate of Darwin, attempted to establish a comparative psychology, an "accurate classification" of the "phenomena of mind", analogous to the treatment of the "phenomena of structure" in comparative anatomy (1882, p. vi). His book *Animal Intelligence* (1882) was concerned with the "facts", while the later *Mental Evolution in Animals* (1884) dealt with the "principles" of the genesis of the faculties of the mind.

While the minds of fellow humans and animals could not be observed, Romanes held that the activities of organisms served as "ambassadors" from which an observer could infer the mental states by analogy, on the basis of knowledge about her own mental states and their association with activities, accessible through introspection. With respect to animals, this procedure implied a form of anthropomorphism, partly justified by assumed psychological continuity due to common descent, partly accepted as the only available path to knowledge of the animal mind.

Romanes' facts of animal intelligence took the form of small narratives about the behaviour of animals related to him through a network of correspondents or published in natural history periodicals or "anecdote books". These anecdotes served as evidence for the "highest level of intelligence to which the animal under consideration can certainly be said to attain" as he states in the excerpt (which is taken from Chapter XVI, "The Dog"). He defined reason or intelligence in contrast to instinct; that is, he believed intelligence showed itself only in circumstances which were untypical for a species—otherwise the responses would be taken care of by reflexes or instincts. Therefore, the relevant displays of the highest faculties were by definition rare, detached and striking events: that is, they were the stuff of anecdote!

Still, Romanes felt it necessary to distinguish himself from the "anecdote mongers". The reason was not that these materials were "mere" anecdotes, as later researchers promoting the experimental method would have it, but that they had been collected for the "mere love of anecdote" (p. vii), and were "merely strung together, with discrimination more or less inadequate" (p. v). Thus, anecdote books and similar formats delivered by popular writers lacked purpose and rigor. In contrast Romanes attempted a "systematic arrangement" (p. vi), "exhibiting in a connected manner the various psychological faculties" (p. 469) and demonstrating the highest level of development achieved by a species. Furthermore, Romanes applied principles to his selection of anecdotes, and thereby disciplined the observations retrospectively, rendering them valuable for this kind of inquiry. As he stated, he chose facts (i.e. anecdotes), which "stand upon the authority of observers well known to me as competent," "are of a kind which do not admit of malobservation", or "are well corroborated by similar accounts received from independent observers" (p. 469). In the anecdotes given in the excerpt, these principles can be observed in action. One observer is introduced as a "well-known traveller and naturalist" (p. 465), malobservation is ruled out as everything happens in plain sight, and several anecdotes are arranged to corroborate each other.

Robert Meunier and Martin Böhnert

1892: GEORGE JOHN ROMANES, *Animal Intelligence* (New York: D. Appleton and Company)

[EXTRACT: pp. 447–470]

Were the purpose of this work that of accumulating anecdotes of animal intelligence, this would be the place to let loose a flood of facts, which might all be well attested, relating to the high intelligence of dogs. But as my aim is rather that of suppressing anecdotes, except in so far as facts are required to prove the presence in animals of the sundry psychological faculties which I believe the different classes to present, I shall here, as elsewhere, follow the method of not multiplying anecdotes further than seems necessary fully to demonstrate the highest level of intelligence to which the animal under consideration can certainly be said to attain. […] It may be well to explain to my numerous correspondents that I select the following cases for quoting, not because they are the most sensational that I have received, but rather because they either contain nothing sufficiently exceptional to excite the criticism of incredulity, or because they happen to have been corroborated by the more or less similar cases which I quote from other correspondents.

[…]

It is, of course, a well-known thing that dogs may easily be taught the use of coin for buying buns, &c. In the 'Scottish Naturalist' for April, 1881, Mr. Japp vouches for the fact that a collie which he knew was in the habit of purchasing cakes with coppers without ever having been taught the use of coin for such purposes. This fact, however, of a dog spontaneously divining the use of money requires corroboration, although it is certain that many dogs have an instinctive idea of giving peace-offerings, and the step from this to the idea of barter may not be large. Thus, to give only two illustrations, Mr. Badcock writes to me that a friend of his had a dog which one day had a quarrel with a companion dog, so that they parted at variance. 'On the next day the friend appeared with a biscuit, which he presented as a peace-offering.' Again, Mr. Thomas D. Smeaton writes to me of his dog that he 'has an amusing practice when he is restored to favour after some slight offence, of immediately picking up and carrying anything that is handiest, stone, stick, paper: it is a deliberate effort to please, a sort of good-will offering, a shaking hands over the past.'

I am indebted for the following to Mr. Groodbehere, of Birmingham; it may be taken as typical of many similar cases: –

> My friend (Mr. James Canning, of Birmingham) was acquainted with a small mongrel dog who on being presented with a penny or a halfpenny would run with it in his mouth to a baker's, jump on to the top of the half-door leading into the shop, and ring the bell behind the door until the baker came forward and gave him a bun or a biscuit in exchange for the coin. The dog would accept any small biscuit for a halfpenny, but nothing less than a bun would satisfy him for a penny. On one occasion the baker (being annoyed at the dog's too frequent visits), after receiving the coin, refused to give the dog anything in exchange, and on every future occasion the latter (who declined being taken in a second time) would put the coin on the floor, and not permit the baker to pick it up until he had received its equivalent.

[…]

The reasoning displayed by dogs may not always be of a high order, but little incidents, from being of constant occurrence among all dogs, are the more important as showing the reasoning faculty to be general to these animals. I shall therefore give a few cases to show the kind of reasoning that is of constant occurrence.

Mr. Stone writes to me from Norbury Park concerning two of his dogs, one large and the other small. Both being in a room at the same time,

> one of them, the larger, had a bone, and when he had left it the smaller dog went to take it, the larger one growled, and the other retired to a corner. Shortly afterwards the

> larger dog went out, but the other did not appear to notice this, and at any rate did not move. A few minutes later the large dog was heard to bark out of doors; the little dog then, without a moment's hesitation, went straight to the bone and took it. It thus appears quite evident that she reasoned—'That dog is barking out of doors, therefore he is not in this room, therefore it is safe for me to take the bone.' The action was so rapid as to be clearly a consequence of the other dog's barking.

Again, Mr. John Le Conte, writing from the University of California, tells me of a dog which used to hunt rabbits in an extensive pasture-ground where there was a hollow tree, which frequently served as a place of refuge for the rabbits when they were pressed:–

> On one occasion a rabbit was 'started,' and all of the dogs, with the exception of 'Bonus,' dashed off in full pursuit. We were astonished to observe that the sedate 'Bonus,' fore going the intense excitement of the chase, deliberately trotted by a short cut to a hollow oak trunk, and crouching at its base calmly awaited the advent of the fleeing rabbit. And he was not disappointed (they frequently escaped without being reduced to this extremity), for the pursuing dogs pressed the rabbit so hard that, after making a long detour, it made for the place of refuge. As it was about entering the hollow trunk, the crouching 'Bonus' captured the astonished rodent.

[…]

Dr. Bannister, editor of the 'Journal of Nervous and Mental Diseases,' writes me from Chicago, that having spent a winter in Alaska, he 'had a good opportunity to study animal intelligence in the Eskimo dogs,' and he reports it as 'a fact of common occurrence,' when the dogs are drawing sledges on the ice near the coast, that on coming to sinuosities in the coast-line, they spontaneously leave the beaten track and strike out so as to 'cut across the windings by going straight from point to point' of land. This is frequently done even when the leading dog 'could not see the whole winding of the beaten track; he seemed to reason that the route must lead around the headlands, and that he could economise travel by cutting across.'

[…]

It would be easy to continue multiplying anecdotes of canine intelligence; but I think a sufficient number of instances have now been given for the only purpose that I have in view—namely, that of exhibiting in a connected manner the various psychological faculties which are presented by dogs, and the level of development to which they severally attain. I may again remark that I have selected these instances for publication from among many others that I could have given, only because they conform to one or other of the general principles to which I everywhere adhere in the quoting of facts. That is to say, these facts are either matters of ordinary observation, and so intrinsically credible; or they stand upon the authority of observers well known to me as competent; or they are of a kind which do not admit of mal-observation; or, lastly, they are well corroborated by similar accounts received from independent observers. I think, therefore, that this sketch of the psychology of the dog is as accurate as the nature of the materials admits of my drawing it. If it is fairly open to criticism on any one side, I believe it is from the side of the dog-lovers, who may perhaps with justice complain that I have ignored a number of published facts, standing on more or less good authority, and appearing more wonderful than any of the facts that I have rendered. To this criticism I have only to answer that it is better to err on the safe side, and that if the facts which I have rendered are sufficient to prove the existence of all the psychological faculties which the dog can fairly be said to possess, it is of less moment that partly doubtful cases should be suppressed, where the only object of introducing them would be to show that some particular faculties were in some particular instances more highly developed than was the case in the instances here recorded.

XIV.

Published in 1903 and issued in over one hundred editions, *The Souls of Black Folk* is a cornerstone not only of the writing of W.E.B. Du Bois's (1868–1963), but of the African American—and indeed American—literary canon. The book is best known for articulating Du Bois's theory of double consciousness, and its assertion that 'the problem of the twentieth century is the problem of the color-line'. These entwined concepts are among the most enduring legacies of Du Bois's thinking as a foundational critical philosopher of race. The 'color-line' is 'the relation of the darker to the lighter races of men in Asia and Africa, in America and the islands of the sea'. Double consciousness entails the inescapable 'two-ness' of the African American, borne by virtue of being Black in 'a [white] world that yields him no true self-consciousness, but only lets him see himself through the revelation of the other [white] world'.

The Souls of Black Folk is arranged as fourteen chapters, nine having previously appeared in essay form. Each opens with lines of text from an American or European literary writer accompanied by a musical score extract from a 'Sorrow Song' or African American spiritual, this juxtaposition of words and music anticipating the thematic range of the chapters to come. These set out to capture, with almost exhaustive comprehensiveness, 'the wretched economic heritage of the freedman from slavery' through Emancipation and Reconstruction, to Du Bois's own Jim Crow era. In formulating his analysis Du Bois draws on methods he terms historical and sociological; cultural as well as economic. This plurality shapes the text's rich amalgam.

The first extract is from the book's analysis of Dougherty County in Georgia's Black Belt. This chapter exposes the extent of economic disenfranchisement among African American communities of the American South. Drawing on his earlier social science analyses of Georgia, home to the largest US Black population, and the only state with 'a detailed record of Negro land-holding' (Du Bois, 1901a: 648; 1901b), Du Bois marries a social scientist's concern with datasets to a novelist's flair (on the latter, see James quoted in Ambar, 2019) for characterisation: 'All this we can only learn by intimate contact with the masses', Du Bois emphasizes; '[w]e often forget that each unit in the mass is a throbbing human soul'. Consequently, focus alternates between macro and micro; from Census statistics (see Du Bois, 1904) and tax returns indicating African American land owner-ship; to the plight of 'Sam and Mary', a representative Black couple, and their descendants. With its trans-national comparative reach extending to old regime Europe, the chapter may be a self-styled 'local study' but it is never parochial.

Rhetorical strategies amplify this multi-layered narrative. Repetition, for example, substitutes pessimism for uplift —'they sink to the class of metayers [sharecroppers]'; 'they sink to metayers' – reversing the emancipatory uplift of Reconstruction-era slave narratives. Shamoon Zamir (1999), drawing on Arnold Rampersad, reads the book as a whole in these terms. He notes this same inversion of 'prophetic models of ascent and uplift' (p. 354) in its final chapter, 'written expressly for the book' (fn. 9, p. 351), on the "Sorrow Songs". Extract two, from this chapter, in showing how spirituals, for Du Bois, simultaneously encapsulate African American experience and constitute the paradigmatic *American* cultural form, demonstrates the extent of the culturalist as well as economic underpinnings to his approach to 'the problem of the color-line'.

Helena Hammond

[See pages 186–187 for references and further reading.]

1903: W. E. B. DU BOIS, *The Souls of Black Folk* edited by Henry Louis Gates Jr. and Terri Hume Oliver (New York and London: W.W. Norton and Company 1999)

[EXTRACTS: pp. 101–103; 154–155; 162–163]

VIII.

Of the Quest of the Golden Fleece

> But the Brute said in his breast, "Till the mills I grind have ceased,
> The riches shall be dust of dust, dry ashes be the feast!
>
> "On the strong and cunning few
> Cynic favors I will strew;
> I will stuff their maw with overplus until their spirit dies;
> From the patient and the low
> I will take the joys they know;
> They shall hunger after vanities and still an-hungered go.
> Madness shall be on the people, ghastly jealousies arise;
> Brother's blood shall cry on brother up the dead and empty skies."
>
> WILLIAM VAUGHN MOODY

[...]

Taking, then, the dissatisfied and shiftless field-hand as a starting-point, let us inquire how the black thousands of Dougherty have struggled from him up toward their ideal, and what that ideal is. All social struggle is evidenced by the rise, first of economic, then of social classes, among a homogenous population. To-day the following economic classes are plainly differentiated among these Negroes.

A "submerged tenth" of croppers, with a few paupers; forty per cent who are metayers and thirty-nine per cent of semi-metayers and wage-laborers. There are left five per cent of money-renters and six per cent of freeholders,—the "Upper Ten" of the land. The croppers are entirely without capital, even in the limited sense of food or money to keep them from seed-time to harvest. All they furnish is their labor; the landowner furnishes land, stock, tools, seed, and house; and at the end of the year the laborer gets from a third to a half of the crop. Out of his share, however, comes pay and interest for food and clothing advanced him during the year. Thus we have a laborer without capital and without wages, and an employer whose capital is largely his employees' wages. It is an unsatisfactory arrangement, both for hirer and hired, and is usually in vogue on poor land with hard-pressed owners.

Above the croppers come the great mass of the black population who work the land on their own responsibility, paying rent in cotton and supported by the crop-mortgage system. After the war this system was attractive to the freedmen on account of its larger freedom and its possibilities for making a surplus. But with the carrying out of the crop-lien system, the deterioration of the land, and the slavery of the debt, the position of the metayers has sunk to a dead level of practically unrewarded toil. Formerly all tenants had some capital, and often considerable; but absentee landlordism, rising rack-rent[1], and falling cotton have stripped them well-nigh of all, and probably not over half of them to-day own their mules. The change from cropper to tenant was accomplished by fixing the rent. If, now, the rent fixed was reasonable, this was an incentive to the tenant to strive. On the other hand, if the rent was too high, or if the land deteriorated, the result was to discourage and check the efforts of the black peasantry. There is no doubt that the latter case is true; that in Dougherty County every economic advantage of the price of cotton in market and of the strivings of the tenant has been taken advantage of by the landlords and merchants, and swallowed up in rent and interest. If cotton rose in price, the rent rose even higher; if cotton fell, the rent remained or followed reluctantly. If a tenant worked hard and raised a large crop, his rent was raised the next year; if that year the crop failed, his corn was confiscated and his mule sold for debt. There were, of course, exceptions to this, —cases of personal kindness and forbearance; but in the vast majority of cases the rule was to extract the uttermost farthing from the mass of the black farm laborers.

The average metayer pays from twenty to thirty per cent of his crop in rent. The result of such rack-rent can only be evil, —abuse and neglect of the soil, deterioration in the character of the laborers, and a widespread sense of injustice. "Wherever the country is poor," cried Arthur Young[2], "it is in the hands of metayers," and "their condition is more wretched than that of day-labourers." He was talking of Italy a century ago; but he might have been talking of Dougherty Country today. And especially is that true to-day which he declares was true in France before the Revolution: "The metayers are considered as little better than menial servants, removable at pleasure, and obliged to conform in all things to the will of the landlords." On this low plane half the black population of Dougherty County—perhaps more than half the black millions of this land—are to-day struggling.

A degree above these we may place those laborers who receive money wages for their work. Some receive a house with perhaps a garden-spot, then supplies of food and clothing are advanced, and certain fixed wages are given at the end of the year, varying from thirty to sixty dollars, out of which supplies must be paid for, with interest. About eighteen per cent of the population belong to this class of semi-metayers, while twenty-two per cent are laborers paid by the month or year. [...] Such laborers receive from thirty-five to fifty cents a day during the working season. They are usually unmarried persons, some being women; and when they marry they sink to the class of metayers, or, more seldom, become renters.

The renters for fixed money rentals are the first of the emerging classes, and form five per cent of the families. […] The men who conduct such farms do not long remain renters; either they sink to metayers, or with a successful series of harvests rise to be land-owners.

In 1870 the tax-books of Dougherty report no Negroes as land-owners. If there were any such at that time, —and there may have been a few, —their land was probably held in the name of some white patron, —a method not uncommon during slavery.

[…]

XIV.

Of the Sorrow Songs

I walk through the churchyard
To lay this body down;
I know moon-rise, I know star-rise;
I walk in the moonlight, I walk in the starlight;
I'll lie in the grave and stretch out my arms,
I'll go to judgment in the evening of the day,
And my soul and thy soul shall meet that day,
When I lay this body down.

NEGRO SONG.

They that walked in darkness sang songs in the olden days—Sorrow Songs—for they were weary at heart. And so before each thought that I have written in this book I have set a phrase, a haunting echo of these weird old songs in which the soul of the black slave spoke to men. Ever since I was a child these songs have stirred me strangely. They came out of the South unknown to me, one by one, and yet at once I knew them as of me and of mine. Then in after years when I came to Nashville I saw the great temple builded of these songs towering over the pale city. To me Jubilee Hall[3] seemed ever made of the songs themselves, and its bricks were red with the blood and the dust of toil. Out of them rose for me morning, noon, and night, bursts of wonderful melody, full of the voices of my brothers and sisters, full of the voices of the past.

Little of beauty has America given the world save the rude grandeur God himself stamped on her bosom; the human spirit in this new world has expressed itself in vigor and ingenuity rather than in beauty. And so by fateful chance the Negro folk-song—the rhythmic cry of the slave—stands to-day not simply as the sole American music, but as the most beautiful expression of human experience born this side of the seas. It has been neglected, it has been, and is, half-despised, and above all it has been persistently mistaken and misunderstood; but notwithstanding, it still remains as the singular spiritual heritage of the nation and the greatest gift of the Negro people.

Away back in the thirties the melody of these slave songs stirred the nation, but the songs were soon half forgotten. […]

[…]

Your country? How came it yours? Before the Pilgrims[4] landed we were here. Here we have brought our three gifts and mingled them with yours: a gift of story and song—soft, stirring melody in an ill-harmonized and unmelodious land; the gift of sweat and brawn to beat back the wilderness, conquer the soil, and lay the foundations of this vast economic empire two hundred years earlier than your weak hands could have done it; the third, a gift of the Spirit. Around us the history of the land has centred for thrice a hundred years; out of the nation's heart we have called all that was best to throttle and subdue all that was worst; fire and blood, prayer and sacrifice, have billowed over this people, and they have found peace only in the altars of the God of Right. Nor has our gift of the Spirit been merely passive. Actively we have woven ourselves with the very warp and woof of this nation, —we fought their battles, shared their sorrow, mingled our blood with theirs, and generation after generation have pleaded with a headstrong, careless people to despise not Justice, Mercy, and Truth, lest the nation be smitten with a curse. Our song, our toil, our cheer, and warning have been given to this nation in blood brother-hood. Are not these gifts worth the giving? Is not this work and striving? Would America have been America without her Negro people?

---

[1] Excessively or unreasonably high rent.

[2] English writer (1741-1820). His Travels in France during the Years 1787, 1788, and 1789 (1792) examines social conditions in France before the Revolution.

[3] Jubilee Hall, a building at Fisk University in Nashville, was completed in 1875. The building was built with the proceeds from the Fisk Jubilee Singers' international singing tour.

[4] The pilgrims landed at Plymouth Rock, Massachusetts, in 1620. In Jamestown, Virginia, in 1619, the first Africans landed in North America. They were either slaves or indentured servants.

XV.

Chandrasekhara Venkata Raman (1888–1970) wrote this letter to *Nature* while returning from his first visit to England on the SS Narkunda of the Peninsula and Oriental Steam Navigation Company. He had been in Oxford to attend the Second Congress of the Universities of the Empire (5–8 July), and left England in September 1921.

Barely two months after the publication of this letter (in November 1921), Raman was nominated to a Fellowship of the Royal Society by Sir Gilbert Thomas Walker, physicist and statistician and Sir George Clarke Simpson, Director of the Meteorological Office, London, who had returned from the Indian Meteorological Department a year before. In the early 1920s, Raman was best known for his work in acoustics but his interest in the scattering of light was already established; his very first letter to *Nature* ('Newton's Rings in Polarised Light', 7 October 1907) had corrected an observation in Thomas Preston's *Theory of Light* (by then in its third edition). In 1907, Raman had been in the Indian Accounts and Audit Services, working after hours on his experiments in a public laboratory; but by 1921, he was traveling as the Palit Professor of Physics at the University Science College, Calcutta.

On this 1921 voyage, Raman carried a pocket sized spectroscope and a Nicol prism and made observations on the Mediterranean, the Red and the Arabian Seas, mailing this letter as soon as he reached the Bombay port. All along the way, he was looking to answer one question: 'What is it that diffracts the light and makes its passage visible?' He had written three letters to *Nature* (whilst still in London): 'The Colours of Breathed-on Plates' (4 August), 'The Radiant Spectrum' (1 September), 'Smoky Quartz' (15 September), and a fourth from the port of Aden on his way back to India: 'A Method of Improving Visibility of Distant Objects' (20 October). Two of the letters engage directly with Lord Rayleigh's theory of the elastic scattering of light, a phenomenon now known as Rayleigh scattering, to explain why the sky is blue. While the two letters build upon the observable claims in Rayleigh's theory, the fifth—the letter we are reading—claims that Rayleigh's account falls short.

Raman's fourth letter had detailed the usefulness of the Nicol prism, a tool and method proposed in 1876 by David Wilson-Barker in *A Manual of Elementary Seamanship* and already in use during WWI. The Nicol prism, based upon the phenomenon of double refraction, transmits waves vibrating in one direction only to produce a plane-polarized beam from ordinary light. Observing water over three seas with this simple prism, Raman concluded that water molecules scatter light just like air molecules which Rayleigh had forwarded as an explanation why the sky was blue!

Back in Calcutta, he immediately commenced correspondence connecting his insight on the colour of the sea with quantum theory all while deepening his study with sixty different common liquids. His colleague Kariamanikkam Srinivasa Krishnan, and he found that the scattered light was polarised, a finding again published in *Nature*; 'A New type of Secondary Radiation' (31 March 1928). Following this, Raman began to measure the exact wave-lengths of the incident and scattered light using a spectroscope. Eventually, this work led to a Nobel Prize for Raman (1930), the first Asian to be thus recognised in science.

Jahnavi Phalkey

1921: C. V. Raman, The Colour of the Sea *Nature* no. 2716 vol. 108 p. 367

The view has been expressed that “the much admired dark blue of the deep sea has nothing to do with the colour of water, but is simply the blue of the sky seen by reflection” (Rayleigh's Scientific Papers, vol. 5, p. 540, and NATURE, vol. 83, p. 48, 1910). Whether this is really true is shown to be questionable by a simple mode of observation used by the present writer, in which surface-reflection is eliminated, and the other factors remain the same. The method is to view the surface of the water through a Nicol's prism, which may for convenience be mounted at one end of a tube so that it can be turned about its axis and pointed in any direction. Observing a tolerably smooth patch of water with this held in front of the eye at approximately the polarising angle with the surface of the sea, the reflection of the sky may be quenched by a suitable orientation of the Nicol. Then again, the sky-light on a clear day in certain directions is itself strongly polarised, and an observer standing with his back to the sun when it is fairly high up and viewing the sea will find the light reflected at all incidences sufficiently well polarised to enable it to be weakened or nearly suppressed by the aid of a Nicol.

Observations made in this way in the deeper waters of the Mediterranean and Red Seas showed that the colour, so far from being impoverished by suppression of sky-reflection, was wonderfully improved thereby. A similar effect was noticed, though somewhat less conspicuously, in the Arabian Sea. It was abundantly clear from the observations that the blue colour of the deep sea is a distinct phenomenon in itself, and not merely an effect due to reflected skylight. When the surface-reflections are suppressed the hue of the water is of such fullness and saturation that the bluest sky in comparison with it seems a dull grey.

By putting a slit at one end of the tube and a grating over the Nicol in front of the eye, the spectrum of the light from the water can be examined. It was found to exhibit a concentration of energy in the region of shorter wave-lengths far more marked than with the bluest sky-light.

Even when the sky was completely overcast the blue of the water could be observed with the aid of a Nicol. It was then a deeper and fuller blue than ever, but of greatly enfeebled intensity. The altered appearance of the sea under a leaden sky must thus be attributed to the fact that the clouds screen the water from the sun's rays rather than to the incidental circumstance that they obscure the blue light of the sky.

Perhaps the most interesting effect observed was that the colour of the water (as seen with the Nicol held at the polarising angle to the surface of the water and quenching the surface-reflection) varied with the *azimuth* of observation relatively to the plane of incidence of the sun's rays on the water. When the plane of observation and the plane of incidence were the same, and the observer had his back to the sun and looked down into the water, the colour was a brilliant, but comparatively lighter, blue. As the plane of observation is swung round the colour becomes a deeper and darker blue, and at the same time decreases in intensity, until finally when the plane of observation has swung through nearly 180° the water appears very dark and of a colour approaching indigo. Both the colour and the intensity also varied with the altitude of the sun.

The dependence of the colour on the azimuth of observation cannot be explained on a simple absorption theory, and must evidently be regarded as a *diffraction* effect arising from the passage of the light through the water. Looking down into the water with a Nicol in front of the eye to cut off the surface reflections, the track of the sun's rays could be seen entering the water and appearing by virtue of perspective to converge to a point at a considerable depth inside it. The question is: What is it that diffracts the light and makes its passage visible? An interesting possibility that should be considered in this connection is that the diffracting particles may, at least in part, be the *molecules* of the water themselves. As a rough estimate, it was thought that the tracks could be seen to a depth of 100 metres, and that the intensity of the light was about one-sixth of that of the light of the sky from the zenith. If we assume that clear water, owing to its molecular structure, is capable of scattering light eight times as strongly as dust-free air at atmospheric pressure, it is clear that the major part of the observed effect may arise in this way.

It is useful to remember that the reflecting power of water at normal incidence is quite small (only 2 per cent.), and becomes large only for very oblique reflection. It is only when the water is quite smooth and is viewed in a direction nearly parallel to the surface that the reflected sky-light overpowers the light emerging from within the water. In other cases the latter has a chance of asserting itself.

C. V. Raman

S.S. Narkunda, Bombay Harbour,

September 26.

XVI.

During the winter of 1919–20 Max Weber (1864–1920) was lecturing four times a week in Munich on 'Outlines of a Universal Social and Economic History' and drafting Ch. 2 of *Economy and Society*, on the 'Basic Sociological Categories of Economic Action', constructing an account of economic institutions from social processes. Working under great pressure to complete a major section of the *Handbook of Social Economics* (*Grundriss der Sozialökonomik*) he had been editing since 1908, he died in June 1920 of pneumonia, leaving this chapter as his last statement on the relationship of social, economic and historical analysis. As in the other two chapters he completed, he proceeded alternately by definition and elaboration, in Ch. 2 the former in forty-three numbered paragraphs. The haste with which he was writing is obvious from a comparison with Ch. 1, which has a far more tightly-organised structure; but the extract here preserves a sense of what Weber was seeking to achieve. It is from §19, continuing the discussion of the "distribution of activity (*Leistungsverteilung*)" contained in §18 into "the manner in which the existing Chancen for work designated for remuneration are appropriated".

This statement typifies the definitional function, and it is followed by elaboration that contains historical narration. First of all, the definitional paragraph was printed in a larger typeface than the following elaboration. Second, Weber made liberal use of emphases (spacing out words in the German style of Sperrungen) which I reproduce here in bold face. Third, Weber consistently uses the same German word, Chance (plural Chancen), for what might otherwise be translated, depending on context, as chance, opportunity, coincidence, probability, or contingency. To emphasise the regularity with which this lexeme occurs in the text I left it in German (hence here italicised). Fourthly, Weber's insistent, if sometimes confusing, terminology here means that "appropriation of work-places" might otherwise be translated as "allocation of workplaces"; but he makes no distinction between "appropriated by someone" and "allocated to someone", using instead the single term "appropriation". This is because "allocation" requires an agency other than the subject of the action, whereas Weber seeks to maintain a focus on the subject of the action.

In this extract Weber is considering the historical evolution of employment and enterprise without presuming any master-narrative of exploitation, power, or individual liberty that has otherwise characterised accounts of the economic organisation of labour, explicitly historical or otherwise. He first lays out a series of relationships and their possible permutations, the first paragraph of elaboration qualifying the definition more discursively. Then in the second paragraph he expands on this historically. The evolutionary story that he is seeking to highlight in this way is however specific to his leading interest —why this form would only develop in the Occident. During the same period Weber had prepared for separate publication his wartime essays on the religion and culture of Judaism, India and China, plus a revised version of the 1904/1905 essays on the Protestant Ethic, and he added to this a foreword drawing emphatic attention to the fact that, far from writing comparative history or sociology, his leading interest was to explain the singularity of European capitalism. The extract here illustrates how he went about this task.

Keith Tribe

1922: MAX WEBER, *Economy and Society: A New Translation. Edited and translated by Keith Tribe* (Cambridge, Mass: Harvard University Press 2019)

[EXTRACT: pp. 232-234]

[...]

Any appropriation of workplaces in gainful enterprises to workers, **just as** in reverse the appropriation of the valorisation of workers ("unfree") to owners, implies a restriction to the free recruitment of the labour force, and hence to the **selection** of workers according to their optimal efficiency, and hence also a limitation on the **formal** rationalisation of economic activity. This substantively furthers the restriction of technological rationality insofar as

the gainful **valorisation** of the products of labour are appropriated to an **owner**;

a) the tendency exists to allocate quotas for work performed (traditional, conventional, or contractual),

b) there is a diminution or, where the worker is entirely appropriated to the owner (slavery of a people), complete disappearance of any interest the worker might have in working at an optimum level;

II. there is appropriation to the worker through a conflict between the worker's self-interest with respect to a traditional life-situation (*Lebenslage*) and efforts on the part of those seeking to realise value to (a) force their work to a technologically optimum level, or (b) employ technological substitutes for their labour. Masters (*Herren*) will thus always be inclined to transform valorisation into a mere source of **rent**. Appropriating the realisation of gains from products to the **worker** thus tends to favour, where circumstances are favourable, the more or less complete expropriation of the **owner** from **management**. Another regularly occurring phenomenon is the emergence of the worker's material dependence on partners in exchange who enjoy an advantage (*Verleger*) and assume the position of manager.

1. There is a practical similarity in outcomes for these two, formally opposed, tendencies: appropriation of the place of work to the worker, and of the worker to an owner. There is nothing especially striking here. First, both are very often already **formally** connected to each other. This is the case when the appropriation of a worker to a master coincides with the appropriation of *Chancen* for gain on the part of a worker to a closed organisation of workers, such as court retainers. In such cases, there is of course extensive stereotyping of the worker's capacity for valorisation, that is, the reduction of work to set quotas, diminution of any interest in work, and therefore workers' successful resistance to any form of technological "reform" or "renewal." But even where this does not occur, the appropriation of a worker to an owner **actually** means that the masters are dependent on the valorisation of **this** particular workforce that he has not, as in a modern factory enterprise, **selected** for employment, but that must simply be accepted without discrimination. This is especially true for slave labour. Every attempt to squeeze more from appropriated workers, varying from the amount they are traditionally accustomed to perform, meets with traditionalist obstruction and could be sustained only by the most ruthless means. Normally, such a course would not be without danger for the master's self-interest, for it would endanger the **traditional** foundation of his dominant position. Consequently, the work of appropriated workers has everywhere had a tendency to settle at a set amount, and where this has been broken by the masters'

power (as in eastern Europe at the beginning of the modern period), the absence of selectivity and the lack of any self-interest or readiness to take risks on the part of appropriated workers has obstructed progression to a technological optimum. Where positions of work have been formally appropriated to the **worker**, the same outcome has ensued, only more rapidly.

2. The case described in the last sentence is typical for developments of the early medieval period (the tenth through the thirteenth centuries). The assarts of the Carolingian period contracted and disappeared, together with all other initial elements of large agricultural enterprise. The rents of landowners and feudal lords became stereotypical and settled at a very low level; an increasing proportion of the natural product (agriculture, mining) and of the money return from craft industry found its way into the workers' hands. The "favourable circumstances" for this path of development, which took **this** form only in the Occident, were (1) the propertied stratum's political and military preoccupations, (2) the impossibility, owing to the absence of suitable administrative staff, of property owners employing workers as anything other than a source of rent, linked to (3) the difficulty of preventing workers from moving freely between the property owners' competing particular interests, (4) the massive *Chancen* for opening up new land, sinking mines, and creating local markets, in connection with (5) the technological tradition of antiquity. The more that *Chancen* for gain were appropriated **to** workers (classical types are in mining and English guilds), instead of the worker being appropriated **to** the owner—expropriation then rendering owners pure receivers of rents (followed even then ultimately by the abrogation or repudiation of any duty to render payment of rent—"the freedom of town life")—the more that workers found that differentiated *Chancen* for making gains in a **market** were quickly opening up in their midst (and were also being introduced externally by traders).

XVII.

Economists across the international community became obsessed with the study of economic cycles in the late 19th and early 20th centuries—short ones for stock market activity ('Kitchins'), middle-length ones for the 'business cycle', and 'long waves' ('Kondratievs'). They were instrumental in developing generic methods of studying time-series statistical data in order to 'pin down' these phenomena. This work took theoretical arguments about the causes or mechanisms that created these different kinds of cycles as secondary, but as we can see in this extract, they could not be ignored. It was certainly data-heavy science compared to the early 19th century account Malthus gave of his demographic-economic oscillations (see **III.**).

The Russian economist, Nikolai Kondratiev (or Kondratieff, 1892–1938), has his name attached to these long 50-year cycles, for though there were others speculating about their existence before him, he was the first to conduct sustained empirical data collection, data preparation and data analysis in order to establish them as a valid phenomenon of the economy. He was the first director of the Conjuncture Institute in Moscow, one of a group of international cycle research institutes (mostly state-founded) created in the 1920s. These economists followed each other's work closely, and they, including Kondratiev, travelled abroad to meet each other.

Kondratiev's two main problems were first, establishing that long waves existed; and second, giving an account of what makes such waves happen. It is in wrestling with this second problem that we find narrative at work, but rather unexpectedly—narratives of consequences not of causes.

Kondratiev points out four characteristics of such long waves that can be associated with the waves, and rejects each of these as being random in origin: so these are 'law governed events'. Perhaps these factors are the causes of his waves? But no, says Kondratiev, these factors are consequences not causes. It would have been easy to tell a causal narrative involving each factor, but he takes the harder argument—and trickier narrative—of inserting each factor, and its own consequential story, into an account of his long waves. Taken together, and assuming that long waves are inherent in the capitalist system, they enable him to tell a generic narrative in which these factors only become salient because of, and within the dynamics of, the wave.

Given the context of post-revolutionary Russia, his careful equivocation might have looked politically astute, but these consequential stories proved less problematic than his analysis and advice on economic growth and the role of agriculture in the new plan for the economy. In 1928 he was dismissed, in 1930 the Institute was shut down, in 1932 he was sent to prison (where he continued with his economics writing), and in 1938 he was executed.

Both of Kondratiev's questions have continued to reverberate through the decades since then. Do these waves exist? It seems that the jury is still out. Equally, economists—of both Marxist and non-Marxist persuasion—have been arguing ever since about the role of those factors, particularly the role of technological change in the timing of economic changes.

Mary S. Morgan

1935: N. D. KONDRATIEFF, The Long Waves in Economic Life (transl: W.F. Stolper) *The Review of Economics and Statistics* vol. 17:6 pp. 105–115

I. Introduction and Method

The idea that the dynamics of economic life in the capitalistic social order is not of a simple and linear but rather of a complex and cyclical character is nowadays generally recognized. Science, however, has fallen far short of clarifying the nature and the types of these cyclical, wave-like movements.

When in economics we speak of cycles, we generally mean seven to eleven year business cycles. But these seven to eleven year movements are obviously not the only type of economic cycles. The dynamics of economic life is in reality more complicated. In addition to the above-mentioned cycles, which we shall agree to call "intermediate," the existence of still shorter waves of about three and one-half years' length has recently been shown to be probable.

But that is not all. There is, indeed, reason to assume the existence of long waves of an average length of about 50 years in the capitalistic economy, a fact which still further complicates the problem of economic dynamics.

The succeeding study is to be confined solely to an inquiry into various problems connected with these long waves. Investigation here is made difficult by the fact that a very long period of observation is presupposed. We have, however, no data before the end of the eighteenth century and even the data that we do have are too scanty and not entirely reliable. Since the material relating to England and France is the most complete, it has formed the chief basis of this inquiry.

[…]

X. Statistical Findings

The evidence we have presented thus far permits some conclusions.

[…]

First long wave	1. The rise lasted from the end of the 1870's or beginning of the 1790's until 1810-17.
	2. The decline lasted from 1810-17 until 1844-51.
Second long wave	1. The rise lasted from 1844-51 until 1870-75.
	2. The decline lasted from 1870-75 until 1890-96.
Third long wave	1. The rise lasted from 1890-96 until 1914-20.
	2. The decline probably begins in the years 1914-20.

[…]

The long waves that we have established above relative to the series most important in economic life are international; and the timing of these cycles corresponds fairly well for European capitalistic countries. On the basis of the data that we have adduced, we can venture the statement that the same timing holds also for the United States.

[…]

XII. The Nature of Long Waves

[…]

It has been pointed out […] that the long waves—as distinct from the intermediate ones which come from causes within the capitalistic system—are conditioned by casual, extra-economic circumstances and events, such as (1) changes in technique, (2) wars and revolutions, (3) the assimilation of new countries into the world economy, and (4) fluctuations in gold production.

These considerations are important. But they, too, are not valid. Their weakness lies in the fact that they reverse the causal connections and take the consequence to be the cause, or see an accident where we have really to deal with a law governing the events. […]

1. *Changes in technique* have without doubt a very potent influence on the course of capitalistic development. But nobody has proved them to have an accidental and external origin.

Changes in the technique of production presume (1) that the relevant scientific-technical discoveries and inventions have been made, and (2) that it is *economically* possible to use them. It would be an obvious mistake to deny the creative element in scientific-technical discoveries and inventions. But from an objective viewpoint, a still greater error would occur if one believed that the direction and intensity of those discoveries and inventions were entirely accidental; it is much more probable that such direction and intensity are a function of the necessities of real life and of the preceding development of science and technique.

Scientific-technical inventions in themselves, however, are insufficient to bring about a real change in the technique of production. They can remain ineffective so long as economic conditions favorable to their application are absent. This is shown by the example of the scientific-technical inventions of the seventeenth and eighteenth centuries which were used on a large scale only during the industrial revolution at the close of the eighteenth century. If this be true, then the assumption that changes in technique are of a random character and do not in fact spring from economic necessities loses much of its weight. We have seen before that the development of technique itself is part of the rhythm of the long waves.

2. *Wars and revolutions* also influence the course of economic development very strongly. But wars and revolutions do not come out of a clear sky, and they are not caused by arbitrary acts of individual personalities. They originate from real, especially economic, circumstances. The assumption that wars and revolutions acting from the outside cause long waves evokes the question as to why they themselves follow each other with regularity and solely during the upswing of long waves. Much more probable is the assumption that wars originate in the acceleration of the pace and the increased tension of economic life, in the heightened economic struggle for markets and raw materials, and that social shocks happen most easily under the pressure of new economic forces.

Wars and revolutions, therefore, can also be fitted into the rhythm of the long waves and do not prove to be the forces from which these movements originate, but rather to be one of their symptoms. But once they have occurred, they naturally exercise a potent influence on the pace and direction of economic dynamics.

3. As regards the *opening-up of new countries for the world economy*, it seems to be quite obvious that this cannot be considered an outside factor which will satisfactorily explain the origin of long waves. The United States have been known for a relatively very long time; for some reason or other they begin to be entangled in the world economy on a major scale only from the middle of the nineteenth century. Likewise, the Argentine and Canada, Austral-

ia and New Zealand, were discovered long before the end of the nineteenth century, although they begin to be entwined in the world economy to a significant extent only with the coming of the 1890's. It is perfectly clear historically that, in the capitalistic economic system, new regions are opened for commerce during those periods in which the desire of old countries for new markets and new sources of raw materials becomes more urgent than theretofore. It is equally apparent that the limits of this expansion of the world economy are determined by the degree of this urgency. If this be true, then the opening of new countries does not provoke the upswing of a long wave. On the contrary, a new upswing makes the exploitation of new countries, new markets, and new sources of raw materials necessary and possible, in that it accelerates the pace of capitalistic economic development.

4. There remains the question whether the *discovery of new gold mines*, the *increase in gold production*, and a consequent *increase in the gold stock* can be regarded as a casual, outside factor causing the long waves.

An increase in gold production leads ultimately to a rise in prices and to a quickening in the tempo of economic life. But this does not mean that the changes in gold production are of a casual, outside character and that the waves in prices and in economic life are likewise caused by chance. We consider this to be not only unproved but positively wrong.

[...] it seems to us, that gold production, even though its increase can be a condition for an advance in commodity prices and for a general upswing in economic activity, is yet subordinate to the rhythm of the long waves and consequently cannot be regarded as a causal and random factor that brings about these movements from the outside.

XIII. Conclusions

[...]

In asserting the existence of long waves and in denying that they arise out of random causes, we are also of the opinion that the long waves arise out of causes which are inherent in the essence of the capitalistic economy. This naturally leads to the question as to the nature of these causes. We are fully aware of the difficulty and great importance of this question; but in the preceding sketch we had no intention of laying the foundations for an appropriate theory of long waves.

XVIII.

While the life and work of Jane Goodall (1934–) are the subject of many, including autobiographic, narratives, her research publications also exhibit many elements of narrative. Unlike reproducible laboratory experiments in behavioural biology, Goodall's data consisted of particular and unique events in the life of a group of chimpanzees. The question of personal differences among chimpanzees in the wild was an essential part of the research programme, as were historical changes in the social structure and habits of the specific group observed. Nonetheless, the task was to identify 'patterns' of behaviour. Unique events had to be interpreted as instances of these patterns. Personality then consisted in individual variations of the pattern; change was the establishment, vanishing or modification of patterns.

The excerpt is taken from one of Goodall's earliest publications, reporting research conducted between 1960 and 1963. The introduction makes clear that questions regarding the evolution of humans were at stake. The work was perceived as challenging the notion that tool-making was solely a human capacity. The perception of the section that links aimed throwing to aggression, for instance, has to be situated in the anthropological and public obsession at the time—in the wake of two world wars and in the midst of the cold war—with humans not only as tool-makers, but as makers of weapons of mass destruction.

The basic evidence-reporting locution of such an observational study is "On one (two, …) occasion(s) I saw …". However, all 'occasions' were to some extent created by Goodall through various means. First, observation consisted in systematic screening and in making careful choices of sites and times for observation, as well as in selecting suitable techniques of observation and recording. Additionally, sites of action were inspected afterwards, measurements were made of abandoned tools, and attention was focussed on clues for events that took place in the absence of an observer. Finally, the artificial feeding area functioned as a reliable generator of occasions.

After reporting the state of research and the site and conditions for observation in the introductory paragraphs, Goodall, in the first subsection recounts a small number of individual observations on the use of sticks, which together with circumstantial evidence and observation in the feeding area are taken to indicate that the use of sticks occurs regularly. Evidence is similarly anecdotal in the section on throwing.

The narration changes in the section on eating termites, which is written in a rather statistical mode. Larger numbers of observations are synthesized into a robust behavioural pattern. Individual observations are only mentioned as deviations from the pattern, such as in infants, who have not yet acquired the necessary skills.

Overall, the narrative of the text gives the impression of being a synopsis of notes, which are filtered and evaluated within a classification scheme that categorizes different behaviours according to aspects of the chimpanzees' life. In this case, all occasions of relevance for the question of tool use were singled out from the flow of recorded behaviours and were drawn together such that significant patterns emerge from the variety of unique events. These patterns could then be interpreted regarding pertinent questions about the evolution of behaviour. For instance, in the conclusion inborn patterns are identified that serve as a basis for learned abilities which might vary among groups or individuals.

Robert Meunier

1964: JANE GOODALL, Tool-using and aimed throwing in a community of free-living chimpanzees *Nature* vol. 201 no. 4926 pp. 1264–1266

The use of natural objects as tools in free-living non-human primates is of interest, not only as throwing light on the capacities of the species itself, but also in connexion with theories concerning tool-using and the development of tool cultures in prehistoric man. There is abundant evidence in the literature that captive apes, and some monkeys, can use objects both as tools and as aimed missiles. Reports of tool-using and aimed throwing in wild primates, however, are scarce. […]

During three years in the Gombe Stream Chimpanzee Reserve in Tanganyika, East Africa, I saw chimpanzees use natural objects as tools on many occasions. These objects consisted of sticks, stalks, stems and twigs which were used mainly in connexion with eating insects, and leaves which were used as 'drinking tools' and for wiping various parts of the body. For six months (mid-June–mid-December) at the end of the period of investigation, 25 chimpanzees, comprising all age and sex classes, came fairly regularly to an artificial feeding area, enabling me to study all aspects of behaviour in greater detail. Under these conditions I saw three chimpanzees use sticks for a completely new purpose, further instances of leaves used for wiping, and, for the first time, aimed aggressive throwing.

Use of Sticks

On two occasions the chimpanzees used sticks to feed on different species of ants. One observation involved the underground nest of *Dolaris* sp. and the other an arboreal nest of *Crematogaster (Atopogyne)* sp., but in both cases the method used was similar: the chimpanzees (groups of four and five respectively) pushed their sticks into the nests, which had been opened up before my arrival, waited for a moment, withdrew them, and removed the ant-mass on the end with their lips. Most of the sticks used in this way were estimated as between 1 ft. 6 in. and 2 ft. 6 in. long (although I measured one which was 3 ft. 6 in.), and they were either broken from nearby branches or picked up from the ground.

[…]

In addition to the foregoing direct observations there is some further circumstantial evidence. On three occasions I found freshly opened *Dolaris* nests with sticks (still with traces of earth adhering to them) lying nearby: another time a chimpanzee broke off a stick and carried it out of sight to a tree where I later found a newly opened nest of *Crematogaster (Atopogyne)*.

During the final six months, at the artificial feeding area, three adolescents, independently, used sticks to try to open boxes containing bananas. After pulling and pushing at the boxes for up to 5 min, each one broke off a stick and stripped it of leaves. Two individuals then tried to push their sticks under the box lids. The third pushed his into the bananas through a hole in the bottom of the box. None of the three had seen either of the others trying to solve the problem in this way.

Use of Stalks and Stems for Eating Termites

Stalks, stems and small twigs were used when the chimpanzees fed on termites *(Macrotermes bellicocus* (Smeath)). This occurred from early November until January (approximately) when the insects extend their passages to the surface of the nest. The termites bit on to tools pushed into these holes: the chimpanzees then withdrew the tools and picked off the insects with their lips. This behaviour was seen on ninety-one different occasions, on twenty-three of which from 2 to 5 individuals were working together. In addition termite heaps which were not ready for working were frequently examined, and on eighteen occasions chimpanzees picked tools before or during such an examination.

The material used was usually 6–12 in. long. Some individuals inspected several clumps of grass, etc., before selecting their tools, and sometimes picked several to carry back to the termite heap and then used them one at a time. Other individuals used more or less anything within reach, including tools left by others. Sometimes tools were carefully prepared: leaves were stripped from stems or twigs with the hand or lips, and long strips were sometimes pulled from a piece of grass which was too wide. No chimpanzee was seen to move farther than ten yards from the site to select a new tool, but individuals were observed to pick a tool for use on a heap which was out of sight and as far as one hundred yards away. One male twice carried a tool for more than half a mile while inspecting a series of termite heaps, none of which was ready for working.

On two occasions I saw an infant approximately 18-22 months old 'fishing' for termites. Every 10 min or so she broke off to play around for a few minutes, or to watch other individuals, before returning to work. This was unlike older individuals, who frequently worked for more than 1 h with intense concentration. This infant showed the complete adult pattern, but her technique was not fully developed. Her tools were too short to be completely effective (the longest I measured was only 2 in.) and after pushing one into a hole she pulled it out immediately, usually jerking off any termite which had caught hold. [...]

[...]

Aimed Throwing

In the Gombe Stream area the throwing of sticks, stones or handfuls of vegetation, apparently at random, frequently formed part of the displays of socially excited or frustrated males. These displays, which occurred when groups met, arrived at feeding areas, or when males were unable to obtain desired objectives, consisted mainly of charging and bending down or dragging branches, together with drumming on the ground or on trees. Objects thrown during these displays were not normally directed towards other individuals: on the few occasions when they were, this seemed accidental. I saw only six individuals throw towards an objective with definite aim: one mature male threw three objects, three mature males each threw two objects, one threw one, and two adolescents each threw one. One such instance occurred when an adolescent broke off a stick, took careful aim, and threw it overarm at another chimpanzee below; on this occasion the action was apparently playful. However, during the final six months objects were thrown at human beings and baboons in cases when the context indicated that the action was aggressive.

Three times objects were thrown at human beings. On two different occasions a mature male threw rocks towards the expedition photographer whose presence was making the chimpanzee afraid to approach a box of bananas. One rock weighed 6-8 lb. and the other was twice as heavy. Both were thrown underarm (the second with two hands) and travelled about three yards in a direct line with their objective, falling short by 1 ft. and 3 ft. respectively. The third incident occurred when the presence of the photographer prevented an adolescent male from approaching to investigate a toy chimpanzee tied to a tent pole. He picked up a small stone and threw it underarm in a direct line with the photographer. It went 5 yards, but fell short by 4 ft.

Other instances of aimed throwing were directed against baboons. Competition for food at the artificial feeding area gave rise to an abnormal number of aggressive interactions between the two species, and on three occasions male chimpanzees threw stones at baboons which approached too closely. One male threw overarm three times in succession, first a handful of leaves and then two small stones, none of which hit their objectives. Two other chimpanzees threw underarm: one individual threw a large stone which travelled about half-way towards a baboon some 10 yards away, and the other threw two small stones, the second of which hit a baboon which was only about 5 ft. away.

[…]

So far, my observations on aggressive encounters between chimpanzees and other species are too limited to enable any conclusions to be drawn with regard to aggressive throwing. However, such results as I have obtained suggest that, as a method or intimidation, aimed throwing is only occasional and that when it does occur the chimpanzee picks up objects of any size which are to hand. […]

Conclusion

The ability to use objects as tools appears to be characteristic of chimpanzees. Schiller suggests that certain inborn manipulative patterns are available to the chimpanzee from which adaptive behaviour, such as tool-using, may be derived. The extent to which the development of such manipulative abilities depends on experience and learning cannot as yet be assessed; but Schiller found that captive individuals had to be familiar with objects before they could use them successfully as tools. The wild-born infant, from an early age, has much opportunity to manipulate and become familiar with leaves, twigs and branches, particularly during play and feeding activities.

[…] It therefore seems probable that the use of sticks, stems and leaves for the specific purposes described here represents a series of primitive cultural traditions passed on from one generation to the next in the Gombe Stream area.

[…]

[Endnotes omitted]

XIX.

E. J. Corey (1928–), a Nobel Prize winner in chemistry, is renowned as one of the greatest synthetic chemists of the second half of the twentieth century. He codified synthesis by elucidating general principles, thus making it a matter of rational planning that drew on databases of established techniques, rather than it being a case of relying solely on the chemist's brilliant insight and capacity for pattern recognition.

The excerpt given here presents two sections from one of Corey's papers. In the first section, he notes the large number of syntheses which have already been accomplished, and the great variety of chemical substances and reactions which the organic chemist has at her disposal, which may lead us to assume that chemists must always work on a case-by-case basis in solving specific problems. Nevertheless, Corey argues, similar principles, implicit in the working practices of chemists, can be seen as informing the planning of syntheses. He goes on to outline some 'axioms' for successful design of chemical syntheses, and the criteria by which a chemist makes judgments about the quality of a given synthetic plan.

Corey's views of synthesis recall discussions of narrative in two main senses. First, scholars of narrative have often insisted that we make sense of narratives by comparing what happens within a given narrative with other courses of actions which were possible, but which did not occur on this occasion. Corey invites chemists in their planning of syntheses to be clear about experimental paths which were available but not taken. Second, some narratologists have sought to identify the constituent parts and rules of assembly which (they claim) underlie the diversity of narrative forms.

In the second section excerpted here, Corey talks about how to go about deciding on a set of synthetic intermediates—the sequence of molecules which a starting structure can be changed into, on its way towards the desired final form. He goes from talking in quite general terms about the factors which can complicate a synthetic problem, to discussing the specific techniques for different structural forms, and hence to specific individual syntheses which exemplify the desired synthetic features. Then he pulls back to talk in more general terms, providing a list of the steps involved in generating a set of intermediates. The list for planning resembles a computer program, but he suggests that in practice chemists may not be able to apply all of the steps, and that a plan provides a general direction of travel informed by experimental practice rather than a specific set of instructions.

Again, an abiding concern for narratologists has been the extent to which computers can facilitate meaningful analysis of narrative, or even the generation of new narratives (Mani 2014). Corey has long argued for the place of computers in helping to plan syntheses, though (as he does here) he has also insisted that such computer tools should complement a chemist's skill and creativity, rather than replace them (Hepler-Smith 2018).

Mat Paskins

[See page 187 for references.]

1967: E. J. COREY, General Methods for the Construction of Complex Molecules *Pure and Applied Chemistry* 14:1 pp. 19–37

[EXTRACTS: pp. 19–30]

[…]

The achievement of a synthesis of a complex organic molecule involves a number of distinctly identifiable operations which, however, are not strictly independent of one another. These include the choice of the molecule to be synthesized, the development of a synthetic strategy and plan in general outline, the selection of specific individual steps and their ordering, and the experimental execution of the synthesis. The absence of a clean separation between component elements makes the task of analysing and understanding Synthesis as an intellectual discipline appear quite forbidding, especially in the most crucial aspect, derivation of a general plan. Further, the enormous diversity and number of organic structures now known to exist, the incredibly broad spectrum of reactions available for synthesis, and the uncertain and severe limits on the applicability of any given reaction all combine to create an impression that the design of a synthesis is apt to be tenuously hypothetical and is mainly a function of the unique circumstances in each particular case and, moreover, that considerations of a highly general nature are neither dominant nor very useful. None the less, a sufficiently great number and variety of syntheses have now been completed to encourage attempts at setting down in a generalized form the process by which a synthetic chemist devises an original but valid synthetic route to a complicated structure. Such an effort surely is more than an intriguing theoretical exercise; it is a prerequisite to a deeper comprehension of Synthesis and the methodologies which are fundamental to it, and it is likely to be a keystone in the rational development of Synthesis to still higher forms. For example, any technique for the automatic generation of synthetic schemes by a computer will require a complete and detailed definition of the elements of Synthesis and their mutual interaction, in a most general sense.

Axioms

There are certain considerations which can be regarded as axiomatic to Synthesis and which serve as pre-conditions for any general analysis of methodology. These include the following:

1. The various elements involved in the solution (and even the selection!) of a synthetic problem are not separable. If a division of these elements in Synthesis is made for purposes of simplifying an analysis, it must be compensated for by allowing their interaction at some stage to produce further modification of the process.

2. A very large number of possible routes to the synthesis of a complex molecule can usually be generated. Each of these involves a sequence of reactions and proceeds via a number of intermediates whose synthesis is more direct than that of the target molecule. Naturally, the starting point for any route should be a readily available synthetic substance.

3. These possible routes are derivable by the recognition of structural units within molecules which can be formed and/or assembled by known or conceivable synthetic operations. (In the discussion which follows, these units are designated as "synthons". In this paper the term "synthetic operations" is used in the molecular sense to denote structural transformations rather than in the laboratory sense, which would imply manipulation.)

4. There are definite but not absolute criteria by which the merits or quality of alternative projected syntheses can be judged. Often there are a sufficiently large number of unknowns to render the selection of a superior synthetic route arbitrary. These criteria, none the less, always serve the very important

purpose of dictating the rejection of a very large number of inferior possibilities.

5. The specific criteria by which synthetic alternatives may be judged are on the whole elementary to the organic chemist. The items which follow are important and indicative.

> A. The probability of achieving the desired change at each step in the sequence should be high. [...]
>
> B. Bypass routes or potential alternatives should exist, particularly where the functioning of one or more of the individual steps is questionable.
>
> C. The solution should be simple. [...]

[...]

Generation of sequences and specific intermediates

The task of devising a definite set of synthetic intermediates connected by specific synthetic operations is usually simplest for acyclic structures. Within this category, as is generally the case, a number of factors can complicate the problem, including substitution and functionality, elements of instability, and the presence of asymmetric centres. [....] It is advantageous to consider first the general principles behind the generation of a sequence of synthetic intermediates for acyclic cases and then to examine the modifications or elaborations which may be appropriate to cyclic structures.

For acyclic molecules the recognition of important synthons is a prerequisite to the generation of possible synthetic intermediates. In the case of highly repetitive structures, e.g., peptides, this exercise is so simple as to be obvious. In more highly variegated structures the recognition of synthons is both difficult and more important. Here smaller molecular fragments are generated by disconnection of synthons, either directly, after the introduction of equivalent synthons, the introduction of control synthons, or the introduction of rearranged synthons. The synthesis of methyl t-butyl ketone (pinacolone) provides a simple example of the value of considering rearrangement; the synthesis of 1,3-butanediol shows the value of considering equivalent synthons, since this leads to the intermediates ß-hydroxy *n*-butyraldehyde and acetaldehyde. The use of cyclic intermediates to produce difunctionality is another form of the generation of equivalent synthons, e.g., the change of a cyclohexene to a Δ^1-cyclopentenecarboxaldehyde via the 1,6-hexanedial system. Protecting groups and temporary rings are useful control synthons. The latter often serve to facilitate the introduction of functional groups, molecular fragments, or centres of asymmetry. From these considerations, and others which need no elaboration, it can be seen that the generation of a set of synthetic intermediates for an acyclic target molecule and the design of a synthesis include the following:

1. Simplification of problem.
2. Systematic recognition of synthons.
3. Generation of equivalent and modified synthons.
4. Addition of control synthons.
5. Systematic disconnection of synthons.
6. Formulation of the possible synthetic transformations which reform the starting structure from the derived intermediate(s).
7. Repetition of items 1-6 for each intermediate and each sequence (parallel sequences may be generated), including previously generated intermediates.
8. Generation of intermediates until the required starting point is reached.
9. Removal of inconsistencies.
10. Identification of unresolved problems.
11. Repetition of items 1-10 to generate alternative schemes.
12. Assignment of merit.

[…]

This list of items 1-12 in the set of general instructions given above bears a vague resemblance to a computer programme; its resemblance to processes by which many of the well-known syntheses of today have been conceived is presumably much better, although many more steps of analysis are implied by 1–12 than have customarily been used. Furthermore, in many successful syntheses, one can be assured, not all of the intermediates were specified in detail beforehand, if only because the chemical situations involved in the syntheses were at some stage too complex to allow clear predictions to be made. In such cases the general outline of a synthesis serves to lend a sense of direction, and the assumption is made that the experimental results will illuminate the fine detail sufficiently to guide the synthesis through the region of uncertainty, a situation not unlike the process of climbing a mountain or traversing a wilderness without benefit of map or trail. Some of the greatest syntheses certainly were accomplished by skilled practitioners working in this venturesome vein. It is probably safe to assume that in all syntheses there is some interaction of this character between the elements of planning and of experimental execution.

The synthetic chemist is more than a logician and strategist; he is an explorer strongly influenced to speculate, to imagine, and even to create. These added elements provide the touch of artistry which can hardly be included in a cataloguing of the basic principles of Synthesis, but they are very real and extremely important. Further, it must be emphasized that intellectual processes such as the recognition and use of synthons require considerable ability and knowledge; here, too, genius and originality find ample opportunity for expression.

The proposition can be advanced that many of the most distinguished synthetic studies have entailed a balance between two different research philosophies, one embodying the ideal of a deductive analysis based on known methodology and current theory, and the other emphasizing innovation and even speculation. The appeal of a problem in synthesis and its attractiveness can be expected to reach a level out of all proportion to practical considerations whenever it presents a clear challenge to the creativity, originality and imagination of the expert in synthesis.

XX.

This celebrated narrative of a Balinese cockfight is, in part, the story of how Clifford Geertz (1926–2006) and his wife became 'real' for the villagers they visited. Geertz initially presents a faintly comic problem: they cannot do much meaningful fieldwork because they are not even recognised by the villagers, and are regarded as more similar to trees or stones than to other members of the village. This changes through the Geertzes' participation in the cockfight, not directly as a result of their joining in with the crowd watching the fight itself, but because like all the other villagers they run when the authorities come, rather than showing their papers and announcing themselves as outsiders.

Throughout the narrative, Geertz is careful to situate the cockfight within a Balinese society which has already been heavily studied by other anthropologists, and which is changing rapidly as a result of independence. He juxtaposes his own observations of the cockfight with previous works of ethnography, but he also uses the fights to challenge presumptions about what previous anthropologists have considered worthy of observation. He notes that previous descriptions of these fights lacked the concrete depth and dimensionalities that we see evidenced in his account of how the Geertzes come, almost accidentally, to be accepted by the village.

Presenting yourself as a part of village life is not however an idle addition to Geertz's account. Anthropologists, unlike most other social scientists, have always needed to show that they were 'present in the field' as first person witnesses. While early anthropologists came as white 'outsiders', often as colonialists, to witness the life of 'others', following WWII, the professional ethos changed so that they were required to be seen as 'insiders', participants in their communities of study. The Geertzes' reaction, and his writing the event into a community-based narrative, is typical of the way in which anthropologists came to evidence their professional credentials.

This initial dramatic scene is then used by Geertz as a way of framing his accounts of 'deep play' in Balinese life—specifically by asking why cock-fighting is so endemic, and betting so dangerously high. These two related questions set up a series of puzzles about money, wealth, kin and the extent of familial relations, village rivalries, and rituals, for Geertz to unravel. Each of these puzzles is portrayed and analysed through a series of mini-narratives of cock-fighting and betting, each being used to demonstrate Geertz's deep study of values in the culture of the Balinese community.

Geertz's approach in this paper exemplifies his method of "thick description" (theory- or concept-laden description—see Geertz 1973), which fits easily into narrative form because it requires the anthropologist to naturalise their observations into local practices and contexts of meaning using microscopic levels of detail. For Geertz, thick description meant not just methods of careful and exhaustive observation, but using theories and concepts in creative interpretation close to the evidence, something perhaps like a deep form of clinical inference. The point of these thick descriptions for the anthropologist is to generalise only within the case to understand this society—not those beyond it.

Mat Paskins and Mary S. Morgan

[See page 187 for reference.]

1972: CLIFFORD GEERTZ, Deep Play: Notes on the Balinese Cockfight *Daedalus* 101.1 pp. 1–37

[EXTRACT: pp. 1–5]

We moved into an extended family compound (that had been arranged before through the provincial government) belonging to one of the four major factions in village life. But except for our landlord and the village chief, whose cousin and brother-in-law he was, everyone ignored us in a way only a Balinese can do. As we wandered around, uncertain, wistful, eager to please, people seemed to look right through us with a gaze focused several yards behind us on some more actual stone or tree. Almost nobody greeted us; but nobody scowled or said anything unpleasant to us either, which would have been almost as satisfactory. If we ventured to approach someone (something one is powerfully inhibited from doing in such an atmosphere), he moved, negligently but definitively, away. If, seated or leaning against a wall, we had him trapped, he said nothing at all, or mumbled what for the Balinese is the ultimate nonword—"yes". The indifference, of course, was studied; the villagers were watching every move we made and they had an enormous amount of quite accurate information about who we were and what we were going to be doing. But they acted as if we simply did not exist, which, in fact, as this behavior was designed to inform us, we did not, or anyway not yet.

This is, as I say, general in Bali. Everywhere else I have been in Indonesia, and more latterly in Morocco, when I have gone into a new village people have poured out from all sides to take a very close look at me, and, often, an all-too-probing feel as well. In Balinese villages, at least those away from the tourist circuit, nothing happens at all. People go on pounding, chatting, making offerings, staring into space, carrying baskets about while one drifts around feeling vaguely disembodied. And the same thing is true on the individual level. When you first meet a Balinese, he seems virtually not to relate to you at all: he is, in the term

Gregory Bateson and Margaret Mead made famous, 'away.' Then—in a day, a week, a month (with some people the magic moment never comes)—he decides, for reasons I have never been quite able to fathom, that you *are* real, and then he becomes a warm, gay, sensitive, sympathetic, though, being Balinese, always precisely controlled person. You have crossed, somehow, some moral or metaphysical shadow line. Though you are not exactly taken as a Balinese (one has to be born to that), you are at least regarded as a human being rather than a cloud or a gust of wind. The whole complexion of your relationship dramatically changes to, in the majority of cases, a gentle, almost affectionate one—a low-keyed, rather playful, rather mannered, rather bemused geniality.

My wife and I were still very much in the gust of wind stage, a most frustrating, and even, as you soon begin to doubt whether you are really real after all, unnerving one, when, ten days or so after our arrival, a large cockfight was held in the public square to raise money for a new school.

Now, a few special occasions aside, cockfights are illegal in Bali under the Republic (as, for not altogether unrelated reasons, they were under the Dutch), largely as a result of the pretensions to puritanism radical nationalism tends to bring with it. The elite, which is not itself so very puritan, worries about the poor, ignorant peasant gambling all his money away, about what foreigners will think, about the waste of time better devoted to building up the country. It sees cockfighting as 'primitive,' 'backward,' 'unprogressive,' and generally unbecoming an ambitious nation. And, as with those other embarrassments—opium smoking, begging or uncovered breasts—it seeks, rather unsystematically, to put a stop to it.

[…]

[…] In the midst of the third match, with hundreds of people, including, still transparent, myself and my wife, fused into a single body around the ring, a superorganism in the literal sense, a truck full of policemen armed with machine guns roared up. Amid great screeching cries of "pulisi! pulisi!" from the crowd, the policemen jumped out, and, springing into the center of the ring, began to swing their guns around like gangsters in a motion picture, though not going so far as actually to fire them. The superorganism came instantly apart as its components scattered in all directions. People raced down the road, disappeared head first over walls, scrambled under platforms, folded themselves behind wicker screens, scuttled up coconut trees. Cocks armed with steel spurs sharp enough to cut off a finger or run a hole through a foot were running wildly around. Everything was dust and panic.

On the established anthropological principle, When in Rome, my wife and I decided, only slightly less instantaneously than everyone else, that the thing to do was run too. […]

[…]

The next morning the village was a completely different world for us. Not only were we no longer invisible, we were suddenly the center of all attention, the object of a great outpouring of warmth, interest, and, most especially, amusement. Everyone in the village knew we had fled like everyone else. They asked us about it again and again (I must have told the story, small detail by small detail, fifty times by the end of the day), gently, affectionately, but quite insistently teasing us: “Why didn't you just stand there and tell the police who you were?” “Why didn't you just say you were only watching and not betting?” “Were you really afraid of those little guns?” As always, kinesthetically minded and, even when fleeing for their lives (or, as happened eight years later, surrendering them), the world's most poised people, they gleefully mimicked, also over and over again, our graceless style of running and what they claimed were our panic-stricken facial expressions. But above all, everyone was extremely pleased and even more surprised that we had not simply “pulled out our papers” (they knew about those too) and asserted our Distinguished Visitor status, but had instead demonstrated our solidarity with what were now our covillagers. (What we had actually demonstrated was our cowardice, but there is fellowship in that too.) […]

In Bali, to be teased is to be accepted. It was the turning point so far as our relationship to the community was concerned, and we were quite literally ‘in.’ The whole village opened up to us, probably more than it ever would have otherwise […], and certainly very much faster. Getting caught, or almost caught, in a vice raid is perhaps not a very generalizable recipe for achieving that mysterious necessity of anthropological field work, rapport, but for me it worked very well. It led to a sudden and unusually complete acceptance into a society extremely difficult for outsiders to penetrate. It gave me the kind of immediate, inside-view grasp of an aspect of ‘peasant mentality’ that anthropologists not fortunate enough to flee headlong with their subjects from armed authorities normally do not get. And, perhaps most important of all, for the other things might have come in other ways, it put me very quickly on to a combination [of] emotional explosion, status war, and philosophical drama of central significance to the society whose inner nature I desired to understand. By the time I left I had spent about as much time looking into cockfights as into witchcraft, irrigation, caste, or marriage.

XXI.

In the latter half of the twentieth century, the principles of organic synthesis were often presented as having been reduced to an underlying logic (see **XIX.**). At the same time, as we see in narrative **XXVI.** of this anthology, synthetic chemists continued to insist upon the creativity and contingency of their field, and the impossibility of seeing a process of synthesis as merely the application of some logical principles.

This extract from a paper by the German chemist, Ivan Ernest (1922–2003), indicates an attempt to narrate the creative atmosphere and complex decision-making, as well as the great difficulties and wrong-turnings, which were involved in the synthesis of prostaglandin, which had been accomplished by Robert Woodward's group at Basel (Lindberg 1984 p. xiii). Ernest remarks that he is trying to provide "a complete and unprotected insight into the processes involved in designing and developing a 'classic' synthesis."

In the opening paragraphs, Ernest describes the decisions which the group made about how to select the target molecule for synthesis. He explains that it could not be the simplest of the prostaglandins, but rather needed to be capable of functioning as a 'key', from which many variants could be produced – this decision was due to the need to examine the potential pharmacological action of the prostaglandins. Decisions about the features which the key molecule had to contain contributed to the complexity of the synthetic challenge, by indicating the structural features which the molecule had to possess. A synthetic route which only came close would not have been acceptable.

The following sections of the excerpt describe some initial thinking, and the final solution at which Woodward's group had arrived. The usual publication format for chemical syntheses would have been only to provide the final route; by also including the byways and frustrations in other parts of his text, Ernest is able to impress on his readers the great difficulty of synthetic work, but also the lessons which can be learned from experiments that do not lead directly to the desired outcome.

In his analysis of fictional narratives, Roland Barthes defines those hinge-points of a narrative where things could have gone differently as "cardinal functions"; at such points, Barthes (1977) claims, "an alternative—and hence a freedom of meaning—is possible". Similarly, far from reducing chemical synthesis to the operations of planning on paper, Ernest's narrative aims to show the importance of working through, and learning from, definite alternative possibilities in the laboratory.

Ernest's research narrative was cited by the chemist Thomas Lindberg in the volume of a collection of essays on *Strategies and Tactics in Organic Synthesis,* (1984) which he claimed to have assembled because "there were no books in which chemists described their syntheses in the way that Ernest did in his paper" (p. xiii). Lindberg called on his contributors to provide a "sincere and more or less complete account of the chronological development of ideas and experimentation which finally led to the solution of the problem" (ibid, p. xiii). The research narratives of the accounts, like Ernest's paper, seek to go beyond the usual "terse communications" published in journals, in order to describe the "blind alleys and dead ends that were encountered in a synthesis" (ibid p. xiii). Notably, the accounts in Lindberg's collection make mention of the inter-personal dynamics within the synthetic team, such as the contributions made by junior members of the group, so they become socially professional narratives as well. Such questions are not discussed in Ernest's paper, even though it is much more detailed than conventional synthetic reports.

Mat Paskins

[See page 187 for references.]

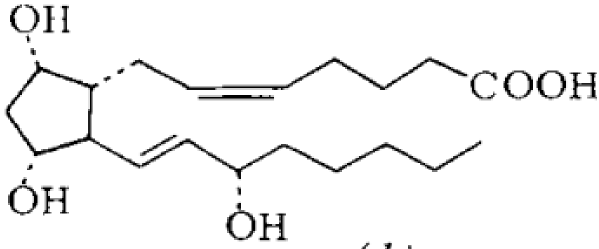
1976: Ivan Ernest, Eine Prostaglandin-Synthese: Strategie und Wirklichkeit *Angewandte Chemie* v.88 pp. 244–252

Die nüchterne und leidenschaftslose Art, in der heute wissenschaftliche Publikationen verfaßt werden, bietet leider oder – vielleicht auch zum Glück – nicht viel Gelegenheit, um die schőpferische Motivierung und Atmosphäre zu beschreiben oder auch nur anzudeuten, in der Ideen entstehen und sich weiter entwickeln. Um diese Lücke mit wenigstens einem Beispiel auszufüllen, und in der Hoffnung auf eine nützliche Wirkung einer solch ungewőhnlichen Darbietung, soll im vorliegenden Aufsatz ein vollständiger und unbeschőnigter Einblick in die Vorgänge des Entwerfens und Entwickelns einer „klassischen" Synthese gegeben werden. Für diesen Zweck wurde die Prostaglandin-Synthese ausgewählt, die vor einiger Zeit im Woodward Research Institute in Basel ausgeführt wurde. Diese Synthese, die bereits als vorläufige Mitteilung publiziert worden ist, wird hier nicht in der üblichen Form dargeboten, d.h., als ob jeder Schritt der endgültigen Fassung bereits von Anfang an vorgesehen gewesen wäre, sondern es wird aufrichtig und mehr oder weniger vollständig über die chronologische Entwicklung von Ideen und Experimenten berichtet, die schließlich zur Lősung des Problems führten.

Die Prostaglandine sind eine umfangreiche Klasse von Verbindungen mit zahlreichen vielversprechenden pharmazeutischen Aspekten; die für die einzelnen Vertreter so typische Vielfalt der physiologischen Wirkungen macht ihre medizinische Anwendung aber immer noch etwas problematisch und die Auswahl einer bestimmten Verbindung sehr schwierig.

Deshalb hatte eine neue Synthese allgemeingültig zu sein und müßte bei Bedarf die Herstellung *jedes* natürlichen Prostaglandins in großem Maßstab gestatten. Außerdem sollte die Synthese den Weg zu einer mőglichst großen Zahl von Analogen őffnen.

Zum Glück sind alle Prostaglandine strukturell eng miteinander verwandt, was die Aufgabe erleichtert, eine gemeinsame



$PGF_{2\alpha}$

(1)

Schlusselverbindung für ihre Syntheseabzuleiten. Bei der Wahl einer solchen Schlüsselverbindung geht man allerdings nicht von der Struktur des einfachsten, sondern vielmehr von derjenigen des kompliziertesten. Glieds der zu synthetisierenden Verbindungsgruppe aus; auf diese Weise ist im allgemeinen die Mőglichkeit der Synthese aller Gleider besser gewährleistet.

Formel *(1)* zeigt ein solch kompliziert gebautes Prostaglandin, nämlich Prostaglandin $F_{2\alpha}$ (abgekürzt $PGF_{2\alpha}$). Diese Verbindung ist nicht nur eines der vom physiologischen und medizinischen Standpunkt aus interessantesten und am besten untersuchten Mitglieder der Gruppe, sondern sie bietet dem synthetisch arbeitenden Chemiker auch eine lohnende Aufgabe, da sie in sich die meisten der Strukturelemente vereinigt, die in einer Prostaglandin-Synthese überhaupt berücksichtigt werden müssen. Tatsächlich wurde gezeigt, daß sich, sobald ein Syntheseweg zu dieser Verbindung gefunden worden war, auch ein Weg zu den anderen Prostaglandinen auftat.

Die synthetischen Probleme der Struktur von $PGF_{2\alpha}$ liegen auf der Hand. Erstens besitzt es fünf Chiralitätszentren, von denen sich vier im Cyclopentanring befinden, während das verbleibende Chiralitätszentrum Bestandteil einer sekundären Allylalkohol-Gruppierung ist. Weiterhin erkennt man zwei zueinander *trans*-orientierte aliphatische Seitenketten mit jeweils einer Doppelbindung. Eine ist *cis*-disubstituiert und isoliert und befindet sich in der Kohlenwasserstoffkette einer Fettsäure, die andere ist *trans*-disubstituiert und Bestandteil des Allylsystems der anderen Kette. Die allylische Hydroxylgruppe ist eine von insgesamt drei sekundären Hydroxylgrup pen; die beiden anderen haften, zueinander *cis*-orientiert, am fünfgliedrigen Ring.

Diese Anhäufung von relativ vielen stereochemischen Strukturmerkmalen in einem nicht allzu komplizierten und nur wenige funktionelle Gruppen tragenden Molekül sollte den grundlegenden Charakter unserer Synthese bestimmen: sie verlangte eine mőglichst stereospezifische Lősung.

[…]

So konnte nach knapp einem Jahr angestrengter Arbeit das Ziel, die Synthese von Prostaglandinen, erreicht werden. Wie dieser Bericht zeigt, war der Weg nicht immer gerade und eben. Jedoch die richtige Wahl der C_8-Verbindungen vom Typ *(2)* als Schlüsselverbindung, die in der Konzeption der Synthese liegende Flexibilität und die konsequente und schőpferische Anpassung der Pläne an neue experimentelle Erfahrungen machten schließlich die Synthese mőglich, ohne irgendwelche der angestrebten Gesichtspunkte zu opfern: eine hohe Stereospezifität, eine geringe Zahl von Syntheseschritten, das Vermeiden besonderer Schutzmaßnahmen, die Verwen-dung einfacher, leicht zugänglicher Ausgangsmaterialien und Reagentien sowie die Anwendung einfacher, leistungsfähiger und leicht ausführbarer Reaktionen.

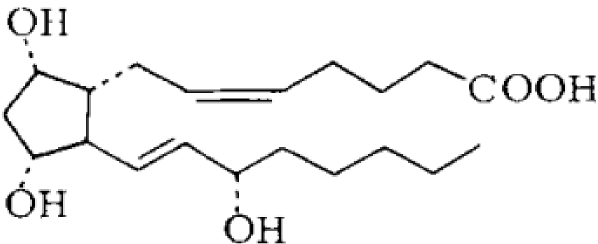
[TRANSLATION]

1976: Ivan Ernest, A Synthesis of Prostaglandins: Strategy and Reality *Angewandte Chemie* v.88 pp. 244-252

Unfortunately, or perhaps luckily, the sober and dispassionate way in which scientific publications are written today does not offer much opportunity to describe or even hint at the creative motivation and atmosphere in which ideas arise and develop. In order to fill this gap with at least one example, and in the hope of such an unusual performance having a useful effect, the present article provides a complete and unvarnished insight into the processes involved in designing and developing a "classic" synthesis. For this purpose, the prostaglandin synthesis that was carried out some time ago at the Woodward Research Institute in Basel was selected. This synthesis, which has already been published as a preliminary release, is not described here in the usual way, as if every step of the final version had been planned from the beginning, but it is reported honestly and more or less completely about the chronological development of ideas and experiments, which ultimately led to the solution of the problem.

The prostaglandins are an extensive class of compounds with numerous promising pharmaceutical aspects; however, the variety of physiological effects so typical of the individual representatives still makes their medical use somewhat problematic and the selection of a particular compound very difficult.

Therefore, a new synthesis had to be universal and, if necessary, had to allow the production of every natural prostaglandin on a large scale. In addition, the synthesis should open the way to the largest possible number of analogues.



PGF$_{2\alpha}$

(1)

Fortunately, all prostaglandins are structurally closely related, which makes the task of deriving a common key compound for their synthesis easier. When choosing such a key connection, however, one does not start from the structure of the simplest, but rather from that of the most complicated member of the connecting group to be synthesized; in this way the possibility of synthesis of all members is generally better guaranteed.

Formula *(1)* shows such a complicated prostaglandin, namely prostaglandin $F_{2\alpha}$ (abbreviated $PGF_{2\alpha}$). Not only is this compound one of the most interesting and best studied members of the group from a physiological and medical point of view, but it also offers the synthetic chemist a rewarding task as it combines most of the structural elements found in a prostaglandin. In fact, it was shown that as soon as a synthetic route to this compound was found, a route to the other prostaglandins also emerged.

The synthetic problems of the structure of $PGF_{2\alpha}$ are obvious. First, it has five chiral centers, four of which are in the cyclopentane ring, while the remaining chiral center is part of a secondary allyl alcohol group. Furthermore, two mutually *trans*-oriented aliphatic side chains can be seen, each with a double bond. One is *cis*-disubstituted and isolated and is located in the hydrocarbon chain of a fatty acid, the other is *trans*-disubstituted and part of the allyl system of the other chain. The allylic hydroxyl group is one of a total of three secondary hydroxyl groups; the other two cling, *cis*-oriented, to the five-membered ring.

This accumulation of a relatively large number of stereochemical structural features in a molecule that is not too complicated and has only a few functional groups should determine the basic character of our synthesis: it required the most stereospecific solution possible.

[…]

After almost a year of hard work, the goal of synthesizing prostaglandins was achieved. As this report shows, the path was not always straight and even. However, the right choice of the C_8 compounds of type *(2)* as a key connection, the flexibility in the conception of the synthesis and the consequent and creative adaptation of the plans to new experimental experiences finally made the synthesis possible without sacrificing any of the desired aspects: a high degree of stereospecificity, a small number of synthetic steps, the avoidance of special protective measures, the use of simple, easily accessible starting materials and reagents as well as the use of simple, efficient and easily executable reactions.

XXII.

The principle that oil and gas are formed by the action of heat and pressure on organic matter deposited in subsiding sedimentary basins was first grasped in the early twentieth century (Höök et al, 2010), since when it has guided the practice of petroleum exploration. Before this realisation however, hypotheses promoting an exclusively inorganic or abiotic origin for hydrocarbons were commonly advanced; Dimitry Mendeleyev (1834–1907) of periodic table fame, was one of the more prominent advocates of this view (ibid).

Abiotic hypotheses hold that hydrocarbons originate from deep within the Earth, so their occurrence is not restricted to sedimentary basins. A shortage of oil in the Soviet Union after the Second World War led to a revival of these hypotheses and research programmes investigating the abiotic genesis of hydrocarbons have persisted in Russia and Ukraine into the twenty-first century (Glasby 2006). British chemist, Sir Robert Robinson (1886-1975) published work on the concept of an abiotic origin in the 1960s, but the person who took the ideas furthest in the West was Cornell University astrophysicist Thomas Gold FRS (1920–2004). Working at a time when the impact of fossil fuel use on the climate was not widely acknowledged, Gold, like the Soviet scientists, was motivated to seek alternatives to the organic consensus by fears of impending oil shortages.

These excerpts summarise the main points of Gold's narrative argument for an abiotic origin of hydrocarbons, which he saw as complementary to the biological account. Gold constructed a narrative which began at the formation of the Earth, and he introduced it with a counterfactual question: "What if" oil and gas were not limited to biological deposits? In recounting the accepted origin of the planets of the solar system by the accretion of "asteroid-sized planetesimals", Gold drew on the well-attested occurrence of hydrocarbons (largely methane) in small asteroids which have fallen to Earth more recently as meteorites to invoke their presence as unoxidised fluids in the inhomogeneous "patchwork" of the interior of the primordial Earth. Here, Gold maintained, they became sufficiently concentrated by diffusion to enable them to force their way from the mantle towards the surface through interconnected fractures, driven by their own buoyancy, a narrative reminiscent of Johnston's "breathing" Earth in **XI.**

In 1986, on the basis of his argument, Gold persuaded the Swedish government to sponsor the drilling of two ultra-deep test boreholes into ancient granitic basement, far from any sedimentary basins. The chosen site had been struck by a large meteorite in the remote past creating deep fractures – a necessary element in Gold's narrative. The drilling programme encountered small quantities of methane, some of which was acknowledged to be possibly abiogenic, as well as 80 barrels of what was described as "black gunk" (Glasby 2006). Gold contended that this was abiotic oil which had been bacterially modified, but others argued that it was merely an artefact of the drilling process (ibid). The outcome was ultimately inconclusive, and narratives of the formation of commercially viable accumulations of oil or gas from abiotic generation have not so far been widely accepted, at least in the West.

Andrew Hopkins

[See page 187 for references.]

1985: THOMAS GOLD, The Origin of Natural Gas and Petroleum, and the Prognosis for Future Supplies *Annual Review of Energy* 10 pp. 53–77

[EXTRACTS: pp. 53–71]

Any estimation of future supplies of hydrocarbons—oil and gas—is necessarily bound up with the question of the origin of these deposits. The theory of a biological origin, which has become accepted almost universally, would place fairly sharp limits on what the earth might provide. The total volume of sediments, the proportion of organic matter they may contain, and the fraction of this that may have been converted into accessible hydrocarbons have all been estimated. So long as many areas of sediments on the earth remain unexplored by the drill, a prognosis based on such evaluations can be a rough judgment at best. Still, it will give some indication of what can be expected, and it is not likely to be in error by orders of magnitude.

In the early 1970s, estimates based on these considerations had indicated that oil was running out—that the earth could provide for no more than 20 to 40 years' supply at the present rate of consumption. These estimates were widely accepted, resulting in drastic increases in the price of oil. It was the estimates, and not an actual shortage, that prompted these changes, with their profound effects on the world economy.

[...]

But what if the source material for oil and gas were not limited to organic deposits, but instead these substances derived wholly or in part from materials incorporated deep in the earth at the time of its formation? This possibility has been considered in the past, and again in recent times with modern data. In that case one would make quite different and very much more optimistic estimates of future supplies. But is there any possibility of that? Is anything coming up from deep sources in the earth?

There is little doubt that large quantities of certain fluids—the so - called “excess volatiles”—have made their way up through the crust over geologic time. The water of the oceans, the nitrogen of the atmosphere, and the large quantities of carbon now resident in the carbonate rocks of the sediments must have been supplied in that way. The erosion of the basement rocks that produced the other components of the sediments would have provided only a very small fraction of these substances.

[...]

The usual assumption seems to have been that all this carbon came up from its original deep sources already in oxidized form, namely as CO_2, There is no compelling reason for such an assumption, and there are several good reasons for considering an alternative possibility, namely that methane or other hydrocarbon fluids were involved. In the oxidizing circumstances of the outermost crust and the atmosphere, most of the carbon would end up as atmospheric carbon dioxide in any case, and the subsequent deposition process of the carbonate rocks would be the same. What would be different is the identification of the unoxidized forms of carbon that we find in the outer crust and in the sediments.

When it was thought that the supply of carbon had all been in the form of CO_2, any unoxidized carbon had to be considered the result of photosynthesis in plants, in which sunlight had supplied the energy necessary for the dissociation of CO_2, The burning of such fuels, then, constituted the regaining of this “fossil solar energy.” But if the primary carbon had come up from depth in the form of methane or other hydrocarbon fluids, the situation would be quite different. A small fraction of this, arrested on the way up and contained at accessible levels in the outer crust, could constitute an enormously larger energy resource than all the biological deposits. The methods of prospecting for oil and gas would then be quite different, and many new locations would come under consideration, which would have been excluded if one was looking for biological deposits only.

[…]

It has become quite clear that the earth, as well as the other terrestrial planets, accreted as solid bodies from solids that had condensed from a gaseous planetary disk. The primary condensates, ranging in size from small grains to asteroid-sized “planetesimals,” all contributed to the formation of the final earth. An accretion from small grains only, could have led to a final body that was inhomogeneous with depth, since at successive epochs different materials might have been acquired. However, when such an accretion process was punctuated by major impacts of competing bodies that had formed on collision orbits, then an orderly, layered composition would be turned into an erratic patchwork. This appears to have been the case in the formation of the earth, just as it was in the formation of all other solid planetary bodies, demonstrated by the ubiquity of impact craters on them.

There is now a great deal of evidence from trace elements and their isotopes that have come up from depth, in various parts of the earth, that the mantle is indeed quite inhomogeneous and its chemical composition patchy, confirming that it has never been all molten. What melting has taken place seems to have been only a small proportion at any one time, but since this partial melting resulted in outpourings that formed the entire outer crust, it produced the impression of an earth cooled from liquid.

Another part of the discussion, where the outlook in earlier times was quite different from that of today, concerned the derivation of hydrocarbons in the solar system. Hydrocarbons used to be thought of as substances that were specifically biological. Methane and other hydrocarbons were clearly the results of biological processes, and there was little indication that these molecules would be produced in other ways. In fact, when methane was first detected in the atmosphere of Jupiter, this resulted in publications suggesting that some form of life on Jupiter must be responsible.

Now we know not only that there is methane in the atmosphere of Jupiter, but that by far the largest proportion of the carbon in the entire planetary system is in the form of hydrocarbons. The greatest quantity is in the massive outer planets and their satellites. Jupiter, Saturn, Uranus, and Neptune have large admixtures of carbon in their extensive atmospheres, chiefly in the form of hydrocarbons – mainly methane. Titan, the satellite of Saturn, has methane and ethane (CH_4 and C_2H_6) in its atmosphere. Its clouds are composed of these substances, and it is likely that liquid methane-ethane mixes are on the surface below, and perhaps make up oceans, rivers, and frozen polar caps, more or less as water does on the earth. The meteorites are considered to provide us samples of the materials from which the planets formed. One class—the carbonaceous chondrites contain some volatile substances, and it is this class that is thought to have supplied the earth with most of its complement of volatiles. While carbon is a minor constituent of the other types of meteorites, it is present at a level of several percent in the carbonaceous chondrites, mostly in unoxidized form, with a certain fraction in the form of hydrocarbon compounds.

[…]

Diffusion of fluids from deep levels through the rocks of the crust is too slow to be important, even on the long time scale of geology. Only a bulk motion through cracks could provide a significant flow through the solid rocks. Transport from deep levels to the surface must therefore occur in two steps.

First, fluids must be assembled by diffusion over small distances into cracks, which are generated and held open by the fluid pressure. That, of course, is a process which must occur to initiate any form of outgassing. Volatile substances liberated under heat must separate out from the solids and build up pressure in the pores. If this process generated only small pores, diffusely distributed and separated from one another, no movement would result.

The second step in the upward transport process can only occur if a region contains a sufficiently concentrated source of fluids, so that the pores that are generated create an interconnected domain of fracture porosity (by a process of hydraulic fracturing, often called “hydrofracking” when done artificially). If such a fluid-filled domain grows to a sufficient size and volume, then buoyancy forces exerted on the light fluid in the denser rock will drive it upward. This would occur in the following way. The connected fluid-filled pores have a pressure gradient in them given by their density; the denser rock must have a higher pressure gradient in it over the same height interval. When such a porosity domain has a sufficiently large vertical extent, the pressure difference between fluid and rock will become so large that the strength of the rock is insufficient to withstand it. The fluid pressure will then become excessive at the top of the domain and force open new pore spaces, while at the same time it will be insufficient at the bottom of the domain to hold the pores open, against the greater rock pressure. In this way the fluids will force their way upward through the largely stationary rock.

[…]

It is the shallow domain where oil is found that has formed the basis of conventional estimates of future supplies. If, however, gas has been streaming up from deep levels, then the type of reservoir where an expanded porosity has been built up underneath the first critical layer can be expected to constitute a major global resource. Instead of finding this an anomaly, in some special circumstances, it would now be the expectation that in all petroliferous areas, and in many such areas not yet identified, such reservoirs may exist. […]

XXIII.

William Thurston (1946–2012) was described in his obituary as a "geometric visionary and one of the greatest mathematicians of the twentieth century" (Gabai and Kerckhoff 2012). His work was "playful, ever curious, near magical and sometimes messy."

This excerpt comes from a paper in which Thurston responds to an article, by the theoretical physicists Jaffe and Quinn, entitled "'Theoretical mathematics': toward a cultural synthesis of mathematics and theoretical physics". Noting that the role of speculation is more prized among theoretical physicists than among mathematicians, Jaffe and Quinn "propose a framework that should allow a healthy and positive role for speculation." In passing, they describe one part of Thurston's work as an example of how not to speculate: it is "a grand insight delivered with beautiful but insufficient hints", which was never published in full and "became a roadblock rather than an inspiration" (Jaffe and Quinn 1993, p. 8).

Thurston does not respond by defending the foundations of his mathematical reasoning; instead, he tells a story about how mathematics develops, which involves the partial formalisation of intuitive concepts, and deliberate decisions about how to grow a mathematical subfield. This excerpt is about the social dynamics of mathematical research communities, and how these conduce to the establishment of new topics of interest—or fail to do so. Thurston begins with the "theory of foliations", noting that the reader does not need to know anything about this theory to follow his argument, but at the same time he notes how the papers he wrote "depended heavily on readers who shared certain background and certain insights", which were not standardized, and this made it difficult for others to keep up. He later adopted a different approach (the one which Jaffe and Quinn criticised), focused on "building the infra-structure and explaining and publish-ing definitions and ways of thinking but being slow in stating or in publishing proofs." In so doing, "[he] left room for many other people to pick up credit," and so the topic remained of interest.

Thurston's narrative is not, perhaps, to be taken at face value: it might be queried by some future historian of mathematics. But the reason for its inclusion in this Anthology, is that it gives an account of Thurston's deliberate attitude towards the likely actions of other mathematicians, especially those in training. Conscious of his difference from them—that many of his own mental models were idiosyncratic—he actively considered the ways in which the community of mathematicians could be thought of as an "ecology", which needed to be tended and developed.

The narratologist Marie Laure Ryan has argued that a crucial aspect which makes narratives compelling and "tellable" is that they do not just recite actions, but also involve the intersections of different characters' plans and intentions which impact the course of events, even when they are not stated explicitly in a narrative. Mapping different characters' plans also becomes remarkably complex, as she argues, because it involves paying attention to their inferences about one another's intentions—characters in narratives make their own narratives about each other's plans, and respond accordingly. Rather than appealing to an abstract ideal of rigour, Thurston's narrative is "tellable" in a very similar sense.

Colin McSwiggen

[See page 188 for references.]

1994: WILLIAM P. THURSTON, On proof and progress in mathematics. *Bull. Amer. Math. Soc.* v.30(2) pp. 161–177

[EXTRACT: pp. 173–176]

6. Some Personal Experiences

[...]

First I will discuss briefly the theory of foliations, which was my first subject, starting when I was a graduate student. (It doesn't matter here whether you know what foliations are.)

At that time, foliations had become a big center of attention among geometric topologists, dynamical systems people, and differential geometers. I fairly rapidly proved some dramatic theorems. I proved a classification theorem for foliations, giving a necessary and sufficient condition for a manifold to admit a foliation. I proved a number of other significant theorems. I wrote respectable papers and published at least the most important theorems. It was hard to find the time to write to keep up with what I could prove, and I built up a backlog.

An interesting phenomenon occurred. Within a couple of years, a dramatic evacuation of the field started to take place. I heard from a number of mathematicians that they were giving or receiving advice not to go into foliations—they were saying that Thurston was cleaning it out. People told me (not as a complaint, but as a compliment) that I was killing the field. Graduate students stopped studying foliations, and fairly soon, I turned to other interests as well.

I do not think that the evacuation occurred because the territory was intellectually exhausted—there were (and still are) many interesting questions that remain and that are probably approachable. Since those years, there have been interesting developments carried out by the few people who stayed in the field or who entered the field, and there have also been important developments in neighboring areas that I think would have been much accelerated had mathematicians continued to pursue foliation theory vigorously.

Today, I think there are few mathematicians who understand anything approaching the state of the art of foliations as it lived at that time, although there are some parts of the theory of foliations, including developments since that time, that are still thriving.

I believe that two ecological effects were much more important in putting a damper on the subject than any exhaustion of intellectual resources that occurred.

First, the results I proved (as well as some important results of other people) were documented in a conventional, formidable mathematician's style. They depended heavily on readers who shared certain background and certain insights. The theory of foliations was a young, opportunistic subfield, and the background was not standardized. I did not hesitate to draw on any of the mathematics I had learned from others. The papers I wrote did not (and could not) spend much time explaining the background culture. They documented top-level reasoning and conclusions that I often had achieved after much reflection and effort. I also threw out prize cryptic tidbits of insight, such as "the Godbillon-Vey invariant measures the helical wobble of a foliation", that remained mysterious to most mathematicians who read them. This created a high entry barrier: I think many graduate students and mathematicians were discouraged that it was hard to learn and understand the proofs of key theorems.

Second is the issue of what is in it for other people in the subfield. When I started working on foliations, I had the conception that what people wanted was to know the answers. I thought that what they sought was a collection of powerful proven theorems that might be applied to answer further mathematical questions. But that's only one part of the story. More than the knowledge, people want personal understanding. And in our credit-driven system, they also want and need theorem-credits.

I'll skip ahead a few years, to the subject that Jaffe and Quinn alluded to, when I began studying 3-dimensional manifolds and their relationship to hyperbolic geometry. (Again, it matters little if you know what this is about.) I gradually built up over a number of years a certain intuition for hyperbolic three-manifolds, with a repertoire of constructions, examples and proofs. (This process actually started when I was an under-graduate, and was strongly bolstered by applications to foliations.) After a while, I conjectured or speculated that all three-manifolds have a certain geometric structure; this conjecture eventually became known as the geometrization conjecture. About two or three years later, I proved the geometrization theorem for Haken manifolds. It was a hard theorem, and I spent a tremendous amount of effort thinking about it. When I completed the proof, I spent a lot more effort checking the proof, searching for difficulties and testing it against independent information.

I'd like to spell out more what I mean when I say I proved this theorem. It meant that I had a clear and complete flow of ideas, including details, that withstood a great deal of scrutiny by myself and by others. Mathematicians have many different styles of thought. My style is not one of making broad sweeping but careless generalities, which are merely hints or inspirations: I make clear mental models, and I think things through. My proofs have turned out to be quite reliable. I have not had trouble backing up claims or producing details for things I have proven. I am good in detecting flaws in my own reasoning as well as in the reasoning of others.

[…]

Neither the geometrization conjecture nor its proof for Haken manifolds was in the path of any group of mathematicians at the time—it went against the trends in topology for the preceding 30 years, and it took people by surprise. To most topologists at the time, hyperbolic geometry was an arcane side branch of mathematics, although there were other groups of mathematic-ians such as differential geometers who did understand it from certain points of view. It took topologists a while just to under-stand what the geometrization conjecture meant, what it was good for, and why it was relevant.

At the same time, I started writing notes on the geometry and topology of 3-manifolds, in conjunction with the graduate course I was teaching. I distributed them to a few people, and before long many others from around the world were writing for copies. The mailing list grew to about 1200 people to whom I was sending notes every couple of months. I tried to communicate my real thoughts in these notes. People ran many seminars based on my notes, and I got lots of feedback. Overwhelmingly, the feedback ran something like "Your notes are really inspiring and beautiful, but I have to tell you that we spent 3 weeks in our seminar working out the details of §$n.n$. More explanation would sure help."

I also gave many presentations to groups of mathematicians about the ideas of studying 3-manifolds from the point of view of geometry, and about the proof of the geometrization conjecture for Haken manifolds. At the beginning, this subject was foreign to almost everyone. It was hard to communicate – the infrastructure was in my head, not in the mathematical community.

[…]

We held an AMS summer workshop at Bowdoin in 1980, where many mathematicans in the subfields of low-dimensional topology, dynamical systems and Kleinian groups came.

It was an interesting experience exchanging cultures. It became dramatically clear how much proofs depend on the audience. We prove things in a social context and address them to a certain audience. Parts of this proof I could communicate in two minutes to the topologists, but the analysts would need an hour lecture before they would begin to understand it. Similarly, there were some things that could be said in two minutes to the analysts that would take an hour before the topologists would begin to get it. And there were many other parts of the proof which should take two minutes in the abstract, but that none of the audience at the time had the mental infrastructure to get in less than an hour.

At that time, there was practically no infrastructure and practically no context for this theorem, so the expansion from how an idea was keyed in my head to what I had to say to get it across, not to mention how much energy the audience had to devote to understand it, was very dramatic.

In reaction to my experience with foliations and in response to social pressures, I concentrated most of my attention on developing and presenting the infrastructure in what I wrote and in what I talked to people about. I explained the details to the few people who were "up" for it. I wrote some papers giving the substantive parts of the proof of the geometrization theorem for Haken manifolds—for these papers, I got almost no feedback. Similarly, few people actually worked through the harder and deeper sections of my notes until much later.

The result has been that now quite a number of mathematicians have what was dramatically lacking in the beginning: a working understanding of the concepts and the infrastructure that are natural for this subject. [...] By concentrating on building the infrastructure and explaining and publishing definitions and ways of thinking but being slow in stating or in publishing proofs of all the "theorems" I knew how to prove, I left room for many other people to pick up credit.

[...]

What mathematicians most wanted and needed from me was to learn my ways of thinking, and not in fact to learn my proof of the geometrization conjecture for Haken manifolds. It is unlikely that the proof of the general geometrization conjecture will consist of pushing the same proof further.

XXIV.

Daniel Pauly (1946–) has been called “the world’s most prolific and widely cited living fisheries scientist” (Malakoff 2002). Much of his work has been directly concerned with the impacts of fishing, and the problems arising from the depletion of fish stocks. In this article, he reflects on a problem in fisheries research which has much wider social and ecological implications, and which he calls the “shifting baseline syndrome”. The syndrome might be characterised as the fear that people lose a sense of what they have lost, and diminish their expectations of what was possible in the past, and might again become possible in the future, accordingly.

Fisheries scientists tend to adjust their expectations about what is normal in line with what they have witnessed, and they set baselines for conservation on this basis. The solution which Pauly proposes concerns the use of oral testimony and written evidence of past experience and practice—especially where these concern a lost abundance—to give a more accurate estimate of former resources. Unlike disciplines such as astronomy and oceanography, fisheries research had in 1995 no formal way of drawing on its history, and such knowledge had been considered as primarily anecdotal in character. Anecdotes were to be found in the reports of fishermen, accounts by anthropologists, and in books such as *Sea of Slaughter* by Canadian environmentalist Farley Mowat (1984).

The views of these reports point towards far larger historical populations (and to a much greater role of the fishing activities of women), than fisheries researchers had previously been willing to contemplate. Such field narratives and anecdotes resemble those of Bartram (**II.**) and Romanes (**XXII.**) in that they contain information which has not been produced as part of, and which can be considered as having been excluded from, prevailing scientific discourses. This knowledge is also, however, sufficiently suggestive that it can be woven together to provide the basis of novel scientific narratives, and to open up new avenues of research.

Pauly’s concept has been widely cited by academic ecologists, and the study of fisheries and other marine sciences has taken on an increasingly historical character since around the turn of the millennium. In a large-scale interdisciplinary review involving “historians, archaeologists, economists, sociologists and geographers”, Manez et al. (2014) claim that “thanks to this collaborative effort of marine and human sciences, researchers have not only identified but, for many regions and species, resolved the problem of shifting baselines…by pushing back the chronological limits of our knowledge” They note, however, that many unanswered problems persist in constructing a general history of the oceans.

The concept of shifting baseline syndrome has found wide application in the environmental sciences, as well as among some social psychologists in discussing changes in the social environment. Establishing the syndrome often involves capturing past experiential knowledge in narrative evidence, and raises the question of how to elicit and record testimony which can capture something of those aspects of the world which are at risk of being lost.

Mat Paskins

[See page 188 for references.]

1995: DANIEL PAULY, Anecdotes and the shifting baseline syndrome of fisheries *Trends in Ecology & Evolution* vol. 10:10 p. 430

Fisheries have recently become a topic for media with global audiences—but then again, fisheries are a global disaster: one of the few that affect, in very similar fashion, developed countries with well-established administrative and scientific infrastructure, newly industrialized countries, and developing countries.

This is quickly summarized:

- Heavily subsidized fleets, exceeding by a factor of 2 or 3 the numbers required to harvest nominal annual catches of about 90 million tonnes.
- Staggering levels of discarded bycatch, representing about one third of the nominal catch, a large unrecorded catch that perhaps raises the true global catch to about 150million tonnes per year, well past most previous estimates of global potential.
- The collapse, depletion or recovery from previous depletion of the overwhelming majority of the over 260 fish stocks that are monitored by the Food and Agriculture Organization of the United Nations.

Fisheries science has responded as well as it could to the challenge this poses by developing methods for estimating targets for management - earlier the fabled Maximum Sustainable Yield (MSY)[1], now annual total allowable catch (TAC) or individual transferable quotas (ITQ). If these methods are to remain effective, fisheries scientists need to follow closely the behavior of fishers and fleets, but this has tended increasingly to separate us from the biologists studying marine or freshwater organisms and/or communities, and to factor out ecological and evolutionary considerations from our models. There are obviously exceptions to this, but I believe the rule generally applies, and it can be illustrated by our lack of an explicit model accounting for what may be called the 'shifting baseline syndrome'.

Essentially, this syndrome has arisen because each generation of fisheries scientists accepts as a baseline the stock size and species composition that occurred at the beginning of their careers, and uses this to evaluate changes. When the next generation starts its career, the stocks have further declined, but it is the stocks at that time that serve as a new baseline. The result obviously is a gradual shift of the baseline, a gradual accommodation of the creeping disappearance of resource species, and inappropriate reference points for evaluating economic losses resulting from overfishing, or for identifying targets for rehabilitation measures.

These are strong claims that I can illustrate best by using analogies. For example, astronomy has a framework that uses ancient observations (including Sumerian and Chinese records that are thousands of years old) of sunspots, comets, supernovae or other phenomena that were recorded by ancient cultures, and this has made possible the testing of pertinent hypotheses. Similarly, oceanography has had, since the days of Commodore F. Maury, protocols for consolidating scattered observations on currents and winds, and later on sea surface temperatures; the latter have enabled the extending of the Comprehensive Ocean and Atmospheric Data Set (COADS) back to 1870, and infer that, indeed, global warming is occurring.

In contrast, fisheries science does not have formal approaches for dealing with early accounts of 'large catches' of presently extirpated resources, which are viewed as anecdotes. Yet the grandfather of my colleague Villy Christensen did report being annoyed by the bluefin tuna that entangled themselves in the mackerel nets he was setting in the waters of the Kattegat in the 192Os, and for which no market then existed. This observation is as factual as a temperature record, and one that should be of relevance to those dealing with bluefin tuna, whose range now excludes much, if not all, of the North Sea.

I could list hundreds of such observations—drawn from the historical or anthropological literature and elsewhere—but here it may be more useful to highlight two small fisheries-related studies that have attempted to consolidate them, and which have led, I believe, to important new insights. In the first, a (female) scientist[2] compiled scattered observations of (male) anthropologists reporting on fishing in the South Pacific, and concluded that, despite cultural emphasis on the catching of large fish by men, the gleaning of smaller reef organisms by women and children often accounted for as much catch as the more spectacular activities of the men (even though it does not enter official catch statistics). This fact, now widely confirmed by field studies, should lead to a re-evaluation of the fisheries potential of coral reefs.

The authors of the second study[3] used the anecdotes in Farley Mowat's *Sea of Slaughter*[4] to infer that the biomass of fish and other exploitable organisms along the North Atlantic coast of Canada now represents less than 10% of that two centuries ago. Some colleagues will find it difficult to accept that the early fishing methods should have had such impact, given their relative inefficiency when compared to our factory ships. However, it must be remembered that the large animals of low fecundity at the top of earlier food webs must have been less resilient to fishing than the survivors that are exploited today. That is, the big changes happened way back, but all that we have to recall them are anecdotes.

Developing frameworks for incorporation of earlier knowledge—which is what the anecdotes are—into the present models of fisheries scientists would not only have the effect of adding history to a discipline that has suffered from lack of historical reflection[1] but also of bringing into biodiversity debates an extremely speciose group of vertebrates: the fishes, whose ecology and evolution are as strongly impacted by human activities as the denizens of the tropical and other rain forests that presently occupy center stage in such debates. Frameworks that maximize the use of fisheries history would help us to understand and to overcome—in part at least—the shifting baselines syndrome, and hence to evaluate the true social and ecological costs of fisheries.

[1] Smith, T.D. 1994. *Scaling Fisheries*, Cambridge University Press
[2] Chapman, M.D. 1987. *Hum. Ecol.* 15,267-288
[3] Maclntyre, F. ,Estep, K.W. and Noji, T.T. 1995. *NACA the ICLARM Quarterly* B3, 7-8
[4] Mowat, F. 1984. *Sea of Slaughter*, Atlantic Monthly Press

XXV.

In these passages from an article in *The Mathematical Intelligencer*, the mathematician and computer scientist A.K. Dewdney (1941–), a long-time columnist for the *Scientific American*, recalls his experiences of writing a mathematical fiction about a two dimensional universe. Dewdney's *Planiverse* (1984) is one of a number of such fictions, the best known of which is perhaps Edwin Abbott's 1884 book *Flatland*. They provide a chance to develop thought experiments about how mathematics and physics would apply in universes with different geometries to our own.

Here, Dewdney presents the natural history of the *Planiverse* as a collaborative enterprise, which got somewhat out of control. Following attention from the press, and questions about whether he was claiming to study a real two dimensional universe, Dewdney obtained a book contract and decided to present his tale of the *Planiverse* as a quest narrative, as a figure named Yendred searches for the third dimension in his flat homeworld.

Dewdney talks here about the use of somewhat fantastical fictions in popular mathematics writing, and how *The Planiverse Project*'s collaborative ethos allowed participants to take some starting conditions and run with them, producing a world which is both consistent and determinedly strange. He contrasts this enterprise with works of science fiction which are "dream[ed] up"; to the extent that the *Planiverse* has a moral, it is found in Dewdney's insistence that even pseudo-reality is stranger than wishful fiction.

Dewdney's remarks about needing the quest narrative to bring together the disparate elements of *The Planiverse Project* echo a longstanding tension in fictions about imaginary societies. How is the account of such a society to be made dynamic, dramatically compelling, as opposed to a mere catalogue of disparate facts? The use of narrative gives a point of view to the action and allows the reader to discover the properties of the world alongside the characters who explore it.

Through its use of a toy universe, a deliberately simplified state of affairs intended for the examination of how physical concepts can play out over time, Dewdney's narrative also recalls the scientific use of simulations (Wise 2017). Like the *Planiverse*, simulations sometimes traverse the realms of scientific curiosity and entertainment. In the mid-1990s the computer scientist Steve Grand produced a series of video games based on artificial life, entitled *Creatures*, which drew explicit inspiration from Dewdney's *Planiverse* (Grand 1997).

Although Dewdney writes here in somewhat ironical terms about journalists' and readers' inclination to take his fiction literally as the discovery of a real two dimensional universe, his *Planiverse* has also been read as an allegory about Sufi mysticism, on the basis of Dewdney's own interest in this topic and his use of Arabic names within the *Planiverse*. This esoteric interpretation is consistent with the use of geometry to dramatize human longings for transcendence and the search for "multiple, invisible dimensions" (White 2018).

Mat Paskins

[See page 188 for references.]

2000: A. K. DEWDNEY, The Planiverse Project: Then and Now *The Mathematical Intelligencer* 22 pp. 46–51

Is a two-dimensional universe possible, at least in principle? What laws of physics might work in such a universe? Would life be possible? It was while pondering such imponderables one steamy summer afternoon in 1980 that I came to the sudden conclusion that, whether or not such a place exists, it would be possible to conduct a gedanken experiment on a grand scale. It was all a question of starting somewhat mathematically. With the right basic assumptions (which would function like axioms), what logical consequences might emerge?

[...] I pictured my toy universe as a balloon with an infinitesimal (that is to say, zero-thickness) skin. Within this skin, a space like ours but with one dimension less, there might be planets and stars, but they would have to be disks of two-dimensional matter. In laying out the basic picture I followed informal principles of simplicity and similarity. Other things being equal, a feature in the planiverse should be as much like its counterpart in our universe as possible, but not at the cost of simplicity within the two-dimensional realm. The simplest two-dimensional analog of a solid sphere is a disk.

What sort of orbits would the planets follow? In our own universe, Newtonian mechanics takes its particular form from the inverse-square law of attraction. A planet circling a star, for example, "feels" an attraction to that star which varies inversely with the square of the distance between the two objects. The same reason in the planiverse leads to a different conclusion. The amount of light that falls on a linear meter at a distance $2x$ from a star is one-half the light that reaches the square at a distance x from the star (see Figure 1); correspondingly, attraction is proportional to the inverse first power of the distance.

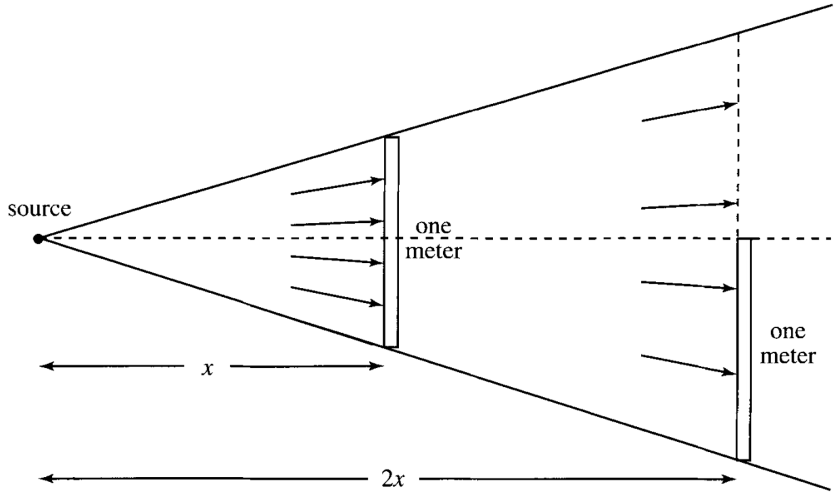


Figure 1. The law of gravity.

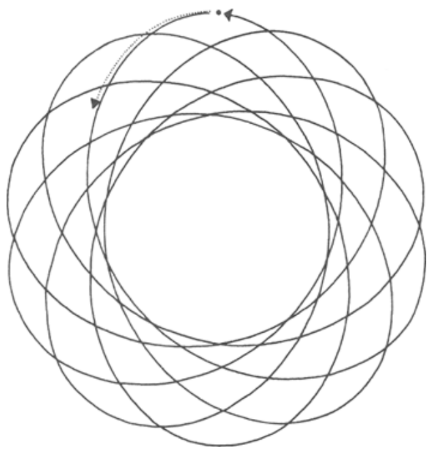


Figure 2. Orbit of a two-dimensional planet.

The resulting trajectory is not a conic section, but a wildly weaving orbit, as in Figure 2.

[…]

In a fit of scientific irresponsibility I sent a letter to Martin Gardner, then author of the Mathematical Games column for *Scientific American* magazine. […]

Gardner wrote back, saying that he not only found the planiverse a delightful place, he would devote a forthcoming column to it. His column, which appeared in July, 1980, lifted our speculations about two-dimensional science and technology to a new level by bringing it to the attention of a much wider public. Among those who read Gardner's column were not only scientists and technologists, but average readers with novel and startling contributions of their own.

I left for a sabbatical at Oxford that summer, hoping to work on the theory of computation and hoping also to get away from the planiverse project, which was claiming more and more of my time. I stayed in an abbey in the village of Wytham, near Oxford. There was leisure not only to work on the logical design for an entirely new way to compute things, but the opportunity to work on the Planiverse Project, a paper symposium with colleague Richard Lapidus, a physicist at the Stevens Institute of Technology in New Jersey. Our symposium had contributions from around the world on everything from two-dimensional chemistry and physics to planetary theory and cosmology. There was, moreover, a section devoted to technology, wherein the only feasible two-dimensional car ever designed appeared for the first time. It had no wheels, but was surrounded by something like a tank tread that ran on disk-bearings. The occupants got in and out of the vehicle by unhooking the tread.

The Planiverse Project was now proceeding at a satisfying rate. I assumed that within a few years it would die away to nothing. We would have had our fun, no harm done.

But a press release, written by a journalist at my home institution in the fall of 1981, changed all that. Wire services picked it up with the glee reserved for UFO reports and escaped lions. There followed a rush of magazIne and newspaper articles, as well as television stories publicizing our two-dimensional world. In particular, a piece in *Newsweek* magazine caught the attention of publishers.

In the midst of a series of papers on programming logic, I was suddenly face to face with a big writing job. [...]

[...]

[...] Think for a moment of even the humblest respects in which a two-dimensional existence on the "surface" of Arde might differ from our own.

The Jordan curve theorem's implications for Arde were profound. Closed curves lurked everywhere.

Consider, for example, Ardean soil, a mechanical mixture of two-dimensional grains and pebbles in which any pocket of water finds itself permanently trapped within the closed circle of surrounding stones. The water cannot percolate, as our groundwater does, up or down. It is trapped, at least until the soil is mechanically disturbed. Consider also the simple matter of Yendred attempting to lift a two-dimensional plank on the Ardean surface. The plank, the ground, and Yendred himself would form a simple closed curve, and the air trapped inside the enclosed space would become increasingly rarefied. The plank would seem to get heavier and heavier. Perhaps readers can imagine themselves to be Ardeans lifting such a plank. If you were Yendred, what technique would you adopt to make it easier?

But for every disadvantage of life in two dimensions, there seems to be an equal and opposite advantage. Bags and balloons are trivial to make-from single pieces of string! Yendred's father, who takes him fishing near the beginning of the book, never has trouble with tangled lines, for knots in two-space are impossible. Moreover, sailing requires nothing more than a mast!

Yendred sets out on his quest shortly after the fishing trip with his father. His home, like all Ardean homes, is underground. The surface of Arde must be left as pristine as possible. There are travelling plants and periodic rains which make temporary rivers, basically floods. Any surface structure would either disrupt the delicate one-dimensional ecology or be swept away, in any case. A simple pole stuck in the ground would become a dam which could never withstand the force of kilometers of water that would rapidly build up behind it.

In the Ardean cities which Yendred must walk through (or over) on his travels to the high plateau, we encounter the acme of two-dimensional infrastructure. There is no skyline, only the typical Ardean surface periodically marred by traffic pits. If an eastbound Ardean should happen to encounter a westbound colleague, one of them must lie down and let the other walk over him/her. Elaborate rules of etiquette dictate who must lie down and who proceed, but in an urban context there is no time for niceties. Whenever a westbound group of Ardeans encounters a west-pit, they descend the stairs, hook up an overhead cable and wait. At the sound of a traffic gong, an eastbound group marches across the cable. [...]

[...]

Yendred, after many adventures, finally reaches the high plateau and meets the mysterious Drabk, an Ardean who has developed the ability to leave the planiverse entirely and move "alongside" it, so to speak. Since *The Planiverse* is about to re-appear, I will not give the plot away, but I had better mention the *deus ex machina* that makes it all possible: In the book a class project results in a program called 2DWORLD that simulates a two-dimensional world, including a disk-shaped planet the students call Astria. Imagine the student's surprise when 2DWORLD turns out to be a sophisticated communication device which, by a Theory of Lockstep, begins to transmit images of an actual two-dimensional universe, including a planet called Arde and a being called Yendred!

When *The Planiverse* first appeared 16 years ago, it caught more than a few readers off guard. The line between willing suspension of disbelief and innocent acceptance, if it exists at all, is a thin one. There were those who wanted to believe (despite the tongue-in-cheek subtext) that we had actually made contact with a two-dimensional world called Arde.

It is tempting to imagine that those who believed, as well as those who suspended disbelief, did so because of the persuasive consistency in the cosmology and physics of this infinitesimally thin universe, and in its bizarre but oddly workable organisms. This was not just your run-of-the-mill science fiction universe fashioned out of the whole cloth of wish-driven imagination. The planiverse is a weirder place than that precisely because so much of it was worked out in the Planiverse Project. Reality, even the pseudo-reality of such a place, is invariably stranger than anything we merely dream up.

XXVI.

The narrative excerpted here, by the chemist Guido Cimino (1941–) and the biologist and philosopher Michael T. Ghiselin (1939–), concerns the evolution by marine organisms of pathways for producing ‘secondary metabolites’ —organic compounds which are not produced as part of an organism’s normal functions. In their opening passage, the authors draw a distinction between “nomothetic”—law-based—sciences like physics, and natural historical disciplines such as geology, palaeontology, and systematic zoology which employ historical narratives. They argue that while chemistry should generally be seen as a nomothetic science, natural products chemistry is an exception because it studies the historical processes by which different metabolic pathways developed; although the secondary metabolites themselves—the chemical compounds formed by the organisms—do not have a relevant history, the different sequences of events leading to different pathways for the production of secondary metabolites are inescapably historical.

The research which the authors go on to review proposes different evolutionary sequences through which organisms might have developed the capability of producing secondary metabolites. In this respect, the approach to narrative here recalls that discussed in Greg Priest’s contribution to this anthology (see **XI.**) , and like Priest they refer to the use of tree diagrams in the reconstruction of evolutionary sequences, and the scientist’s belief that such sequences can be reconstructed step by step.

The historical story in the case of the secondary metabolites is somewhat complicated by the evolution of “biosynthetic capacity”—organisms’ ability to produce novel chemical products. The authors draw an analogy between the experiments of human organic chemists and the changes which occur within organisms as a result of changes to their enzymes. The sequences of the syntheses which occur within organisms can be grasped through chemical reaction schemes, akin to those used by human chemists to plan and document their syntheses (as seen in the narratives by Corey, **XIX.** and Ernest, **XXI.**). In turn, many organic chemists have drawn inspiration from the synthetic sequences employed by plants and other organisms, reasoning that “biomimetic” syntheses which can be conducted in conditions similar to those found in living organisms are likely to be more efficient than laboratory syntheses which require high temperatures, extremes of alkalinity or acidity, and so on.

Cimino and Ghiselin extend the analogy between human chemists and processes occurring within organisms in the closing sections of this passage. They discuss two sequences through which organisms may develop new syntheses. The first involves the gradual building up of new syntheses from first principles, adding more steps and ultimately producing a molecule of greater size or complexity. The second is the “retrosynthetic mode”—a name which recalls Corey’s attempts to codify the planning of chemical syntheses (see **XIX.**) For Corey, retrosynthesis means devising a synthetic scheme by starting from the final form of a molecule and working backwards using familiar transformations to find simpler and simpler precursors. Here, it involves an organism’s development of the capacity to produce desired molecules found in its food in an independent way.

Rather than a strict distinction between the nomothetic and natural historical sciences, then, we may see this as a case of two modes of narrative ordering complementing each other. On the one hand, the phylogenetic trees and historical accounts of evolutionary biology; on the other, the reaction schemes and close attention to chemical substances characteristic of organic chemical synthesis.

Mat Paskins

2001: GUIDO CIMINO and MICHAEL T. GHISELIN, Marine Natural Products Chemistry as an Evolutionary Narrative. In McClintock and Baker (eds.) *Marine Chemical Ecology (*Boca Raton: CRC Press) pp. 115–154

[EXTRACT: pp. 115–119]

Chemistry is generally looked upon as one of the 'nomothetic' sciences, i.e., one that seeks to establish the laws of nature and does not concern itself with particular objects or events. Natural products chemistry is an exception, being very much concerned with what are called 'individuals' in a broad metaphysical sense. Like geology, paleontology, and systematic zoology, it is very much a natural history discipline. The various natural kinds of secondary metabolites are classes of molecules, and, being classes, they do not evolve any more than does the calcium carbonate that forms the shells of molluscs. What do evolve are individual populations and lineages, which change with respect to the properties of the organisms and their parts, including the enzymes that produce secondary metabolites. Metabolism has a history and we ought to be able to reconstruct that history just like the chemical and physical aspects of defense. There seems to have been an arms race between shelled molluscs and the crabs that have preyed upon them. A chemical arms race involving molluscs in which the shell has become reduced is a straightforward extrapolation.

It is no longer fashionable to dismiss secondary metabolites as mere waste products or as substances that no longer play an important role in the lives of organisms. They are distributed in the body very much like other features that have obvious value in the struggle for existence. Even today, however, much of the discussion of the supposed function of natural products continues to treat natural selection as little more than background material. Indeed, historical situations are often invoked to explain away anomalies where efforts to find adaptation fail. Adaptation can be treated as if it were nothing more than a condition or state. At least implicitly, however, the product is defined in terms of the process. In other words, when we claim that something is an adaptation, we presuppose a historical narrative, even if the narrative is concerned only with the very recent past. If we really want to understand the adaptive significance of secondary metabolites, we need to ask some truly historical questions.

These authors' contributions [the authors are here referring to themselves] in this area have mainly dealt with the evolution of chemical defense in opisthobranch gastropods. Faulkner and Ghiselin (1983) addressed the question of whether the reduction of the shell in these animals preceded the evolution of chemical defense (a post-adaptive scenario) or whether the loss of the shell was made possible by the presence of chemical defense (a pre-adaptive scenario). The latter hypothesis was preferred on the grounds that in groups in which the shell is relatively well developed, chemical defense is already present. The reasoning is basically a matter of plotting features on the branches of a phylogenetic tree and inferring the sequence of events. But the biological plausibility of the sequence in question may provide an additional line of evidence. This is a traditional mode of reasoning that goes back to Darwin and his follower Anton Dohrn, who founded the Zoological Station at Naples. Evolution proceeds by steps; in each step the functioning of the organism as a whole is conserved, but particular functions often succeed one another over time.

Various patterns in the evolution of chemical defense have been documented, including detoxification, modification and sequestration of metabolites, and their positioning in places where they will most effectively repel predators. Of particular interest is the evolution of *de novo* synthesis. The work of these authors has suggested how this might happen. It also suggests that asking questions about the evolution of biosynthetic capacity might provide a unifying theme for the study of natural products chemistry.

[…]

It is well understood that the synthesis and modification of metabolites is under enzymatic control. The enzymes may function as catalysts, and the reactions themselves are not restricted only to living systems. So the evolution of biosynthetic capacity is largely the result of changes in enzymes by mutation, gene duplication, and other familiar processes. The organisms synthesize and modify secondary metabolites in a stepwise fashion, much as organic chemists do, and in neither case are the laws of nature violated.

The term “secondary metabolite” is generally understood to mean that the chemicals in question are not directly involved in the basic maintenance of the organism. Secondary metabolites are produced from a remarkably limited range of starting materials known as primary metabolites. […] One should bear in mind that much of the diversity of metabolites can be explained as a result of stepwise synthesis of larger and larger units, with some divergent variants in skeletal structure and a lot of rearrangements and other modifications of the basic structures. Such patterns of synthesis can be explained historically, and stepwise modification of biosynthetic pathways through time is a basic phylogenetic theme. The fact that the same compound may be synthesized by different pathways is not an impediment to such historical analysis, but rather an opportunity. Different pathways often reflect separate historical origins.

Before discussing how organisms might evolve such pathways, it is convenient to consider how they might acquire pathways from other organisms. One such possibility is through symbiosis, especially mutualism. Such mutualism is well documented in the phylum Porifera, or sponges, which often contain bacteria within their tissues that produce some of the metabolites that defend the sponge and presumably the bacteria as well. The sponges did not have to evolve the chemicals that defend them. Another possibility is lateral gene transfer. The well-known spread of antibiotic resistance between lineages of bacteria makes such a transfer seem highly plausible. Lateral gene transfer may be quite common among marine microorganisms. It can enable them to acquire the capacity for biosynthesis without having to evolve it through the modification of pre-existing metabolic apparatus. Such capacity means that the organisms are not constrained by the necessity of obtaining the metabolites from symbionts or food. But whether such a transfer is not just possible but has in fact occurred, has to be established on the basis of empirical evidence. For multicellular animals it is mere conjecture.

We have suggested elsewhere that there are two modes by which the capacity for biosynthesis of secondary metabolites might evolve. The first of these is the straightforward and well-documented anasynthetic mode in which more and more steps are added and, perhaps, a molecule of increasing size is produced. Such evolution has been well documented in terrestrial plants, and some marine examples are discussed below. In some cases it has been shown that the end product of the most derived evolutionary stage is accumulated in the tissues, but that the intermediates are present in lesser concentrations. These intermediates, however, may be the metabolites that are concentrated in the tissues of related forms that represent ancestral conditions in a historical sequence. This case presents a chemical analogue of the traditional notion that ontogeny recapitulates phylogeny. Such recapitulation occurs only under restricted conditions, namely, where there has been terminal addition of new stages. There is nothing to prevent the secondary loss of developmental or biosynthetic stages, and it is possible that earlier steps in a pathway might be affected.

Another possibility is called the retrosynthetic mode. We recognized this possibility because opsithobranchs have evolved the capacity for *de novo* synthesis of metabolites similar to those that they originally derived from food. How could they evolve an entire pathway that supposedly had not been part of their evolutionary heritage, a process that usually takes a long time and a lot of unusual events? One possibility was lateral gene transfer. However, the metabolites in question sometimes did not have the same chirality as those in the food organisms, suggesting that different enzymes of different historical origin are involved. The chirality is just one example of the fact that when nudibranchs have evolved *de novo* synthesis, the metabolites that they produce, although similar, are almost never identical to the originals. So [...] we proposed that the predators first evolved the ability to modify the last stages of biosynthetic pathway while still relying upon intermediates that are available in food. Given that the intermediates are present in the food, any mutation that increases their concentration would be selectively advantageous. Steps in the synthesis could be added backward until replacing a point in which a precursor was available that the predator could synthesize on its own.

[...]

To the beginner, both chemistry and biology appear bewilderingly complex. Yet, in spite of the wealth of detail, everything seems much simpler when viewed in light of the fundamental concepts.

[...]

Organisms do what they do because they repeat what it was that contributed to their ancestors' reproductive success. What they do in some other context, whether it be the artificial conditions of the laboratory or the more natural ones of field studies, may be quite irrelevant to this issue of adaptive significance. [...] There is no such thing as a 'rigorous' experimental test of a hypothesized raison d'etre for the presence of a secondary compound. Comparative biology is the only thing we have to go on when we deal with the past. Natural products chemistry is an integral part of such comparative biology. Its role, however, is not to “reduce” the organisms to chemicals. Rather, the chemicals are placed within the context of a narrative about the organisms. The organisms synthesize and modify the metabolites and use them as resources in the struggle for existence. The two sciences can come together because the narrative must be plausible both as chemistry and as biology. There can be no contradiction of the laws and principles of either.

XXVII.

Geology may be characterised as a narratively-based science (e.g. Frodeman 1995), though this point is frequently obscured by the prescriptive formats required by many technical journals. There is however at least one genre of scientific paper in which geology's nature as a narrative science stands out: the regional review article. This is typically a synthesis of previous work on the geology of a particular geographical area presented in the form of an integrated account of its geological evolution. The narrative nature of such publications is emphasised in the abstracts that typically preface scientific papers.

The example under consideration here is the abstract of an 83-page review paper which synthesised work published since 1893 on the geological development of Scotland (see also **XXVIII.**). The self-professed aim of the authors was to "pull together information" into "a continuous story" about a geologically interesting region. To do this they drew on 365 published sources to trace Scotland's three billion year geological history. The components of their synthesis included the results of field mapping, geophysical surveys and geochemical studies, while radiometric dating allowed events to be placed into a quantitative temporal sequence.

The paper focuses on two specific aspects of the geological evolution of Scotland, namely plate tectonic processes and the form and composition of igneous bodies such as granitic intrusions and basaltic lavas. The word *tectonomagmatic* in the title is a neologism, coined since the plate tectonic revolution of the 1960s, which expresses the close connection between these two elements. The paper consequently de-emphasises other aspects of geology such as stratigraphy and geomorphology.

Although the abstract is replete with geological terminology, it does not take specialist knowledge to appreciate its overall narrative structure. The narrative begins with igneous activity on the putative supercontinent of Kenorland. Subsequent, events include episodes of continental collision and break-up, the opening and closing of oceans, the deformation and metamorphism of rocks by extremes of heat and pressure, and the injection and eruption of various forms of magma. The narrative ends about 50 million years ago when Scotland was close to its present-day configuration. It is easy to spot the key feature that distinguishes a narrative from a simple time-ordered chronology (e.g. Morgan 2017), for the events are related to each other by direct or indirect causal connections indicated by words and phrases such as *contributed to*, *formed*, *resulted in*, *marked by*, *produced*, *affected* and *caused*. In addition to these explicit indications, the geologically literate reader will be able to infer other complex causal relationships that are signalled by the technical language employed.

Finally, while the overall trajectory of the narrative is obviously not teleological, it does have a narrative arc that paints a bigger picture of events and processes consistent with its component parts, with physical laws, and with the norms of geological reasoning that have been developed over more than two centuries. In other words, the piece has its own coherent narrative logic (Frodeman 1995).

Andrew Hopkins

[See page 189 for references.]

2007: RAY MACDONALD AND DOUGLAS J. FETTES, The tectonomagmatic evolution of Scotland *Transactions of the Royal Society of Edinburgh: Earth Sciences* 97 pp. 213–295

[EXTRACT: p. 213]

ABSTRACT: Scotland has a magmatic record covering much of the period 3100–50 Ma. In this review, we pull together information on Scotland's igneous rocks into a continuous story, showing how magmatic activity has contributed to the country's structural development and assessing whether the effects of older magmatic events can be recognised in later episodes.

The oldest igneous rocks are part of supracrustal sequences within the Lewisian Gneiss Complex, formed when Scotland was part of the supercontinent Kenorland. The supracrustal rocks were intruded between 3100 and 2800 Ma by granodiorites and tonalites, which were metamorphosed and deformed in a major tectonothermal event between 2700 and 2500 Ma. The break-up of Kenorland (2400–2200 Ma) was marked by the intrusion of mafic dyke swarms of tholeiitic affinity. The convergence of continental masses to form the supercontinent Columbia resulted, at ~1900 Ma, in a series of subduction-related volcanic rocks and gabbro–anorthosite masses. Subsequent continent–continent collision formed a series of granite–pegmatite sheets at ~1855 Ma and ~1675 Ma and reworked much of the earlier rocks in the amphibolite facies. Columbia was breaking up by 1200 Ma, an event marked by remnants of basaltic magmatism in the NW of the country. Re-assembly of the continental fragments to form the supercontinent Rodinia resulted in the Grenville Orogeny, which in Scotland was marked by basement reworking but no confirmed magmatic activity. Early attempts to split Rodinia produced a rift-related, bimodal, mafic–felsic sequence in the Moine Supergroup of the Northern Highlands, at least some of the mafic rocks having mid-ocean ridge basalt affinities. Crustal thickening during a disputed orogenic event, the Knoydartian, may have caused regional migmatisation. The final

break-up of Rodinia occurred in Scotland at ~600 Ma, when very extensive tholeiitic magmatism characterised the later parts of the Dalradian Supergroup, while a series of granites intruded the Moine and Dalradian successions.

Ordovician and Silurian times saw the closure of the Iapetus Ocean and the convergence of Laurentia, Avalonia and Baltica. The collision of a major arc system with Laurentia caused the Grampian event (480–465 Ma) of the Caledonian Orogeny, marked by ophiolite obduction, the generation of (largely) anatectic granites, volcanism in the Midland Valley and Southern Uplands, and intrusion of a major gabbro–granite suite in the NE. The late-Caledonian events (435–420 Ma) were largely post-collisional and were marked by the emplacement of alkaline igneous intrusions in the NW, calc-alkaline granitic intrusions over much of the country, widespread volcanic activity and regional dyke swarms. Laurentia, Avalonia and Baltica amalgamated to form the supercontinent Laurussia. Magmatic activity recommenced at 350 Ma, when intra-plate alkaline magmatism affected much of southern Scotland, in particular, through into Permian times. The alkaline magmatism was interrupted at ~295 Ma by a short-lived event in which tholeiitic magmas were intruded as sills and dykes in a swarm ~200 km wide. In the early Palaeogene, lithospheric attenuation related to proto-North Atlantic formation and the splitting of Pangaea was complemented by the arrival of the Iceland mantle plume. Huge volumes of mafic magma were emplaced as lava fields, central complexes and regional swarms, locally increasing crustal thickness by 30%.

XXVIII.

The article “The Tectonomagmatic Evolution of Scotland” by Macdonald and Fettes (2006), the abstract of which is discussed in **XXVII.** is illustrated by more than 60 images, most of which are sketch maps and photographs. The first three, Figures 1 and 2 and Table 1, are relatively simple images that work together to introduce the reader to an outline of the geological history of Scotland via a diagrammatically related narrative, which is effectively a retelling of the text discussed in **XXVII.**

The outline map in Figure 1 shows the five tectonic components, known as terranes, which make up the geology of Scotland together with their bounding faults (dashed lines). A terrane is an elongate fragment of the Earth’s crust that has been permanently sutured to a continent with which it has collided following the closure of an ocean; each terrane having a distinct geological history. Continents have been understood to grow by this process of lateral terrane accretion since the plate tectonic revolution of the 1960s.

In Figure 2 the positions of continents and amalgamated units known as supercontinents (Kenorland, Columbia, etc.) are plotted at six points in the geological past using orthographic global maps (Ga and Ma are abbreviations for billions and millions of years ago respectively). These form a series of snapshots that depict changes in the Earth’s configuration over time (with Scotland shown on each globe as “Sc”), recalling an iconic set of images used by Alfred Wegener in his unsuccessful argument for continental drift (Wegener, 1929, Figure 5). As Macdonald and Fettes acknowledge however, “the number, configuration and geological history of… supercontinents are highly controversial” (ibid, p. 214). This is especially true for the older depictions. Figure 2 therefore presents a more speculative narrative of the geological evolution of Scotland than Figure 1 or Table 1, which are both based directly on field observations.

The five stratigraphic columns in Table 1 relate to each of the terranes shown in Figure 1. The numbers posted vertically indicate millions of years before the present, and so tell the historical narrative of the geology of Scotland’s terranes. The capitalised text on the far left of Table 1 refers to periods of supercontinent growth and break-up, which can be directly correlated with Figure 2 (and its caption text), as can certain mountain building episodes (orogenies) that result from continental collisions. The symbols in the stratigraphic columns and the accompanying labels pick out notable igneous events, including periods of volcanic activity, and the intrusion of dykes and granitic bodies.

Taken together, the three sets of images cover all three spatial dimensions as well as that of (geological) time. By cross-referencing them, the reader is equipped to track the interconnected events that make up the broad tectonic and igneous development of Scotland over a period of more than two billion years. They construct the outline of a coherent, though not necessarily definitive, geological narrative—expressed in four dimensions—of the evolution of Scotland.

Andrew Hopkins

[See page 189 for reference.]

2007: RAY MACDONALD AND DOUGLAS J. FETTES, The tectonomagmatic evolution of Scotland *Transactions of the Royal Society of Edinburgh: Earth Sciences* 97 pp. 213–295

[EXTRACT: pp. 215–217]

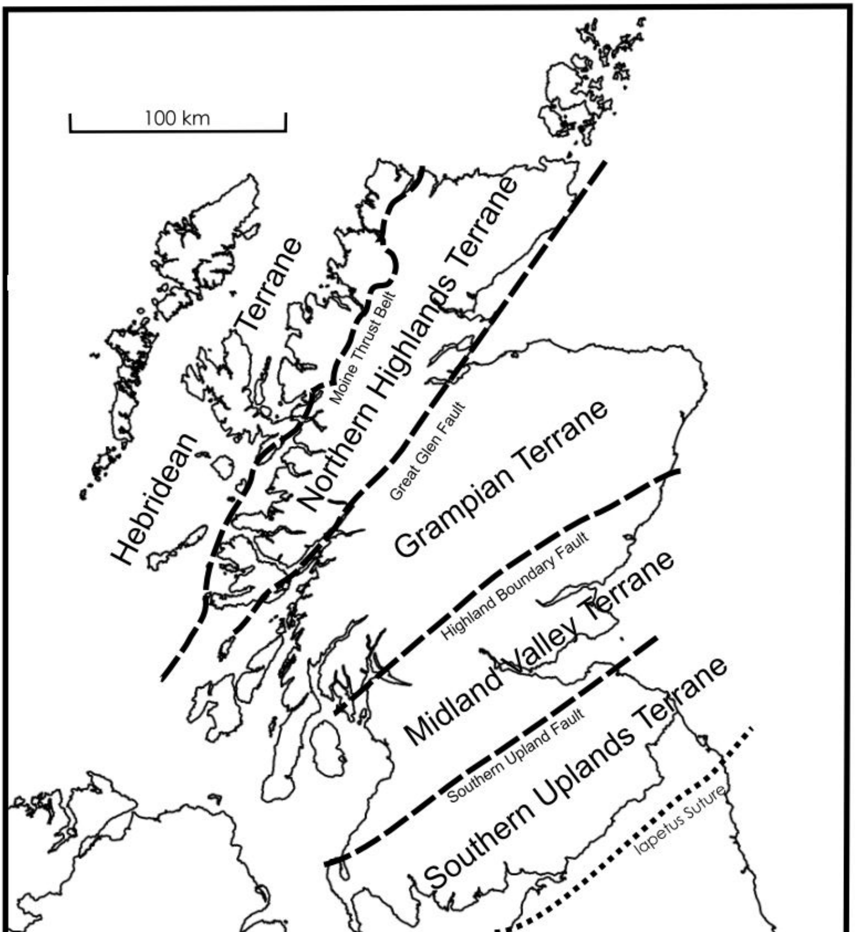


Figure 1 The major terranes of Scotland

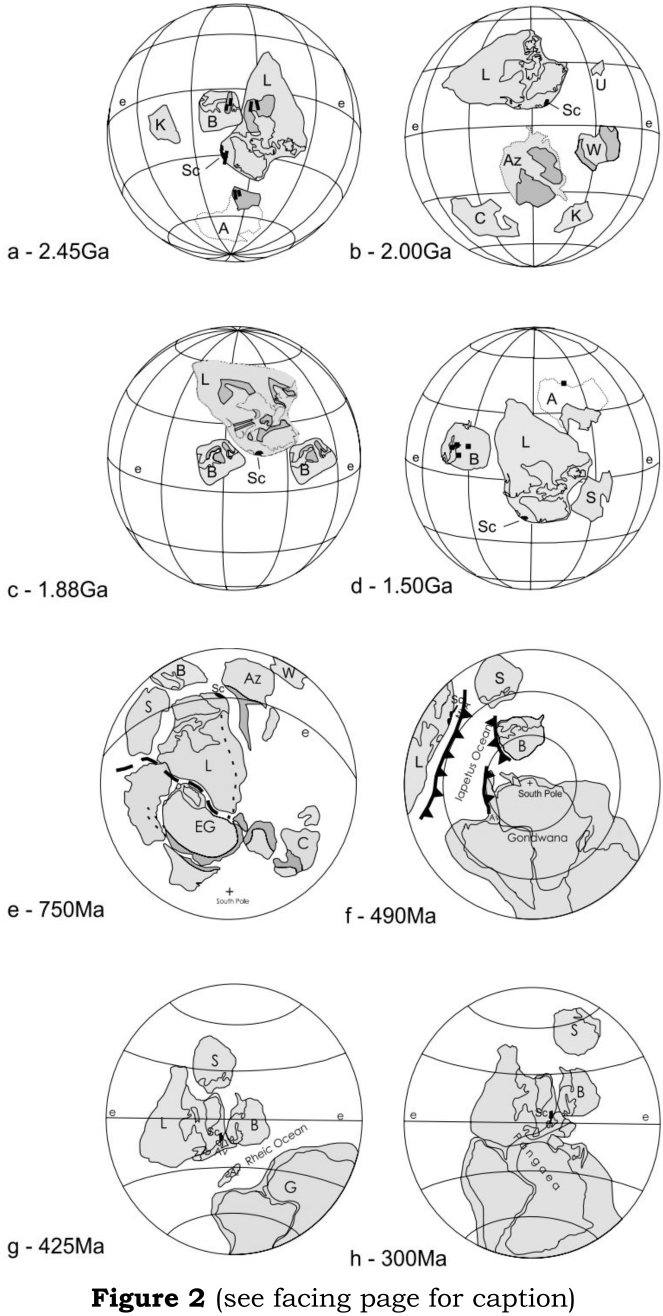


Figure 2 (see facing page for caption)

Continental reconstructions illustrating the varied history of Scotland: (a) the initial break-up of Kenorland; regional dyke suites, including the Scourie Dyke Suite, are shown as dark stripes; (b) the final dispersal of Kenorland; (c) the growth of Columbia; darker shading shows the orogenic belts, including the Nagssugtoqidian, the Laxfordian and the Kola–Lapland; two possible positions for Baltica are shown; (d) the break-up of Columbia; dark squares show sites of associated magmatism; (e) the initial break-up of Rodinia; darker shading shows the Grenville orogenic belts; bold-dashed line shows the main zone of rifting; (f) the initial growth of Laurussia as Iapetus closes; dark lines indicate subduction zones; (g) the final growth of Laurussia as Avalonia and Baltica collide with Laurentia; (h) the supercontinent Pangaea. Abbreviations: (A) Australia; (Ar) Armorica; (Av) Avalonia; (Az) Amazonia; (B) Baltica; (C) Congo; (e) equator; (EG) East Gondwana; (G) Gondwana; (K) Kalahari; (L) Laurentia; (MVA) Midland Valley Arc; (S) Siberia; (Sc) Scotland; (U) Ukraine; (W) West Africa. […]

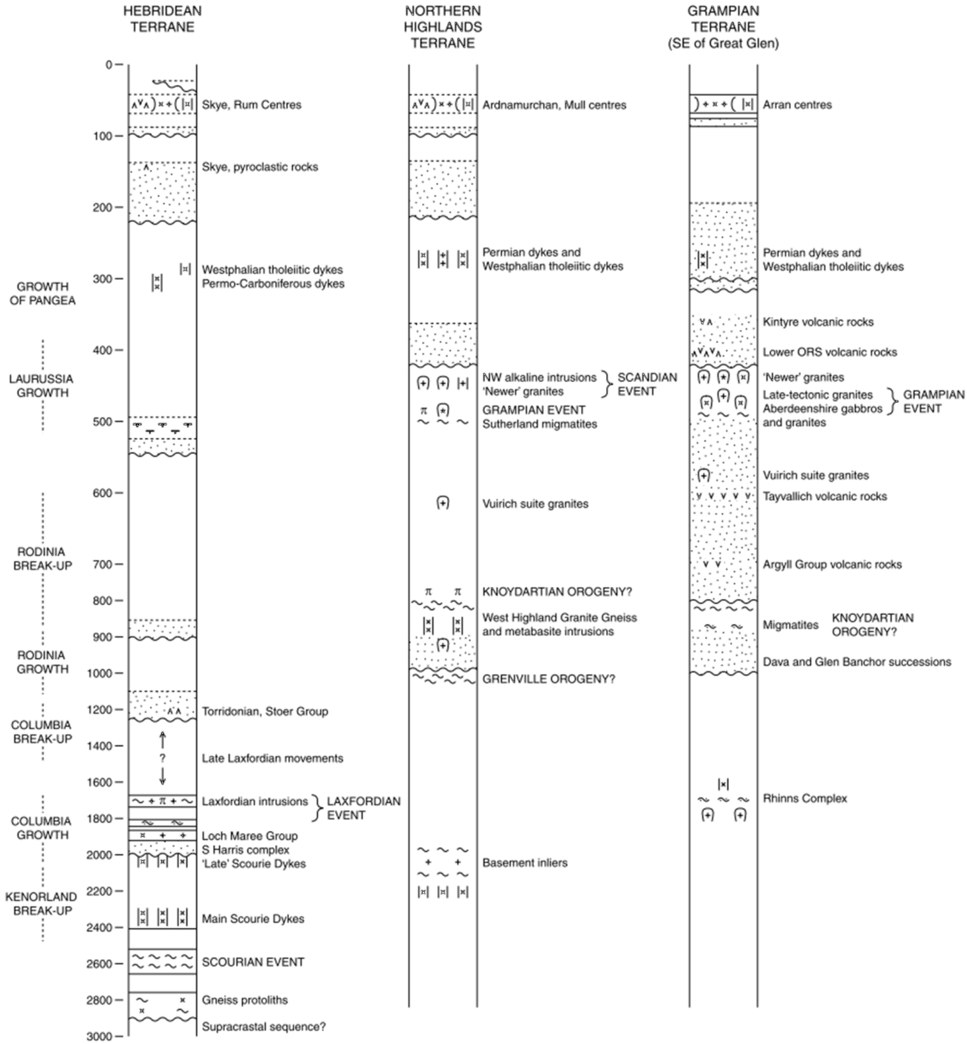


Table 1 Geological events in the Scottish terranes

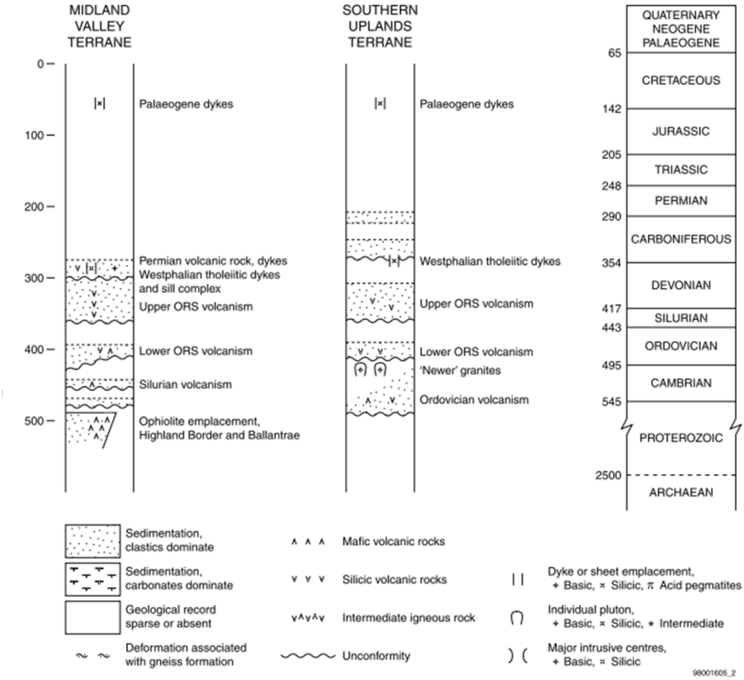


Table 1 *continued*

XXIX.

Cosmology is an inherently narrative science that tells the story of the origins and evolution of the universe. Yet technical papers in cosmology rarely take narrative form, relying instead on dense tracts of abstract mathematics. For the most part, cosmologists narrate the cosmic story in other forums—in lecture halls and tea rooms, in textbooks and popularisations. These shared stories provide the interpretative context in which technical papers are read.

Occasionally however, this narrative context shows itself in writings addressed to a specialist audience. This example, by three theoretical physicists, was originally written for an annual essay competition aimed at stimulating research on gravitation. The essay format allowed the authors to write in a more narrative style than normal, as is signalled by the references to “poetic charm” and “legend” in the opening quote.

The standard story underlying modern cosmology is that the universe began with the Big Bang, a state of very high temperature and density before which neither space nor time existed. The newly-formed universe expanded and cooled, allowing matter to clump together, though gravity gradually slowed the rate of expansion. Yet this leads to inconsistencies with some of the observable properties of the universe—known as the flatness and horizon problems. These are resolved if, shortly after the Big Bang, there had been a brief “inflationary” period of accelerated expansion. However, observations in 1998 showed that the expansion of the universe is currently speeding up rather than slowing down. This suggests the presence of “dark energy” that causes the current accelerated expansion.

The essay proposes an alternative to the prevailing story: rather than coming into being ex nihilo, the universe underwent a continuous sequence of expansion and collapse in a cycle driven by unobserved extra dimensions called ‘branes’. In presenting this new model, the authors bring together two distinct stories, as do many scientific writings: the ontological story (in this case the causal chain of cosmic events) is told at one remove through an epistemological story of scientists’ attempts to understand the world and the concepts they devise.

The authors draw on Greek myth to name their model the “phoenix” universe, and the destructive phase of the cosmic cycle, “ekpyrosis”. This provides a template that establishes the causal chain of events necessary for any narrative. The universe is animated (it “arises from the ashes”) and dark energy is endowed with heroic agency as it “saves the universe from its ashen fate”. In a rhetorical move that is commonplace in theoretical physics, a human-like presence is also conjured—here jokingly referred to as the “braneless observer” who misunderstands what they see.

Paul Ricoeur (1980) has argued that narrative is the means through which we give expression to time. This essay may not be a fully developed narrative, but in grappling with the nature of time, it not only shares a project with narrative but challenges the reliance of conventional narrative on beginnings and endings.

Felicity Mellor

[See page 189 for reference.]

2009: JEAN-LUC LEHNERS, PAUL J. STEINHARDT and NEIL TUROK, The Return of the Phoenix Universe *International Journal of Modern Physics D* vol. 18:14 pp. 2231–2235

> *"Those solutions where the universe expands and contracts successively…, have an indisputable poetic charm and make one think of the phoenix of legend".*
>
> Georges Lemaitre, 1933

Two breakthroughs of the 20th century changed forever our understanding of the universe: the observation that the universe is expanding, made in the 1920s, and that the expansion rate is accelerating, made in the 1990s. The full implications have yet to be realized. The currently favored inflationary picture does not explain the origin of the expansion—the big bang—or provide a rationale for the current acceleration. Recently, though, a new cosmological model has emerged that breathes new life into an old idea—the phoenix universe—providing an explanation for both the bang and the dark energy, and suggesting why the latter must be small and positive today.

The "phoenix" was first introduced into cosmology by Georges Lemaitre shortly after Hubble's discovery that the universe is expanding. Friedmann and Lemaitre had discussed the expanding universe model several years earlier, but its realization in nature forced cosmologists to face up to its baffling beginning: the big bang, the moment about 14 billion years ago when the temperature and density reached infinite values. The standard interpretation today is that the bang marked the beginning of space and time. However, this is far from proven: all we really know is that Einstein's equations fail and an improved theory of gravity is needed. In fact, the idea of a "beginning," the emergence of the universe from nothing, is a very radical notion. A more conservative idea is that the universe existed before the big bang, perhaps even eternally. Historically, this motivated many of the founders of the big bang theory, including

Friedmann, Lemaitre, Einstein, and Gamow, to take seriously an "oscillatory" universe model in which every epoch of expansion is followed by one of contraction and then by a "bounce," at an event like the big bang, to expansion once more. For the model to work, the matter must exceed the critical density required for its self-attraction to slow the expansion and eventually reverse it to contraction. But, by the end of the 20th century, observations had shown the opposite: the matter density is subcritical and the expansion is speeding up.

Yet, today, the phoenix universe has been revived due to the development of a new cyclic theory of the universe that incorporates dark energy and cosmic acceleration in an essential way. To explain the theory, it is useful to invoke a picturesque version inspired by string theory and M-theory in which space-time consists of two three-dimensional braneworlds separated by a tiny gap along an additional spatial dimension. One of these braneworlds is the world we inhabit. Everything we can touch and see is confined to our braneworld; the other is invisible to us. According to this picture, the big bang corresponds to a collision between the braneworlds, followed by a rebound. Matter, space, and time exist before as well as after, and it is the events that occur before each bang that determine the evolution in the subsequent period of expansion.

Unlike Lemaitre's phoenix universe, the matter density is subcritical, consistent with observations. The big bang repeats at regular intervals because a springlike force keeps drawing the braneworlds together along the extra dimension, causing them to collide every trillion years or so. Associated with the springlike force are kinetic and potential energy, which play an important role as the source of dark energy in the cyclic model.

The dark energy equation of state ω is defined as the ratio of the pressure (kinetic minus potential energy of the braneworlds) to the total energy density (kinetic plus potential energy). When the braneworlds are farthest apart, the total energy is predominantly potential and positive, corresponding to $\omega \approx -1$, similar to a cosmological constant. Although this potential energy is negligible right after a collision, it decreases slowly and, about nine billion years later, overtakes the matter density, causing the expansion of the braneworld to accelerate. The acceleration cannot last forever, though, because the spring eventually releases, causing the braneworlds to hurtle toward each other. Now, the potential energy decreases and becomes negative while the kinetic energy grows, causing ω to increase sharply from $\omega \approx -1$ to $\omega \gg 1$ and initiating a period known as "ekpyrosis". From the point of view of a "braneless observer," someone who is unaware of the extra dimension and the other braneworld and reinterprets the goings-on in terms of the usual Einstein general relativity, the universe appears to be undergoing a peculiar period of ultraslow contraction in which the scale factor $a(t) \sim (t_{\text{bang}} - t)^{2/3(1+\omega)}$ as t approaches t_{bang} with $\omega \gg 1$. The dark energy continues to dominate the universe during this ekpyrotic contraction phase, and the matter density remains negligibly small.

The ekpyrotic phase is key, because it removes any need for inflation. The horizon problem is resolved simply because the universe exists long before the big bang, allowing distant regions to become causally connected. To see how the flatness puzzle is solved without inflation, recall that the problem arises in a slowly expanding universe, where a small deviation from flatness at early times grows into an unacceptably large one by the present epoch. But, now, just run the story backward: as space slowly contracts, an initially large deviation from flatness shrinks to an infinitesimal one. In an ekpyrotic contraction phase, because $\omega \gg 1$, the deviation from flatness is diminished by more than it grows during the subsequent expansion phase, thus explaining why it is negligibly small today.

Both ekpyrotic contraction and inflation can generate large scale density fluctuations from microscopic quantum fluctuations. In inflation this occurs because quantum fluctuations are stretched exponentially while the Hubble horizon increases very slowly, and so the fluctuations end up spanning superhorizon scales. In the ekpyrotic contraction phase, the same feat is accomplished because the quantum fluctuations remain nearly fixed in scale while the Hubble horizon shrinks rapidly. By the time the phase ends, quantum fluctuations formed inside the horizon span superhorizon scales, resulting in a spectrum of nearly scale-invariant fluctuations very similar to inflation, although with observably different predictions for primordial gravitational waves and non-Gaussian density fluctuations.

An important caveat arises, though, for the best understood example of ekpyrosis, where the density perturbations are generated by a so-called entropic mechanism. The ekpyrotic energy maintains $\omega \gg 1$ only if the quantum fluctuations remain within a narrow range. Otherwise, ω drops precipitously, inhomogeneities and curvature grow, and space collapses into a warped amalgamation of black holes. The chance of avoiding decimation is small: during every e-fold of contraction, quantum fluctuations reduce the fraction of space with $\omega \gg 1$ by $1/e$. Since the ekpyrotic phase lasts for about 120 e-folds, the fractional volume of space that makes it smoothly to the bounce and re-emerges in a flat, expanding phase is $f \approx e^{-360}$. This fraction is so tiny that, if the ekpyrotic phase started today, 14 billion years after the big bang, the entire observable universe (10^{84} cm^3 across) would be decimated.

Dark energy saves the universe from this ashen fate by causing the expansion to accelerate. If acceleration continues for at least 560 billion years (> 56 e-folds), a volume of at least a cubic centimeter will retain its $\omega \gg 1$ ekpyrotic form all the way to the next crunch and emerge unscathed: flat, smooth, and isotropic. As tiny as a cubic centimeter may seem, it is enough to produce a flat, smooth region a cycle from now at least as large as the region we currently observe. In this way, dark energy, the big crunch, and the big bang all work together so that the phoenix forever arises from the ashes, crunch after crunch after crunch.

The revival of the phoenix universe could also resuscitate an old proposal for solving one of the deepest mysteries in science: why the cosmological constant (or, equivalently, the dark energy density when $\omega \approx -1$) is 10^{120} times smaller than dimensional analysis suggests. The proposal involved introducing a mechanism which causes the cosmological constant to relax to smaller values. Starting out large, it naturally decreases but its downward drift slows dramatically as it becomes small. Should it ever slip below zero, gravitational collapse follows swiftly. The result is that, for a vast majority of the time and throughout almost all of space, the cosmological constant is tiny and positive, just as we observe.

Attempts to incorporate this idea into models where the big bang is the beginning failed because the relaxation process takes vastly longer than 14 billion years. There is plenty of time in a cyclic universe, though. The relaxation can occur without disrupting the cycles and vice versa, so that an overwhelming majority of cycles occur when the cosmological constant is small and positive. By incorporating the effects of dark matter, ordinary matter, and radiation on the rate of drift, it may even be possible to explain the quantitative value observed today.

XXX.

Nick Haddad, a professor of ecology at North Carolina State University, here weaves together a narrative about solving a problem in conservation with one of personal recovery. This narrative appeared as part of a special section in the open-source journal PLOS BIOLOGY, about "Conservation stories from the front lines". The editors of this section note that scientific stories are told, "…over raucous conference cocktails and long hours on the road, across lab benches and conference call lines, and around campfires after long days in the field." They contrast these sociable stories with "the often dull, sometimes tedious reports that fill the scientific record" (Gross et al 2018), and propose an experimental form of "peer reviewed scientific narrative: Conservation Stories." These editors claim that "narratives focus on causal linkages among a sequence of events influenced by the actions of specific characters…[which]…often carry an emotional punch". But they also want to be wary of these kinds of narrative because they can also be "seductive, even among technical experts", and have been "critiqued as manipulative and inappropriate."

Just as the 'science studies community' wants to present a warts and all 'front line' picture of science, incorporating social dynamics, material contingencies, economic considerations, distinct from the more sanitised version intended for public consumption, these Conservation Stories show how in recent years some scientists themselves have sought to tell similarly detailed, thick, narratives. Yet Haddad also appeals explicitly (as a sociologist or historian would not) to a mythological form of narrative: the resurrection story. His goal in doing so is to combine details of the hope and possibility of conservation with his own changing beliefs and with his waning and restored optimism about the ways in which humans can learn about, and effectively intervene within, ecosystems. Haddad writes about trying to conserve butterflies which live exclusively on artillery ranges. In discussions of conservation and ecology, the fact that some organisms live in military settings is a picturesque and curious fact. Such details form part of a wider sensitivity on the part of ecologists and conservationists to the interactions between human activities and ecosystems, rather than the attempt to return to a pure and pristine nature, undisturbed by human interference. Haddad's butterfly narrative is one in which his initial overconfidence about creating an undisturbed environment proves ruinous for them; only through accessing the disturbance of the artillery range, and recognising the role of the other animals which live there, does he realise that "bombs and beavers" together reproduce the forms of "natural disturbance" which the butterflies require in order to thrive.

Haddad weaves his account of butterfly conservation into a narrative of his own recovery from a traumatic brain injury. This autobiographical aspect to the paper is an appeal to his readers, to consider how those involved in conservation efforts can keep going in the face of repeated disappointment and startling renewal.

Mat Paskins

[See page 189 for reference.]

2018: NICK M. HADDAD, Resurrection and resilience of the rarest butterflies *PLoS Biol* 16 (2) e2003488

For as long as I can remember, I have been attracted to things that are rare. As a child, these things included coins, stamps, and football cards. When I was invited as a young professor to lead efforts in science and conservation of one of the world's rarest butterflies, I accepted enthusiastically. Beginning in 2002, I focused my efforts on conservation and recovery of St. Francis' Satyr (*Neonympha mitchellii francisci*) and its small remaining populations. I am by nature an over-optimist, and I knew (or hoped anyway) I could succeed and conserve the butterfly quickly.

The only place in the world that St. Francis' Satyr can be found is in southern North Carolina. It has required active conservation since the subspecies' discovery in 1983 at the Fort Bragg Army Installation. At that time, its total worldwide population was thought to number 100 butterflies in one population that occupied 1 ha in area. By 1990, the population had declined to zero, and the subspecies was declared extinct.

St. Francis' Satyr's resurrection was nearly as surprising as its original discovery. In 1993, Fort Bragg biologist Erich Hoffmann discovered a population in the most inaccessible place, the range where the army fires guns, flares, and heavy artillery. With this new knowledge and with a wider search of the installation, Hoffmann found St. Francis' Satyr to occupy 10 ha of wetlands. Although a tiny area, it provided an apparently stable starting point from which my lab could work to expand and grow populations.

St. Francis Satyr's physical features are unremarkable. It is small and brown. Most frequently, I see individuals sitting low on blades of grass. The butterfly lives along streams in open, grassy wetlands. A common assumption in conservation is that if we can exclude people from wild places, those landscapes—and the populations they support—will recover. So I set out to keep people, including scientists and soldiers, out of the way. I set up initial hands-off prescriptions, sat back…and watched the populations slide toward extinction.

I am at the same time bemused and appalled as I look back on my initial self-assuredness. Of seven populations that my group monitored outside artillery ranges, I watched as one was extirpated in 2003, the next in 2005, and the next three in 2008, 2009, and 2010.When I began my efforts in 2002, the population size of St. Francis' Satyr reached 1,500. By 2011, it had descended to 100 [4]. Despite my effort and oversight, extinction appeared inevitable.

As I watched populations collapse, there was one thing that nagged at me. St. Francis' Satyr's saving grace is that most of its total range is confined within areas that at first seem hostile: artillery ranges. Because these areas are nearly completely closed to access, the reasons they harbored St. Francis' Satyr had remained a mystery. Knowing butterflies were in there, my lab could create hypotheses that might explain their presence, hypotheses that were all poorly supported. How could St. Francis' Satyr possibly thrive amid the cacophony of artillery?

My lucky break came just as St. Francis' Satyrs were in their sharpest decline. It was then that I was granted access to the artillery ranges for the very first time. When I first entered, I nervously made my way for kilometers through the shards of casings scattered about the landscape. Yet it wasn't the danger of the debris that was at the top of my mind. With the impending collapse of St. Francis' Satyr populations beyond the artillery ranges, I was thrilled by my first encounters with the burgeoning populations.

On my first trip in, I was awed by the ranges' unique habitats. Unlike habitats outside, the ranges are wide open woodlands harboring rare plants and animals. Whereas wetlands outside the ranges are thick with trees, shrubs, and vines, the wetlands inside are maintained wide open, conditions ideal for St. Francis' Satyr. Problems with maintaining these conditions outside the ranges are overcome within them by two forms of disturbance. Artillery replicates historical and natural fire regimes, igniting vegetation and spreading unimpeded by infrastructure. Beaver (because they are not considered pests) create long networks of flooded wetlands. Together, bombs and beavers create open wetlands and butterflies.

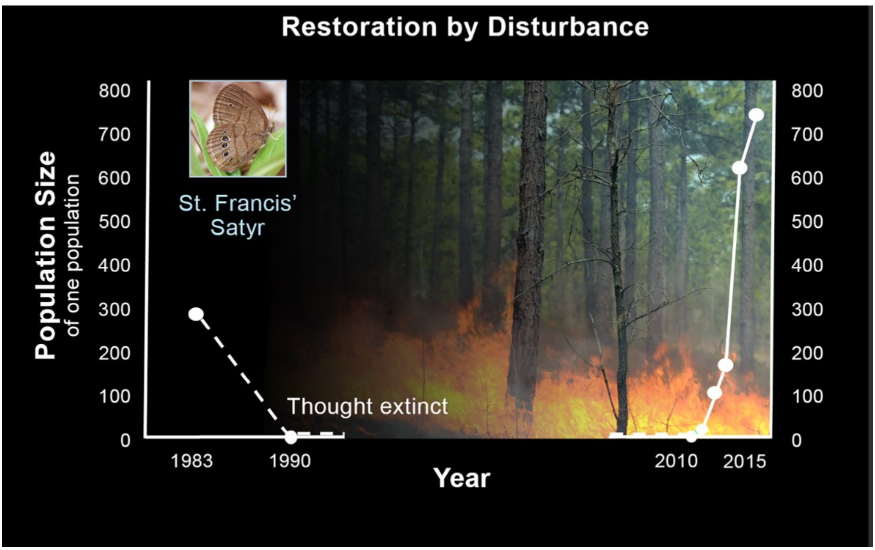
My visits to the artillery ranges coupled with near extinction outside finally broke me from my hands-off approach to restoration. I now recognized that more, not less, disturbance is needed. Wetland habitats change over time, as natural disturbance resets vegetation before succession transforms grassy wetlands to forest. For a butterfly whose caterpillars eat sedges, trees are a destructive force. Paradoxically, so are disturbances caused by beaver and fire. Butterflies cannot survive flooding, fires, or succession. For populations to survive, I reasoned, they must disperse along stream corridors to move from degrading to improving habitats. Even though the disturbance may harm one isolated butterfly population, it quickly generates high-quality habitats that increase those populations in the longer term. This variety of disturbed and recovering habitats across the landscape increases the entire population across St. Francis' Satyr's range. This caused me to propose disturbances that replicate range-like conditions in new areas.

Without artillery at our disposal, my lab became ersatz beavers and began recovery of St. Francis' Satyr habitat. Working where there were no St. Francis' Satyrs, we created an experiment to test effects of different types of disturbance caused by beaver. In some plots, we removed trees. This could be done by brute force, as we felled trees with chainsaws and hauled them out by hand. Dam creation is more challenging, both in materials and sizes of dams. We discovered a company that could make custom-size dams that we could install easily by inflating them with water from a river or stream. Our dams were 150 feet long by 18 inches high, a size meant to impede water flow and create grassy wetlands hospitable for the butterfly.

We installed our experiment in 2011. One block was located 200 m from an existing St. Francis' Satyr population, and the experimental sites were colonized immediately. The other block was distant from existing populations, and we had to seed it with butterflies raised in a research greenhouse. Populations began to grow. In successive years, population sizes rose to 50 then 100 then 200 butterflies (Fig). Today, the population size in our experimental restoration sites has reached 750 butterflies. When analyzed together with the artillery ranges, our restored area now supports one-fifth of the global St. Francis' Satyr population. Success here has given me hope that we can restore other places on and off the military installation. My 15-year effort now seems to be bearing fruit in the sense that we are observing the first signs of recovery.

And then I learned a harder lesson: while playing a pickup basketball game three years ago, I fell, cracked my head, and sustained a severe traumatic brain injury. I lost a period of my life. My first recollections were two months later, looking up from a bed in an acute rehabilitation hospital. It was easy for me to stumble around the unit, see and interact with other patients, and believe I was now "fine." For two months without memory and many more months of recovery, my family and close friends helped me in some ways that I know and others that I will never know. St. Francis' Satyr also helped in my recovery, as the butterfly gave and guided my focus toward the power of restoration, both of my life and of nature.

A short time after I'd regained my memory, I encountered one of the many nurses who cared for me in the intensive care unit. The first thing Zandro asked was this: "Nick, how are those rare butterflies?" I wondered out loud: how did you know about my interest in rare butterflies? He responded, "You talked about them every 90 seconds for three straight days." That comment was at the same time amusing and profound. In an unexpected way, it reinforced how central those rare butterflies had become to my work and my life. Through months of needed recovery, I had idle time to reflect on many things, including St. Francis' Satyr. I was eventually able to look with fresh eyes at data we had collected in the years before and after our restoration efforts (Fig). I looked at graphs of butterfly numbers over time, the same graphs I'd looked at tons of times before, and saw two different patterns. Before restoration, population size fell rapidly toward zero (left half of Fig). The magnitude and rate of decline were frightening until the population was extirpated. When I had proposed restoration, I'd viewed it primarily as a test of principles of basic ecological science. By the time restoration had begun, the population had descended to such a low level that the need for restoration success had grown urgent.



Then, in the same graphic, I observed resurrection (right half of Fig). Restoration success was in no way certain. I watched the surprising speed at which the population recovered. Our efforts resulted in a rare success at the intersection of science and endangered species conservation.

[…]

[…] The lesson learned from the rarest butterflies is that habitat loss includes the loss of natural ecosystem processes such as disturbance. Recovery of the rarest butterflies, and assuredly for other insects, must include restoration of natural disturbance.

REFERENCES AND FURTHER READING

I.

Gat, Azar (1991) '*The Origins of Military Thought: From the Enlightenment to Clausewitz*' New York: Oxford University Press.

Puységur, Jacques-François de Chastenet (1748) '*Art de la guerre par principes et par règles. Tome Premier*' Paris: Quai des Augustins.

II.

Branch, M. P. (ed.) (2004) '*Reading the Roots: American Nature Writing before Walden.*' University of Georgia Press.

Lee, B. G. (1972) 'William Bartram: Naturalist or 'Poet'?' *Early American Literature*, 7(2), pp. 124–129.

III.

Keynes, J.M. (1933) 'Robert Malthus 1766–1834: The First of the Cambridge Economists' in *Essays in Biography*. London: Macmillan.

IV.

Shapiro, A. (2014) 'Darwin's foil: The evolving uses of William Paley's Natural Theology 1802–2005' *Studies in History and Philosophy of Science Part C: Biological and Biomedical Sciences*, v.45, pp. 114–123.

VI.

Buckland, A. (2010) 'Losing the Plot: the Geological Anti-Narrative' *19: Interdisciplinary Studies in the Long Nineteenth Century* 11. doi: https://doi.org/10.16995/ntn.578

Secord, J. (2005) 'Introduction' in Lyell, C. *Principles of Geology* London: Penguin [1830].

VII.

Gooding, D. (1990) *Experiment and the Making of Meaning*. Dordrecht: Kluwer.

VIII.

Piddington, H. (1848) *Sailor's Horn-Book For The Law Of Storms*, London: Smith, Elder and Co.

XI.

Priest, Greg (2018) 'Diagramming evolution: The case of Darwin's trees' *Endeavour*, 42, pp. 157–171.

XIV.

Ambar, S. (2019) 'Du Bois and James at Harvard: The Challenges of Fraternal Pairings and Racial Theory' *Journal of race, ethnicity, and politics*, Vol.4(2), pp. 352–377

Du Bois, W.E.B. (1901a) 'The Negro Landholder of Georgia' *Bulletin of the [US] Department of Labor*, 35, Washington DC: Government Printing Office, pp. 647–777.

Du Bois, W.E.B. (1901b) 'The Negro as he really is' *The World's Work, 2*, pp. 848-866.

Du Bois, W.E.B. (1904), 'The Negro Farmer' *Bureau of the Census, Negroes in the United States* No. 8, Washington DC: Government Printing Office, pp. 69–98.

Zamir, Shamoon (1999) '"The Sorrow Songs"/"Song of Myself": Du Bois, the Crisis of Leadership, and Prophetic Imagination' pp. 346–364 in W.E.B. Du Bois, *The Souls of Black Folk*, edited by Henry Louis Gates Jr. and Terri Hume Oliver, New York and London: W.W. Norton and Company [1903].

XIX.

Hepler-Smith, Evan (2018) 'A Way of Thinking Backwards' Computing and Method in Synthetic Organic Chemistry. *Historical Studies in the Natural Sciences* 48.3, pp. 300–337.

Mani, Inderjeet (2014) 'Computational narratology' in Hühn, P., Meister, J., Pier, J. and Schmid, W. (eds.) *Handbook of Narratology*. 2nd ed. Berlin, München, Boston: De Gruyter, pp. 84–92.

XX.

Geertz, Clifford (1973) 'Thick Description: Toward an Interpretive Theory of Culture' in *The Interpretation of Cultures. Selected Essays*. New York: Basic Books, pp.435–436.

XXI.

Barthes, R. (1977) *Introduction to the Structural Analysis of Narrative, Image-Music-Text.* Trans. S. Heath. London: Oxford.

Lindberg, T. (ed.) (2013) *Strategies and tactics in organic synthesis*. Elsevier.

XXII.

Glasby, G. (2006) 'Abiogenic Origin of Hydrocarbons: An Historical Overview' *Resource Geology*, 56:1, pp. 85–98.

Höök, M., Bardi, U., Feng, L., Pang, X. (2010) 'Development of oil formation theories and their importance for peak oil' *Marine and Petroleum Geology*, 27, pp. 1995–2004.

XXIII.

Gabai, D., & Kerckhoff, S. (2015) 'William P. Thurston, 1946-2012'. *Notices of the American Mathematical Society*, 62(10), pp. 1318–1332.

Jaffe, A., & Quinn, F. (1993) 'Theoretical mathematics: toward a cultural synthesis of mathematics and theoretical physics'. *Bulletin of the American Mathematical Society*, 29:1, pp. 1–13.

XXIV.

Malakoff, David (2002) 'Daniel Pauly profile: going to the edge to protect the sea' *Science*, 296, pp. 458–461.

Manez, Kathleen Schwerdtner, et al. (2014). 'The future of the oceans past: towards a global marine historical research initiative.' *PLoS One* 9.7: e101466.

Mowat, Farley (1984) *Sea of Slaughter*. Atlantic Monthly Press.

XXV.

Abbott, Edwin A. (1884) *Flatland: A Romance in Many Dimensions*. New York: Dover Thrift Edition (1992 unabridged).

Dewdney, A. K. (1984) *The Planiverse: Computer Contact with a Two Dimensional World*. Picador Books.

Grand, S. (1997) 'Creatures: an exercise in creation' *IEEE Expert*, 12: 4, pp.19-24.

White, C. (2018) *Other Worlds: Spirituality and the Search for Invisible Dimensions*. Cambridge: Harvard University Press.

Wise, M. Norton (2017) 'On the narrative form of simulations' *Studies in History and Philosophy of Science* Part A 62, pp. 74–85.

XXVII.

Frodeman, R.L. (1995) 'Geological reasoning: Geology as an interpretive and historical science' *Geological Society of America Bulletin*, 107, pp. 960–968.

Morgan, Mary S. (2017) 'Narrative ordering and explanation' *Studies in History and Philosophy of Science* Part A 62, pp. 86–97.

XXVIII

Wegener, A. (1929) *The Origin of Continents and Oceans*. Translated from the 4th edition by J. Biram (1966), New York, Dover Publications.

XXIX.

Ricoeur, P. (1980) 'Narrative time' *Critical Enquiry*, 7:1, pp. 169–190.

XXX.

Gross, L., Hettinger, A., Moore, J. & Neeley, L. (2018) 'Conservation stories from the front lines' *PLOS Biology*. 16.2: e2005226.