



For further details about the project please visit: [www.narrative-science.org](http://www.narrative-science.org) This work was funded by the ERC under the European Union's Horizon 2020 research and innovation programme (grant agreement No. 694732).

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AN ANTHOLOGY  
OF  
NARRATIVE SCIENCE

Edited by Mat Paskins and Mary S. Morgan.



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# Preface

In the following pages you will find a collection of eight scientific narratives from the last four centuries. They include discussions of the arduous crafts of making steel and constructing carriages; an account of mental illness which combines the perspectives of the patient and his doctors in a style modelled on Chaucer's *Canterbury Tales*; an economist speculating how wealth would be distributed in a newly settled country; and attempts to envisage the branching pathways of chemical synthesis. The variety of subject matters is matched by a range of form in these narratives, even to the extent of narratives being reported in diagrammatic form rather than textual. Each narrative is accompanied by a commentary which aims to bring out its salient features and provide some brief contextual information. These commentaries also provide annotations to unfamiliar terms.

The texts for the anthology were gathered by the team of postdoctoral researchers working with Mary S. Morgan on the Narrative Science Project at the London School of Economics, and the collection was designed and edited by Mat Paskins. The term narrative means many things to different people, and has a tendency to creep: organisations and advertising campaigns have narratives; so do novels, histories and studies in historical sciences like evolutionary biology. So, we argue, do chemical reactions, thought experiments, and mathematical equations.

Although some of our chosen narratives address a public audience, these narratives are not popularising: they do not make existing findings and theories into a story to make them more palatable for non-specialists. Instead they

play a role within scientific argumentation, fleshing out abstract notions, juxtaposing different viewpoints, relating processes which unfold over time, exploring divergent possibilities. These narratives often incorporate contingent and tacit details to an extent that can seem to go beyond their immediate scientific purpose. In arranging the collection we were struck by unexpected overlaps and recurring features in narratives remote in time and from different scientific fields. A natural philosopher from the eighteenth century and a sociologist of science from the twentieth both grapple with the problem of how to put details of their craft practices into words; thought experiments rely on imagining the colonisation of a pristine, previously unsettled terrain, while chemists typically rely on a poetic imaging of their narratives into diagrammatic forms.

Because narrative has so many meanings and applications, readers of this collection may come from a variety of different backgrounds. If you are a scientist, we hope it will offer encouragement to reflect on the range of narrative practices in your own field, on the functions these narratives serve, and on how present-day narratives echo or transform the narratives of the past. If you are an historian, philosopher or sociologist of science, we hope it will provide evidence for the value of considering narrative approaches when you think about the construction and legitimisation of scientific knowledge. If you are a literary critic, we hope that these cases might provide comparisons to the narrative strategies employed in fictional as well as factual literary works, and invite your analysis of how narrative works outside the domain of imaginative literature. And if you are a general reader, we hope that these narratives will give ways to grasp some aspects of the scientific past about which you may not have previously thought.

We do not make any overall claim here about the rise



and fall of narrative knowledge over past centuries. Rather, in our commentaries we highlight the ways in which scientists in the natural, human and social sciences have drawn upon the cultural resources and forms of knowledge available to them in constructing their narratives. These have changed over time, but narrative approaches persist even in sciences which have become very specialised or mathematically formalised.

In reading through the collection you may also think that we missed the most important narratives. Good! We will be producing a second anthology later in 2019, and want to know what our readers think is missing. You can contact the project at [E.H.Narrativescience@lse.ac.uk](mailto:E.H.Narrativescience@lse.ac.uk)

# I.

Jared Eliot was a clergyman, physician and agricultural improver who lived in Connecticut during the colonial era. In this pamphlet he relates the discovery of magnetic black sand near his home, his attempts to discover the sand's properties, and experiments to work it into iron. The sand is presented as a natural philosophical mystery, which also points towards prospects for material improvement and offers little moral fables about the importance of taking risks.

As well as describing tests into material properties, Eliot's account focuses on three social questions. The first is the place of 'projectors'—those who seek to develop new techniques, and take risks to do so. Since the financial crises associated with the South Sea Bubble during the early eighteenth century, projectors had been treated with suspicion and scorn, and many natural philosophers believed there was a need to challenge the claims which projectors made (Stewart 1992). Eliot argues that this is a false way of thinking about the potential value of projection, and dramatises how risks with untried techniques can lead to valuable new discoveries. Part of Eliot's motivation is to defend an ideology of improvement and experiment which is closely associated with colonial settlement. He contrasts his own acceptance of projection—and of failure—with what he considers the failure to develop of American Indians. Eliot's grandfather had attempted to 'improve' local American Indian communities, and had translated the Bible into Natick.

Eliot promotes his projection through interaction with workers. In the excerpt from the narrative given here, he describes the scepticism of the smith he employs to work the sand, and the measures he took to overcome this resistance—including buying a bottle of rum.

He accompanies these descriptions with little parables intended to demonstrate how the social interactions involved in bringing natural philosophy into practice can be compared with other kinds of hope and expectation. Finally, Eliot uses his story to construct an idea of what sociability should involve in more general terms. The material trials are staged in front of an audience, who like the smith are dubious about whether Eliot will succeed. Eliot also reflects on what details are worthy of inclusion in a narrative. He describes shifting into the form of a journal, the better to capture the suspense involved in his uncertain experimental enterprise. This is intended to show that his experiments would be worth undertaking, whether or not they met with success. And he emphasises the importance of including "minute particulars" in writing, as a way to remain connected to reality and not ascend into the clouds. This is a common eighteenth century empiricist position, but notably Eliot draws comparison with the seventeenth century essayist and jurist William Temple, who had related the "common incidents of life". This kind of easy social intercourse is modelled by Eliot's writing and his experimental practice.

Mat Paskins

1762: Jared Eliot, *An Essay on the Invention, or Art of Making Very Good if not the best Iron from Black Sea Sand* (New York: John Holt), 2 -17.

What I have further to write on this Subject, not only for the Sake of Order and greater Precision, but even from Necessity too, will be in the Manner of a Journal. For now entering on the Borders of Terra incognita, can advance not one Step forward, but as Experience my only Pole Star shall direct, am obliged to write as poor Men live, from Hand to Mouth, and as Light springs up before me as I advance.

To trace the Matter therefore from the Beginning; Some Years past viewing this black Sand, the present Subject of writing, with particular Attention, was very much of th Mind that the black Particles were Iron; if it had been tried with a Magnet and was not affected by it, it would not have alter'd my Resolution to try it if an Opportunity should present, having been credibly informed that the Mount Ore of *New Milford*, was not affected by the Magnet, even when it was pulverized.

Having no Opportunity of making a Trial, the Matter rested till the last Year, when my Son erected Iron Works in this Town, in order to refine Pig Iron; upon that I resumed the Purpose of making a Trial, to see if Iron could be made from it; it is true, I might have sent of this Sand to an Iron Work at a Distance: If I had, how unavailing would it have been, where I had neither Interest nor Authority to stem the Tide of Prejudices that there is against Projectors, and the seeming Impossibility of making Iron from Sea Sand? [...]

My Son's Iron Works being erected as I said before;

some Time last November I rode down to the beach, where I found the black Sand had in a great Measure disappeared. I filled my SaddleBags with the black Sand, which I gathered from the Surface of the red Sand, but the Chassery was not then finished, and the Refinery not being fit for such a Purpose, the Trial could not be made, it was therefore necessarily suspended till the first Week in January. The Sand being weighed, which was designed to be used in the Trial, we desired the Workman to use his best Skill to see if he could make Iron from it: Against which, the Workman objected three Things.

1st, That he was not a Bloomer by Trade, but a Refiner, therefore it was out of his Province; but as there was such an Affinity between these two Occupations, we did not doubt his Skill and Ability to do it, if done it could be.

2nd, That notwithstanding the Weight and Appearance of this Sand, he much doubted whether it contained Iron Ore or not.

3rd, He farther observed that if it were Iron Ore, it consisting of such very fine heavy Particles like Flesh without Bones, having nothing to sustain it, would run down among the Coals, sink to the Bottom, full too low, and consequently never gather into a Mass and form a Loop.

This, I must confess, was a formidable Objection, a Difficulty which I had foreseen, and which we knew not how to obviate, otherwise then by adding to the Sand some Cinder or Slag, which had flowed from Pig Iron in the Working; notwithstanding this Difficulty, my Son and I were determined at all Events, that a Trial should be made. Besides the Exertion of Authority, we thought it best to engage the Workman to exert himself, and use his best Skill by motives of Honour and Interest; let him know the Importance of the Undertaking, he being a sober Man, who would use strong Drink with Moderation and Temperance,

I promised him that if he could produce a bar of Iron from that Sand, I would send him a Bottle of Rum. The Workman used great Diligence, Labour, Patience, and as far as I was capable of judging Skill too: As there were a Number of People waiting to see the Issue of this singular Undertaking, who grew uneasy with long Attendance, we diverted ourselves as well as we could. [...]

After some Time found a Substance adhering to Crow-bar, which he shook off, tried with the Hammer, and found it Malleable, says, this is certainly Iron; after five Hours Labour, long look't for came at Last, he drew out a large Loop; it bore Shingling and Hammering well, and when completed the Bar weighed Fifty Pounds and a Half, and proved excellent good Iron; Part of it was tried by the most Skilful of our black Smiths, who pronounced it to be equal to the best of refined or Swedes Iron.

Some may say, that such trivial Stories, and a long Detail of minute Particulars concerning a little Bag of Sand and a Bar of Iron, is really below the Dignity of Writing. I do not know what such Persons intend, by Dignity of Writing, unless they can Mean, that when a Person is to write a Letter or a Book, he must ascend into the Clouds, think himself going about something quite different from the common Actions of Life: That he must divest him himself of what Sociability, that easy Freedom, that Familiarity which is so much the Support and Pleasure of Conversation; must now put on a distant and forbidding Air, assume a solemn Man, a formal Stiffness, as if clad in Buckrum; and being thus equipt is in Appearance like a Hog in Armour, very different from the inimitable Sir William Temple, who relates the common Incidents of Life, in such an easy agreeable Manner, as to engage the Attention, captivate the Mind, and excite the Admiration of every Reader.

## II.

David Ricardo's economic writings are conventionally presented as being the first in classical economics to adopt modernist or 'scientific' modes of argument: abstract, and about an idealised world not the real world. His *Principles* (1817, 1st edition -1821, 3rd edition) certainly offered a new style in the field of political economy, proceeding through textual arguments that include numerical thought experiments. These look deceptively simple, but are designed to cement the text argument. Ricardo's ambition was to "determine the laws of distribution" (Preface), that is, to explain the principles according to which the shares of income were divided between three classes: the owners of land, labour and capital.

He begins his argument about what determines the amount of rent paid to land owners with something close to 'once upon a time': imagining first a new land with plenty of fertile soil, and then exploring what happens if grades of less fertile soil are brought into cultivation as population grows. Each little numerical thought experiment (presented as a farm 'accounting') offers a mini-narrative, but as the overall argument often runs on through several pages, and takes off into different side possibilities ("the more skilful rotation of crops", or "better choice of manure"), the set taken together has something of a narrative structure, with side channels for trying out and illustrating alternative scenarios.

The narrative's punchline: "he obtains a greater share [of the total to be distributed], and secondly the commodity in which is paid is of greater value." tells of the landlord's double gain from rent: he gets more rent (because more capital is expended on his land, so yields have gone up), and the value of the corn unit in which

rent is paid in his accounts has also gone up in value (as more labour is used on his land). This remains mysterious unless the reader has worked through the full set of examples, that is, through all the little narratives-cum-thought experiments, and connected them up, to understand the final outcome from the overall narrative.

Ricardo was a considerable landowner, who took much interest in his estates, and his arguments on cultivation can be linked to the debates at the time over spade husbandry: the relative productivity of men digging with spades vs horse plough cultivation, and the use of such labour-intensive techniques in solving rural unemployment put forward by the socialist Robert Owen. The form of his numerical simulations are found paralleled in evidence to the contemporary House of Lords enquiry into agriculture, in letters by farmers reporting their own actual experiments to the *Farmer's Weekly*—presented here in an example which follows Ricardo's text—and to an important tradition in agricultural experiments in chemistry.

Mary S. Morgan

1817: David Ricardo, *On the Principles of Political Economy and Taxation*, (London: John Murray) 52 -57

On the first settling of a country in which there is an abundance of rich and fertile land, a very small proportion of which is required to be cultivated for the support of the actual population, or indeed can be cultivated with the capital which population can command, there will be no rent; for no one would pay for the use of land, when there was an abundant quantity not yet appropriated and, therefore, at the disposal of whosoever might choose to cultivate it.

It is only, then, because land is not unlimited in quantity and uniform in quality, and because in the progress of population, land of an inferior quality, or less advantageously situated, is called into cultivation, that rent is ever paid for the use of it. When in the progress of society, land of the second degree of fertility is taken into cultivation, rent immediately commences on that of the first quality, and the amount of that rent will depend on the difference in the quality of these two portions of land.

When land of the third quality is taken into cultivation, rent immediately commences on the second, and it is regulated as before, by the difference in their productive powers. At the same time, the rent of the first quality will rise, for that must always be above the rent of the second, by the difference between the produce which they yield with a given quantity of capital and labour. With every step in the progress of population, which shall oblige a country to have recourse to land of a worse quality, to enable it to raise its supply of food, rent, on all the more fertile land, will rise.

Thus suppose land No. 1, 2, 3; to yield, with an

equal employment of capital and labour, a net produce of 100, 90, and 80 quarters of corn. In a new country, where there is an abundance of fertile land compared with the population, and where therefore it is only necessary to cultivate No. 1, the whole net produce will belong to the cultivator, and will be the profits of the stock which he advances. As soon as population had so far increased as to make it necessary to cultivate No. 2, from which ninety quarters only can be obtained after supporting the labourers, rent would commence on No. 1; for either there must be two rates of profit on agricultural capital, or ten quarters or the value of ten quarters must be withdrawn from the produce of No. 1, for some other purpose...

It often, and indeed, commonly happens, that before No. 2, 3, 4, or 5, or the inferior lands are cultivated, capital can be employed more productively on those lands which are already in cultivation. It may perhaps be found, that by doubling the original capital employed on No. 1, though the produce will not be doubled, will not be increased by 100 quarters, it may be increased by either five quarters, and that this quantity exceeds what could be obtained by employing the same capital on land No. 3.

In such case, capital will be preferably employed on the old land, and will equally create a rent; for rent is always the difference between the produce obtained by the employment of two equal quantities of capital and labour. If, with a capital of 1000l., a tenant obtain 100 quarters of wheat from his land, and by the employment of a second capital of 1000l., he will obtain a further return of either five, his landlord would have the power at the expiration of his lease, of obliging him to pay fifteen quarters, or an equivalent value, for additional rent; there cannot be two rates of profit. If he is satisfied with a diminution of fifteen quarters in the return for his second 1000l., it is because no



employment more profitable can be found for it. The common rate of profit would be in that proportion, and if the original tenant refused, some other person would be found willing to give all which exceeded that rate of profit to the owner of the land from which he derived it.

**From the Farmer's Journal, Letter from A. L, Tetbury,  
April 26, 1817, "Method of Employing the Agricultural  
Poor"**

Several portions of land in a large field, in equal divisions, were marked out, and all planted with potatoes of the same kind, the same soil, the cultivation the same in every respect, except that in one division no manure was put on the ground before planting with potatoes. All the other divisions were manured with different quantities of manure, progressively increasing from ten cartloads per acre up to forty, which was the highest quantity put on any division; the consequence was, that the crop without any manure, cost £6 per acre, including rent, &c., and produced 24 sacks per acre, which sold, at 5s per sack, for exactly £6: and, therefore, left no profit whatever for the grower, or interest for his capital employed. The other divisions produced from two and a half to four sacks additional for every additional cart-load of manure (which was chiefly sweepings of the streets of a town, and cost 5s the load when on the ground); and the highest, manured at 40 loads of manure to the acre, yielded 160 sacks of Potatoes per acre, which at 5s per sack is £40, or 150 per cent profit.

### III.

Authors of scientific books often use their prefaces to set out their stall, and advance a particular agenda. In this excerpt, William Bridges Adams incorporates an extensive autobiographical narrative into his discussion of how carriage construction can be reformed according to mechanical principles, and the ways in which this connects with the increasing market for luxuries (including carriages) in London. Adams married Elizabeth Place, daughter of Francis Place, in 1819; the following year they travelled together to Chile, where Adams was supposed to manage the estate of Lord Cochrane.

Adams was a prolific contributor to the periodical press of the 1830s, publishing articles on topics ranging from political economy to mechanical improvements, to satirical squib about the invention of a hat so waterproof that its owner would never need to purchase a replacement. Many of his interventions were published under the pen name Junius Redivivus. Junius had been the name of an anonymous correspondent to newspapers during the 1760s and 70s, who wrote to expose corruption and endorse liberty. Adams self-consciously modelled his own productions after those of the earlier Junius. His style attracted the attention of the liberal philosopher and economist John Stewart Mill, who was searching for a model for periodical authorship, and found it in Adams' works (Camlott 1999). Mill wrote that Junius Redivivus was "the very best popular writer whom the enlightened radicals count in their ranks—though I like his *personal* articles in the *Examiner* less than the many admirable papers he has written in the *True Sun*, *Mechanics Magazine* & various other periodicals" (Mill 1833).

Historians have seen *The Mechanics Magazine*, one of Adams' favoured venues for publication, as embodying a "low" scien-

tific culture, in contrast with more elite publications (Sheets-Yenson 1985). Adams' example suggests, however, how artisanal mechanical writers of the 1830s situated themselves in relation to more established scientific publications. Adams alludes to the immense volume of information about specific manufactures collected in Charles Babbage's 1832 book *On the Economy of Machinery and Manufactures*, and models his own compilations of coachbuilding practice on Babbage's book. Like Babbage, he also promises to take his readers beyond existing and unthinking craftsmanship, to a more enlightened mechanical practice which recalls the one he has discovered as a result of the contingencies and opportunities of his own wandering career. Just as Adams' apparently aimless experiences have instructed him (more or less by accident, although also through the enlightened behaviour of his teachers) in an appropriate melding of theory and practice, so he will do the same for his readers. But he will not pretend that carriagebuilding can be reduced completely to a set of rules or minor details.

Controversies on technical and rather arcane subjects such as the appropriate shape for the teeth of millwheels often became remarkably vituperative—shown here in letters from the *Mechanics Magazine*, which follow the excerpt from Adams' text. Adams participated in these battles with great relish; his book on carriages is written in a quieter register, presumably in the hope of attracting clients as well as appealing to members of the trade.

Mat Paskins

1837: William Bridges Adams, *English  
Pleasure Carriages: Their Origin, Histories,  
Varieties, Materials, Defects, Improvements and Capabilities,* (London:  
Charles Knight & Co.), iii -x.

THE object of this work is general utility, rather than technical instruction in minor details. The principles on which carriages ought to be constructed, rather than the arbitrary proportions of parts, are what the author has sought to make clear. The utmost care used in giving accurate measurements, with considerable labour to the writer, and more to the reader, would fail in enabling a mere copyist to construct a good carriage, and a builder with clear ideas would not need them. In architecture, there are certain rules laid down by which all proportions may be regulated; but in carriages, so many considerations intervene, as to produce uncertainty, what actual strength may be required. Seasons, localities, weight, speed, and many other circumstances, conspire to defy precise calculation. The builder of a steam engine knows to a fraction the amount of strain his work will have to bear; but the carriage builder can only take average proportions to meet varieties of circumstances, unless his vehicle be intended for a railroad.

The writer makes no pretension to set a complete work before the reader; circumstances rather than original intention have caused the book to be produced, just as Mr. Babbage in the pursuit of a specific object acquired a mass of general knowledge, which he afterwards gave to the world, for the benefit of those who might not have the opportunity of collecting it as he had done. The writer, when a boy, was “brought up,” in the usual phrase, to the “art and

mystery of coachmaking," and acquired the common amount of routine skill in imitating certain prescribed forms; but as to the principles of the art, as to the mode of originating the forms, he was left in utter ignorance. In fact, those to whose instruction he was confided were as ignorant as himself. Had the patterns of the factory been destroyed by accident, the business must have stopped, unless external talent had been brought in to renew them. Impatience of mere routine excited in the writer the desire of becoming familiar with other branches of the art, and he acquired something of skill in many varieties of work. Circumstances afterwards brought him into connexion with an eminent engineer, and a new world seemed to open upon him, gorgeously decked in all the wonders of machinery. The minds of most boys are essentially inclined to the material; they prefer the real to the imaginative, utility in art, to taste or splendour in art, and feel far more interest in Robinson Crusoe's canoe than in a Lord Mayor's barge. Thus, after becoming familiar with the properties even of simple machines, the inferior mechanical construction of carriages became an object of contempt to the writer; and taste for beauty of form not having been awakened in him, he ceased to feel any interest in them. His daily occupation became a drudgery to him, to which almost any other employment seemed preferable; and the simple reason was, that he had no scope for improvement, no exercise for advancing intelligence. The motive of the writer in recording this is, that the same mischievous system still prevails, though opportunities for selfimprovement are more numerous than before. Ere he had well attained mature years, the writer was obliged by ill health to seek a more genial climate than his own, and he passed several years alternately sojourning and travelling in various countries, where

perceptions of the beautiful in Nature and in Art gradually began to stir and awaken his dormant faculties. Being thrown much on his own resources in mechanical difficulties, his invention was frequently tasked in various ways; and the excitement of overcoming obstacles served to stimulate his perseverance, and quicken his observation as to mechanical capabilities.

When the writer returned to England with renewed health, he entered into partnership with his father, in the firm of Hobson and Company, of Long Acre. For a considerable time he applied himself to the mercantile department of the business; after which, peculiar circumstances gave him leisure to pursue systematically a series of mechanical improvements, as well as to study the principles of the art of vehicular locomotion. He sought for books to aid him; but the knowledge to be gleaned from them was so imperfect, and so extensively scattered, so devoid of all system, and so very unsatisfactory, that he chose rather to seek the practical schools of the workmen, and the verification of experimental research, and thereon to build his own theories. Having accumulated the results for his own satisfaction, it struck him that information acquired with so much trouble must be valuable to a large class of readers, both the constructors and purchasers of carriages. Farther reflection convinced him that a succinct detail of the progress of wheel carriages in England from the earliest period down to the present time would be a desirable addition, and the result has been the publication of the work now offered to the reader.

Whatsoever may be the imperfections, the writer cannot doubt that, in the absence of any existing available work on the subject, the information herein collected will be esteemed valuable by the large and increasing class of

those who keep carriages, whether for their own use or for the purpose of hiring out to the public. Whoever is about to build a house endeavours to procure drawings and models beforehand, to make sure that his fancy may not lead him astray; and carriages being articles of considerable expense, it must surely be desirable to acquire such knowledge as a book can give on the subject, previous to making a purchase. To constructors, this work must be useful, even to those who might feel inclined to contest any of the positions the writer has laid down; for it will give them the opportunity to verify anything which they find not borne out in practice. It may perhaps induce some one of greater skill than the writer to put forth another work. He would be glad to hail such a work as might be worthy of being entitled *The Science of Vehicular Locomotion*. In the meantime, constructors residing at a distance from the metropolis, and those of foreign lands, will probably avail themselves of the opportunity of knowing the existing state of the art of carriage construction in the English metropolis, where it is generally supposed to have attained its nearest approach to perfection. If there be any constructors so illiberal as to suppose this injurious to their interests, they will do well to reflect that ere foreigners can rival English constructors, they must train their workmen in every branch, direct and indirect, to possess greater skill than English workmen. The circulation of English models on the Continent would tend perhaps to improve the Continental manufacture; but they would to a greater extent tend to increase the already extensive taste for English carriages. As this work may occasionally fall into the hands of readers who feel no direct interest in pleasure carriages either as purchasers or constructors, some may perchance be inclined to question its utility, except as it relates to the mechanism of

vehicular locomotion. They may perchance condemn pleasure carriages altogether, as luxuries unjustly engrossed by wealthy people. This would be as unjust as the sweeping condemnations so frequently passed on the public vehicles in the streets. Pleasure carriages are instruments of elegant human enjoyment; and to say that none ought to enjoy them because all cannot enjoy them, would be to possess a spirit akin to that of the “dog in the manger.” If those who possess pleasure carriages were to enjoy them at the expense of those who do not possess them, the evil would be a monstrous one: but such is not the fact. Partial instances might be adduced; but they are only exceptions to the rule, as, for example, the case of a swindler. But, after all, those who do not use pleasure carriages as the mass of the community—though they do not benefit directly, still benefit indirectly by them. They are objects of beauty constantly exposed to public view, as much so as architectural erections—perhaps more so, being locomotive, and far more so, though in a humbler way, than paintings or statues. It is an undeniable fact, that the daily habit of beholding beautiful objects has an imperceptible effect in refining the national taste. Pleasing colours and pleasing forms tend to soften rugged natures; and whatever may be the disputes as to the evil or good resulting to a nation from the existence of a permanent leisure class, it is quite certain that if no part of the community be at leisure to study the arts of elegance, the public will be far less refined, far more devoid of taste, than if they have a standard type, as it were, to imitate, when any individual may acquire the means to imitate, without understanding the principles of that which he imitates.

In most branches of art, men of vulgar minds are to be found who deem that art is on the decline because works

of art are progressively diminishing in price ; forgetting that an increasing demand more than compensates for a decreasing price. Amongst carriage constructors men of this class may be found, who prophesy the decline of carriage building in England, because they cannot maintain a lazy monopoly. They obstinately shut their eyes to the fact that the streets of London are now occasionally blocked up with pleasure carriages, as they frequently are with carts. The simple fact is, that ordinary skill is now common, and carriage builders have become a more numerous body. It is a more difficult thing than formerly to stand out from the crowd, and it can only be accomplished by combined talent and energy. The arts of elegance, or luxury, as they are sometimes called, will be pursued with more earnestness than ever as people advance in wealth; and if there be a fear, it is, lest the arts of wisdom be neglected for the pursuit of external enjoyments, as is so much the case in the wealthy cities of the United States.

Porchester Terrace, Bayswater,

June, 1837.



FROM A LETTER TO THE MECHANICS' MAGAZINE, 17  
January 1835

The members of the Society of Industry, Manufactures, &c., of Paris, are about 260 in number; and include some of the most eminent men of science and mechanics in France. On the other side we have "Junius!" Then, in the name of wonder, who is "Junius", that his solitary opinion should outweigh the opinions of a whole society? Have we seen any proofs of his mechanical knowledge in the Mechanics' Magazine?—No! Have we seen any proofs of his mechanical knowledge in the Monthly Repository?—No! Have we seen any proofs of his mechanical knowledge in the Producing Man's Companion?—Alas! "Junius," the echo of the last answer is just in time to reply to this question also with a—"No!" Where, then, shall we find proofs of the great mechanical knowledge that is to outweigh the opinions of so many individuals? Answer, "Junius," or the public will assuredly prefer the opinions of the learned men of Paris to yours.

Birch Rod

FROM A LETTER TO THE MECHANICS' MAGAZINE, 14  
february 1835

"Who is Junius, that his solitary opinion should outweigh the opinions of a whole society?" *Answer.*—A voice, and nothing more. My voice is transferred to types and speak [sic] in these pages. I gave an opinion on an axle, but I did not in any way vouch the correctness of that opinion. Had that opinion been quoted by another person, as a proof of the excellence of any mechanical subject, then it would have been a fair question to ask "who is Junius." [...] "Birch Rod" seems to consider that I have no claim to mechanical skill, beyond "using mechanical drawing instruments"—that I do not "deal in *Practical* details." He is in error. What I write is founded entirely on *practical* knowledge. Mechanical science frowned upon my birth, and all the mechanical theories I possess have been built up in my own brain, nurtured by the experience gained out of my hardened hands.

Junius Redivivus

## IV.

In this observation Eugène Azam recounts the case history of a Bordelaise woman, dubbed Félida X, who suffered from what was variously labelled as double consciousness, split personality, or periodical amnesia. Azam first examined Félida in 1858, then returned to study her in 1875 with a view to publishing her case, which he did in a number of scholarly venues from 1876. With its communication to the Académie des sciences morales et politiques in May 1876, Azam, then a professor at the Bordeaux Medical School, came to wider scholarly prominence, and his text quickly appeared in the popularising scientific weekly, *La Revue scientifique*—source of this extract, and in contemporaneous translation.

In the months, then years, that followed, Félida's case became a source of evidence and an almost ubiquitous point of reference in debates among philosophers, physicians and aspiring "scientific" psychologists, out of which an autonomous discipline of psychology emerged in France. It added to descriptions of hysteria, seemed to disturb a notion of the unified self, and constituted an exemplar for various forms of amnesia and somnambulism. At its core was the observation that Félida switched periodically from her *état normal* (normal state), in which she was serious, hardworking, taciturn, into another state, the *condition seconde* (second condition), thereupon becoming more vivacious and affectionate. When in her normal state, Félida had no memory of anything that occurred during her periods of *condition seconde*, while the reverse was not true.

Beyond the singularity of the case, what made Azam's observation such a valuable source of scientific evidence was the richness of its narrative structure. In this extract, an account of Félida's states is intertwined with an almost equally detailed

narrative of the circumstances, reflections and procedures that shaped Azam's research into her case. He becomes a full character in the observation, here narrating early reactions to informal discussions with colleagues about Félida. Set against general scepticism, the support of one eminent colleague appears as a critical branching point in the history of hypnotism in France. That Bazin handed Azam an English book on "nervous sleep" is a contingency tied to the particularities of Azam's research discussions and networks. But weaving this episode into Félida's case history allows Azam to signal the case's importance, and also to configure it into larger events in the development of "psy" sciences in France. This larger narrative portrays Félida as the originary subject for Azam's 1860 research into hypnotism, which attracted considerable attention at the time. Concomitantly, Azam asserts his priority in an increasingly significant field of enquiry—Charcot's interest in hypnotism dates from the mid-1870s—while also reminding readers of his previous valuable contributions to the progress of medico-psychological science in France.

Azam moves from recollecting Félida's founding role in the study of hypnotism to a typical episode of hypnotic experimentation. The change is marked by a shift into present tense narration, indicating a set of procedures and symptoms which follow a regular pattern. Some details remain particular to Azam (who acts in the first person) and Félida (in the detail about states), and these are narrated as part of a causal sequence. Where Félida's reactions follow a wellknown pattern (e.g. as discussed by Azam in his 1860 article), the text shifts to list form, presenting a series of typical hypnotic phenomena. For readers in the know, this list provides a (cognitive) script, inviting them to recreate the experiment in imagination, on Félida, or even maybe on a chook.

Kim M. Hajek

1876: Eugène AZAM. "Académie des sciences morales et politiques. Amnésie périodique, ou doublement de la vie." La Revue scientifique de la France et de l'étranger X, no. 47 (20 mai 1876): 481 -89: 483.

« A cette époque (1859), je racontai ce fait à divers confrères la plupart me crurent le jouet d'illusions ou de tromperies ; seuls, trois hommes éminents, après avoir vu Félicité X\* avec moi, m'encouragèrent dans son étude : Parchappe, le célèbre aliéniste ; Bazin, médecin en chef de l'asile public des femmes aliénées et professeur à la Faculté des sciences de Bordeaux, et M. Gintrac père, directeur de l'École de médecine et correspondant de l'Institut. Pour tous les autres, la science était faite, et tout ce qui est en dehors du cadre connu ne pouvait être que tromperie.

Pour ces esprits d'élite elle était à compléter en ce qui touche à l'étude si délicate des fonctions du cerveau, et aucun fait ne devait être négligé. M. Bazin me mit entre les mains un livre presque inconnu en France, *Crypneurology, or the nervous sleep*, de Braid, où l'hypnotisme est décrit ; c'est la lecture de ce livre qui fut l'origine des recherches qui occupèrent le monde savant à la fin de 1859 et que j'ai résumé en 1860 dans *les Archives de médecine et de chirurgie* et dans *les Annales médico-psychologiques de Paris*. Ces recherches signalées par Velpeau à l'Institut ont été confirmées par MM. Broca, Follin, Verneuil, Alfred Maury, Baillarger, Lasègue, etc...., et ne sont tombées dans une sorte d'oubli que par suite de leur malheureuse analogie avec les pratiques justement décriées du magnétisme animal.

C'est sur Félicité X... et particulièrement sur une de ses amies, Maria X..., que j'ai fait les expériences qui ont été la base de cette étude, laquelle après Braid et nombre d'au-

teurs anciens a établi l'action du strabisme convergent sur les fonctions cérébrales, tant chez l'homme que chez les animaux.

Pour ne pas sortir de mon sujet, je ne décrirai que ce que j'observai sur Félicité X... en ce qui touche à l'hypnotisme : Félicité étant dans l'un de ses deux états et assise en face de moi, je l'invite à regarder attentivement un objet quelconque placé à 15 ou 20 centimètres au-dessus de ses yeux ; après huit à dix secondes, elle clignote et ses yeux se ferment. Pendant quelques instants elle ne répond à aucune question, le sommeil dans lequel elle paraît être la séparant complètement du monde extérieur de plus elle est anesthésiée— après ce temps très-court elle répond aux questions posées et présente ce fait particulier, que dans ce somnambulisme provoqué et quel que soit son état au moment où elle a été endormie, elle est toujours dans l'état normal.

Alors elle présente les phénomènes ordinaires de ce somnambulisme, catalepsie, anesthésie, hyperesthésie de la peau, développement exagéré de l'odorat, du toucher, exaltation du sens musculaire, tous phénomènes faciles à produire par le procédé indiqué même sur les animaux (poules, chats) et sur lesquels je n'ai pas à insister ici. »

The coherence of Azam's narrative diminishes, however, in this English translation that circulated at the time (the only full published translation of the observation to date), especially when it comes to the broader history of hypnotism. A 'false friend' sees '*resumer*' translated as 'resumed', not 'summarised', and a rather rapid parsing of Velpeau's actions mean that Azam's priority is no longer communicated clearly. Velpeau then appears as the key researcher working on hypnotism in 1860 rather than as key supporter, as the Academy member who read Azam's work on his behalf (in the usual procedure). The narrative accordingly shifts to present Azam as a follower of others' scientific developments, and blurs Félicité's key role in the lineage of hypnotism research in France. The overall effect is to create gaps in the narrative fabric of Azam's observation; the Bazin book episode is notably disconnected from the main story. For us, critics of the 21st century, it also signals the way translation practices can influence narratives as they circulate, reminding us not to neglect language when we examine the history of science.

1876: Eugène Azam, translated by James I. Tucker. "Periodical Amnesia; Or, Double Consciousness." *Journal of Nervous & Mental Disease* 3, no. 4: 584 - 612: 590–91. [Annotated by Kim M. Hajek].

“At this time, (1859) I related this circumstance to several confrères. Most of them believed me to be the victim of delusion or imposture. Only three prominent men, after having seen Félicité X. with me, encouraged me to study her case. Parchappe, the celebrated alienist; Bazin, physician chief to the public asylum for women, and Professor in the Faculty of Science in Bordeaux, and M. Gintrac, sen., director of *l'Ecole de Médecine*, and correspondent of the Institute. With all the others, science was complete, and everything which was outside its prescribed limits, could be nothing but delusion. [For all the others, science was complete, and anything outside its known limits could only be trickery.] For these advanced minds, this case contributed to complete that part of science which pertains to the delicate study of brain functions, and no fact should be overlooked. M. Bazin put into my hands a book, almost unknown in France; viz., “Braid’s Neurohypnotism, or the Nervous Sleep,” wherein hypnotism is described. It is the reading of this book which gave origin to those researches which occupied the learned world at the close of the year 1859, and which I resumed [summarised] in 1860, in the *Archives de Médecine et de chirurgie*, and in the *Annales médico-psychologiques de Paris*. These remarkable researches by Velpeau, in the Institute, [These researches, communicated by Velpeau to the Institute], have been [were] confirmed

by MM. Broca, Follin, Verneuil, Alfred Maury, Baillarger, Lasègue, etc., and have fallen into a sort of neglect only, in consequence of their unfortunate analogy with the practices of animal magnetism, which have been justly denounced.

It is upon Félicité X\*\*\*, and particularly upon one of her friends, Maria X\*\*\*, that I have made the experiments which, form the basis of this study, that according to Braid, and numerous other older authors, has established the action of a convergent strabismus up on the cerebral functions as much in man as in animals.

In order not to depart from my subject, I will [only] describe what I have observed in Félicité, pertaining to hypnotism, Félicité being in one of her second conditions [one of her two states], and seated opposite to me, I asked her to look attentively at an object, placed 15 to 20 centimetres above her eyes. After eight or ten seconds, she winked [blinks] and closed her eyes [her eyes close]. For a few moments she did not answer questions, the sleep in which she appeared to be, separating her completely from the external world. She is anaesthetic; after a short time answers questions, and presents this peculiar phase [particular fact], that in this induced somnambulism, or whatever may have been her condition when she was asleep, she is always in the normal state. [fact, that in this artificial somnambulism, this whatever her state was at the point when she was hypnotised, she is always in the normal state]. Then she presents the ordinary phenomena of somnambulism, catalepsy, anaesthesia, hyperaesthesia of the skin, exalted sense of smell, touch; exaltation of the muscular sense; all of these phenomena easy to produce by the procedure indicated, even in animals (hens, cats, etc.) upon which I will not dwell in this place [all phenomena easy to produce by the procedure indicated, even in animals (hens, cats), and on which I have no need to insist here].”



## V.

This unusual case history is drawn from Edward B. Angell's (1856–1947) practice in Rochester, New York, where he worked as a neurologist for around 50 years over the turn of the 20th century. While double consciousness was not common, there was significant American interest in the condition (also known as double personality) particularly from the 1880s onwards. Nor was it yet rare, neither in turn-of-the-century American psychology nor in the new *Journal of Abnormal Psychology*, to find detailed accounts of a single case, as well as more synthetic psychological studies

It is the narrative form of Angell's case which distinguishes it from other case histories of the period. In structuring his case history as a "modern collection of Canterbury tales" (p. 156), Angell self-consciously plays with the narrative conventions of the neurological (or psychological) case. Most notably, he separates out the distinct strands of expertise and witnessing that are more usually intertwined into a coherent whole by an authoritative narrator/author. This has the effect of raising the textual status and visibility of Angell's informants even when their stories are not narrated in the first person (e.g. the aunt's tale). These other 'tellers' become almost as important as the case's author, Angell, a point that is marked by labelling all the accounts in the same way: as "tales".

But a "tale" in English can be a 'tall tale', or one told for purposes of narrative competition, as in the *Canterbury Tales*. Inconsistencies and competition between others' "tales" and Angell's own interpretation of events and phenomena correspondingly tends to lessen the overall coherence of the case history. In some cases the reader is likely satisfied as to the "correct" view, for instance when the aunt reveals the patient's real name. But others remain ambiguous: how are we to resolve the contradiction

between the patient's 'guarantee', in his final tale, of having attended McGill University, and the neurologist's refutation of this point? And if the patient's earlier tales were formed from "unconscious falsification of memory" (p. 168), was his final tale in any way shaped by the suggestions given to this "very susceptible", hypnotised subject (see Carroy, 1996).

Either way, the reader must confront the possibility that some narrators in this scientific text are unreliable. If this makes Angell's text akin to modernist fiction, it also has implications for the role of narrative in scientific reasoning. On the one hand, as readers encounter ambiguities and contradictions in the successive tales, without being "corrected" by Angell's interpretation (until the end), they are invited implicitly to work through the process of 'adding in and throwing out' relevant information (Ankeny, 2011). That is, the narrative structure of Angell's article prompts readers to replicate the kind of reasoning undertaken by the neurologist. The narrative remains open. And since most of Angell's readers would have been neurologists or psychologists, they may very well have reached different interpretations (see Hacking, 1996, p. 36).

On the other hand, competition between the various tales means that ambiguity also pervades the neurologist's own tale. That does not necessarily make it a scientific narrative to be discarded. On the contrary, by separating Angell's tale from the patient's, wife's, etc., this case history points to the way narrative knowing is crucial to scientific enquiry in neurology/psychology: First, by highlighting the multiple layers of "telling" that go into "knowing" about a particular condition; then, by signalling the power (even necessity) of narrative to "fill in gaps" (p. 168), and otherwise to make 'plausible' connections, between disparate pieces of information.

Kim M. Hajek



1906: E. B. ANGELL, "A CASE OF DOUBLE CONSCIOUSNESS -Amnesiac Type, with FABRICATION OF MEMORY" *The Journal of Abnormal Psychology* 1 No. 4: 155 -169

THE subject of the present sketch, a frank, open-hearted Englishman of some twenty-five years of age, was introduced to me late in January of the present year by his physician, Dr. H. J. Vary, of Rochester, to whom I am greatly indebted for much of my material....

THE PATIENT'S FIRST TALE My home was in Musselburgh, near Edinburgh, Scotland.... I was called to England last October, sailing from New York on the Kaiser Wilhelm. I became ill during the voyage and remember nothing until awaking one morning in the Edinburgh Hospital. I was told that I had had brain fever; that I had been taken from the ship at Plymouth, and brought to Edinburgh by Dr. Macdonald, my brother-in-law, and placed under the care of Dr. Black.... I was married on Christmas Day, and on the 26th fell ill and called Dr. Vary, under whose care I was for a few days.... As to what happened subsequently I am quite hazy, but distinctly remember being registered at the Grand Union Hotel in New York, of going from New York to Buffalo, via Lehigh Valley Railroad, of being in Canandaigua and various other places, and of finally coming to, in Sayre, Pa., .... Dr. Vary came to Sayre and we returned together on January 21.

THE DOCTOR'S TALE Doctor Vary was called to see the patient on December 26, 1905.... He was depressed and mind clouded. I regarded these conditions as being due to the excitement of the previous day. He was irrational at

times... January 9: Patient left home about 10 A.M. to stop at my office and do two errands and to return home for his wife, who was to accompany him down town. He failed to call at my office, but did the errands. That was the last we saw or heard of him until January 19 (ten days), when his wife received a letter from him. This was written in pencil, postmarked Sayre, Pa., and headed " Memorial Hospital," Sayre, Pa. He wrote that he had just come to his senses to find himself there under the care of Dr. Fox....

**THE AUNT'S TALE** On February 14 the patient's aunt, Miss C., of England, reached Rochester, and gave us in his presence the following unquestionable facts regarding the patient's life: His name is not Robbins, but Horace Rawlins. ... Young Rawlins left home some twelve years ago, since which time he has not been home, although Mr. Rawlins asserts he has been in England three times....

**PATIENT'S SECOND TALE** In 1900 the patient himself (Horace Rawlins) and a Horace Robbins graduated from McGill University in engineering courses and both went to Winnipeg, Manitoba, to accept positions. Both were taken ill with some " fever." Robbins was taken first, became unconscious and died, just as Rawlins became unconscious.... Rawlins left Winnipeg to recuperate, and from that day has been called by the name of Robbins, and all the history given by patient belongs not to himself, but to Robbins, who is dead.... Upon writing to McGill University we learned that neither patient nor his friend Robbins ever attended the University...

**THE WIFE'S TALE** "I met my husband in July, 1905... failed to receive any news of him whatever until November, when I received a telegram which was sent from St. Cath-

rine's, Ontario, and signed presumably by a cousin of his, telling me that my intended husband was ill with brain fever in Edinburgh, Scotland. ... On January 9 he [my husband] was going down town to do some business, and while at breakfast felt very sleepy and could not keep awake. He left the house at about ten o'clock, and that was the last I saw of him until Dr. Vary brought him to the hotel in Sayre about two weeks later....

THE PATIENT'S FINAL TALE, WRITTEN BY HIMSELF SINCE THE FULL RECOVERY OF HIS NORMAL MENTALITY. My name is Horace Rawlins, and my age is twentyseven years.... My family, consisting of my mother, two brothers, and two sisters, at present reside at Cedar Road, Hampton Wick, Middlesex, England. They never were in Scotland.... not finding a position, I went to Montreal, where I worked until October, when I went to McGill University.... These statements I can vouch for and can prove, so I write them knowing they are absolutely true.

The statements made below are presumably of events that have occurred since my leaving Montreal, although I cannot vouch for them all, so I write them knowing that they may be either fact or fancy.... I went to Winnipeg with a Mr. Horace Robbins, who was the son of a colonel in the British Army. Mr. Robbins was taken ill a day or so before I was and died in a day or two, and I have apparently been living his life since that time, until the past winter. [...]

I met a friend of mine at the New York Central station in Rochester on the evening of the day I left home, and have seen him since, and he tells me that I was very drunk the night that he saw me in January. This must have been due to the mental condition I was then in, as I have never to my knowledge touched alcoholic beverages of any kind

whatever.... From the time I first saw Dr. Angell, he, Dr. Vary, and my wife all seemed to exercise a great influence over almost everything I did. I was seemingly easily moved by suggestions of either of them....

**THE NEUROLOGIST'S TALE** My own examination of Mr. Rawlins, begun on January 22, at his home... Indeed, his general symptoms were such as are commonly met with in hystericals.... I found him very susceptible to hypnosis. Within three minutes or less he was in the lethargic stage, responding to any idea presented to him....And a dreamer of dreams he was. Both Dr. Vary and myself are satisfied that his tales are but creations of an unstable imagination. His name is not Robbins, it is Rawlins; he never was a student at McGill University; he never was in a hospital at Winnipeg. His friend Robbins was a myth....he did not sail on the Kaiser Wilhelm last October, nor was he ill in Edinburgh....He did not go to New York; he was not in Buffalo. ...There is a subjective, unconscious falsification of memory, a species of amnesia, for the real events of an uneventful existence, and the gap is filled with visions, with real unrealities, with plausible impossibilities.



## VI.

In immediate reaction to J.M. Keynes' *General Theory* of 1936- which revolutionised ideas about 'the economy' (our aggregate national economy) young economists tried to investigate the mechanism of Keynes' new recipe for getting the economy out of the Great Depression of the 1930s. To do so, they translated Keynes' 'theory' into little mathematical models, and investigated how the recipe would work. This exploration, by Paul Samuelson, is one of the first, and to see what would happen he used a process now called simulation, using arithmetic calculations by hand (for 1939 was the pre-computer age). The key point from Keynes' recipe was that government spending 'stimulates' economic activity: its spending is 'multiplied' by extra consumption and extra investment activities of others in the economy; thus the initial government spending creates economic growth, and so will be good for solving the depression.

There are two kinds of narratives here. One is the narrative of the economist as he does the research and reacts to initial findings to try other things: the research narrative. The other kind is found in the sequences of model outcomes, shown in the Tables. Table 1 reveals what happens as the economy develops through the time periods (downwards) following successive one units of spending by the government. (This sequence can also be seen in Chart 1, reading horizontally.) This results in a somewhat surprising pattern in which 'total national income' (ie, the income of the economy as a whole) creeps through a low level cycle in overall activity and then returns to its base level.

Table 2 reports Samuelson's exper-

iments with the same model, but different values of the parameters as reported in the columns. These columns show sequences in which: the economy expands slowly to a fixed level; exhibits different kinds of cycle; or, in the final column, explodes upwards. Samuelson expressed his surprise at such 'capricious' results, capricious not just because of their variability, but also because of the unlikely outcome of, for example, an exploding economy.

Conventionally, the columns of these tables don't look like narratives: they are not texts- they show not tell. But in other respects these tables fulfill narrative scholars' requirements for something to be a narrative: they offer time sequences in which changes are evident, and the dynamic for those changes are found in the dependency relations evident in the model's equations. Indeed, these equations represent behavioural reactions (causal reactions) between the groups of actors in different sectors of the economy. But, these tabular results are more significant than just arithmetic sequences each offers a narrative that can be told with the model; and the set can be understood as the typical set of narratives that can be told with a Keynesian model, offering a range of policy outcomes with the same policy. These narratives are as critical as the model equations in providing something like the 'identity characteristics' of the Keynesian model. (Morgan 2001).

Mary S. Morgan

1939, PAUL A. SAMUELSON, "INTERACTIONS BETWEEN the Multiplier Analysis and the PRINCIPLE OF ACCELERATION", *The Review of Economics and Statistics* , vol. 21, no. 2: 75-78.

Few economists would deny that the "multiplier" analysis of the effects of governmental deficit spending has thrown some light upon this important problem. Nevertheless, there would seem to be some ground for the fear that this extremely simplified mechanism is in danger of hardening into a dogma, hindering progress and obscuring important subsidiary relations and processes. It is highly desirable, therefore, that model sequences, which operate under more general assumptions, be investigated, possibly including the conventional analysis as a special case. [...]

In order to remedy the situation in some measure, Professor Hansen has developed a new model sequence which ingeniously combines the multiplier analysis with that of the *acceleration principle of relation*. This is done by making additions to the national income consist of three components: (1) governmental deficit spending, (2) private consumption expenditure induced by previous public expenditure, and (3) in private investment, assumed according to the familiar acceleration principle to be proportional to the time increase of consumption. The introduction of the last component accounts for the novelty of the conclusions reached and also the increased complexity of the analysis.

A numerical example may be cited to illuminate the assumptions made. We assume governmental deficit spending of one dollar per unit period, beginning at some initial time and continuing thereafter. The marginal propensity to

consume  $\alpha$ , is taken to be one-half. This is taken to mean that the consumption of any period is equal to  $\alpha$  of the national income of the previous period. Our last assumption is that induced private investment is proportional to the increase in consumption between the previous and the current period. This factor of proportionality or *relation*,  $\beta$ , is provisionally taken to be equal to unity; i.e., a time increase in consumption of one dollar will result in one dollar's worth of induced private investment.

In the initial period when the government spends a dollar for the first time, there will be no consumption induced from previous periods, and hence the addition to the national income will equal the one dollar spent. This will yield fifty cents of consumption expenditure in the second period, an increase of fifty cents over the consumption of

TABLE I.—THE DEVELOPMENT OF NATIONAL INCOME AS A RESULT OF A CONTINUOUS LEVEL OF GOVERNMENTAL EXPENDITURE WHEN THE MARGINAL PROPENSITY TO CONSUME EQUALS ONE-HALF AND THE RELATION EQUALS UNITY

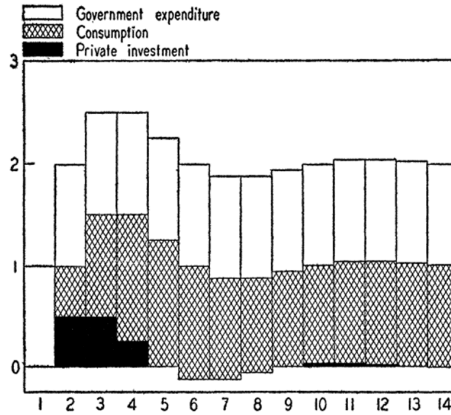
(Unit: one dollar)

Period	Current governmental expenditure	Current consumption induced by previous expenditure	Current private investment proportional to time increase in consumption	Total national income
1 . . . . .	1.00	0.00	0.00	1.00
2 . . . . .	1.00	0.50	0.50	2.00
3 . . . . .	1.00	1.00	0.50	2.50
4 . . . . .	1.00	1.25	0.25	2.50
5 . . . . .	1.00	1.25	0.00	2.25
6 . . . . .	1.00	1.125	-0.125 *	2.00
7 . . . . .	1.00	1.00	-0.125	1.875
8 . . . . .	1.00	0.9375	-0.0625	1.875
9 . . . . .	1.00	0.9375	0.00	1.9375
10 . . . . .	1.00	0.96875	0.03125	2.00
11 . . . . .	1.00	1.00	0.03125	2.03125
12 . . . . .	1.00	1.015625	0.015625	2.03125
13 . . . . .	1.00	1.015625	0.00	2.015625
14 . . . . .	1.00	1.0078125	-0.0078125	2.00
.....	.....	.....	.....	.....

\* Negative induced private investment is interpreted to mean that for the system as a whole there is less investment in this period than there otherwise would have been. Since this is a marginal analysis, superimposed implicitly upon a going state of affairs, this concept causes no difficulty.



CHART I.—GRAPHIC REPRESENTATION OF DATA IN  
TABLE I  
(Unit: one dollar)



the first period, and so according to the relation we will have fifty cents worth of induced private investment. Finally, we must add the new dollar of expenditure by the government. The national income of the second period must therefore total two dollars. Similarly, in the third period the national income would be the sum of one dollar of consumption, fifty cents induced private investment, and one dollar current governmental expenditure. It is clear that given the values of the marginal propensity to consume, and the relation,  $\beta$ , all succeeding national income levels can be easily computed in succession. This is done in detail in Table 1 and illustrated in Chart 1. It will be noted that the introduction of the acceleration principle causes our series to reach a peak at the 3rd year, a trough at the 7th, a peak at the 11th, etc. Such oscillatory behavior could not occur in the conventional model sequences, as will soon become evident.

For other chosen values of  $\alpha$  and  $\beta$  similar model sequences can be developed. In Table 2 national income totals

are given for various selected values of these coefficients. In the first column, for example, the marginal propensity to consume is assumed to be ~~one~~ *one*, and the *relation* to be equal to zero. [...] For this case no oscillations are possible. In the second column the oscillations in the national income are undamped and regular. In column three things are still worse; the oscillations are explosive, becoming larger and larger but always fluctuating around an "average value." In the fourth column the behavior is no longer oscillatory but is explosive upward approaching a compound interest rate of growth.

By this time the investigator is inclined to feel somewhat disorganized. A variety of qualitatively different results emerge in a seemingly capricious manner from minor changes in hypotheses. Worse than this, how can we be sure that for still different selected values of our coefficients new and stronger types of behavior will not emerge? Is it not even possible that if Table 2 were extended to cover more periods, new types of behavior might result for these selected coefficients?

TABLE 2.—MODEL SEQUENCES OF NATIONAL INCOME FOR SELECTED VALUES OF MARGINAL PROPENSITY TO CONSUME AND RELATION

(Unit: one dollar)

Period	$\alpha = .5$ $\beta = 0$	$\alpha = .5$ $\beta = 2$	$\alpha = .6$ $\beta = 2$	$\alpha = .8$ $\beta = 4$
1	1.00	1.00	1.00	1.00
2	1.50	2.50	2.80	5.00
3	1.75	3.75	4.84	17.80
4	1.875	4.125	6.352	56.20
5	1.9375	3.4375	6.6256	169.84
6	1.9688 *	2.0313	5.3037	500.52
7	1.9844	.9141	2.5959	1,459.592
8	1.9922	-.1172	-.6918	4,227.704
9	1.9961	.2148	-3.3603	12,241.1216
..	.....	.....	.....	.....

\* Table is correct to four decimal places.

## The Model

The national income at time  $t$ , can be written as the sum of three components: (1) governmental expenditure  $g_t$ , (2) consumption expenditure  $C_t$ , and (3) induced private investment,  $I_t$ .

$$Y_t = g_t + C_t + I_t.$$

But according to the Hansen assumptions

$$C_t = \alpha Y_{t-1}$$

$$I_t = \beta[C_t - C_{t-1}] = \alpha\beta Y_{t-1} - \alpha\beta Y_{t-2}$$

and

$$g_t = 1.$$

Therefore, our national income can be rewritten

$$Y_t = 1 + \alpha[1 + \beta]Y_{t-1} - \alpha\beta Y_{t-2}.$$

[...] The limitations inherent in so simplified a picture as that presented here should not be overlooked. In particular, it assumes that the marginal propensity to consume and the *relation* are constants; actually these will change with the level of income, so that this representation is strictly *original* analysis to be applied to the study of small oscillations. Nevertheless, it is more general than the usual analysis. Contrary to the impression commonly held, mathematical methods properly employed, far from making economic theory more abstract, actually serve as a powerful liberating device enabling the entertainment and analysis of ever more realistic and complicated hypotheses.



1958: T. A. GOUDGE, "CAUSAL EXPLANATIONS IN NATURAL HISTORY" *The British Journal for the Philosophy of Science* Vol. 9 No. 35: 196-198.

Let us now look at an actual specimen of causal explanation in natural history. It is advanced to account for a particular occurrence, namely, the occupation of dry land by the first vertebrates, the primitive amphibians which inhabited fresh-water pools and streams in Carboniferous and Devonian times, and which possessed rudimentary limbs. I will display the specimen at some length before commenting on it.

Why should the amphibians have developed these limbs and become potential land-dwellers? Not to breathe air, for that could be done by merely coming to the surface of the pool. Not because they were driven out in search of food, for they were fish-eating types for which there was little food to be had on land. Not to escape enemies, for they were among the largest animals of the streams and pools of that day.

The Devonian, the period in which the amphibians originated was a time of seasonal droughts. At times the streams would cease to flow...If the water dried up altogether and did not soon return, ...the amphibian, with his newly-developed land limbs, could crawl out of the shrunken pool, walk up or down the stream bed or overland and reach another pool where he might take up his aquatic existence again. Land limbs were developed to reach the water, not to leave it.

Once this development of limbs had taken place, however, it is not hard to imagine how true land life eventually resulted. Instead of immediately taking to the water again, the amphibian might learn to linger about the drying pools and devour stranded fish. Insects were

already present and would afford the beginnings of a diet for a land form. Later, plants were taken up as a source of food supply...Finally, through these various developments, a land fauna would have been established. (A. S. Romer, *Man and the Vertebrates*, 3rd edn. Chicago, 1941, pp. 478).

This piece of reasoning has a number of distinctive features. Although it is part of a scientific discussion, it is not concerned to discover or to confirm a general law. It does not establish any new empirical fact which is simply to be added to the store of human knowledge. It does not make an explicit, positive prediction about what will be found by future investigations, though there is a sense in which it makes certain negative predictions, [...]. What the reasoning does is to propose a theoretical pattern composed of accepted statements of facts ("The Devonian...was a time of seasonal droughts"), statements inferred from these statements of facts ("At times the streams would cease to flow..."), statements which are plausible conjectures about various occurrences ("The amphibian...could crawl out of the shrunken pool, walk up or down the stream bed...learn to linger about the drying pools and devour stranded fish..."), etc. This pattern presents an intelligible sequence of events such that the phenomenon to be accounted for 'falls into place' as the terminal phase of the sequence. The phenomenon ceases to be isolated, and is connected in an orderly way with states of affairs which lead up to it.

Looking more closely at the reasoning, we can say that what it does is to formulate by means of the theoretical pattern a possible sufficient condition of the phenomenon to be explained. This condition is both complex and formulated in broad terms against a background of assumptions, theories, and information which serves to limit it. The ex-

planatory pattern has specifiable 'boundaries' which allow it to be presented as a relatively isolated unit. Thus, although the explanation of the origin of land fauna presupposes the origin of vertebrates, we do not have to explain the latter phenomenon in order to explain the former. The pattern specifying the possible sufficient condition involves a number of components which the reasoning undertakes to enumerate. These components are either statements of necessary conditions [...] or statements of contingent contributory conditions [...]. It is fairly evident that no exhaustive list of the component conditions is given. It is also evident that where conjectured events are introduced into the pattern, their plausibility depends on the relevance they have to the overall sufficient condition. Without the latter there would be no basis for deciding what events can be plausibly included in the pattern and what events must be excluded. This is the main, though not the only, controlling factor in the situation.

It is tempting to think of such a causal explanation as conforming to the model of a deductive system. One is inclined to feel that the statements making up the explanation must be capable, at least in principle, of being organised deductively or perhaps even axiomatically. But this seems to me to be the wrong model to have in mind. As the above example shows, an explanatory pattern in natural history is closer to the model of a *coherent narrative* than to that of a deductive system. The narrative 'pictures' a certain temporal sequence of events. But the sequence is not such as to allow each event to be deductively inferred from its predecessors.

## VIII.

In 1910 the chemist and doctor Paul Ehrlich wrote “We must learn how to aim and, how to aim through chemical variation!” („Wir müssen also zielen lernen, und zielen lernen durch chemische Variation!“ transl. SB). The concept of the magic bullet still underpins the work of the chemists in the R&D departments of pharmaceutical companies such as Hoffmann Roche AG or Novartis AG in Basel, Switzerland. What Ehrlich called the magic bullet, is now referred to as “biochemical targeting”. Targeting means that a disease can be reduced to a biochemical cause within the human body (the target). After having singled out the biochemical structure of the target, the chemists start a process named “screening”: looking out for a variety of chemical compounds potentially capable of inhibiting or activating the target effectively. A “screening hit” forms a perfect chemical bond with the target similarly to a key that fits a lock but not all such hits will produce a successful drug since the compound might still be too toxic for human use; consequently the chemist needs to develop it further, or search further afield, until a successful candidate is identified.

Simple in theory, this endeavor turns out to be among the most complex, costly, interdisciplinary and tedious ones within the field of industrial research. To find such a compound that eventually enters the market as a drug usually takes up to 12 years because the chemical space that researchers in drug discovery have to navigate is vast. The size of the chemical space of compounds that are potentially pharmacologically active is estimated to be  $10^{10}$ . So, even if you do know what kind of characteristics your compound is required to have, there is no straightforward way to tell what it actually looks like from a chemical perspective because one cannot easily map the chemical structure onto its

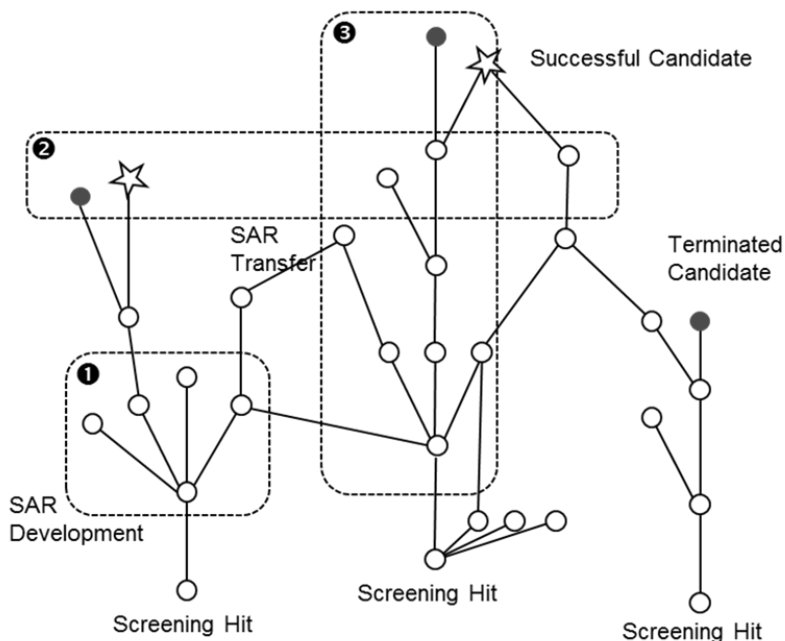
bonding behavior. Some of the most successful antibiotics for example have the most odd chemical structures.

Like any creative practice, drug discovery at its core is based on a series of making decisions successfully: What compound are we going to make first? Which one is next and why? Etc. Decision making is only effective though, when a) the amount of choices to begin with can be reduced reasonably to allow for a start and, b) when strategies exist that enable the researcher to move forward by correcting wrong decisions.

How do chemists in drug discovery actually make decisions given the lack of any reasonable theory within the vastness of the chemical space? Besides some rules of thumb, chemists tell themselves narratives to facilitate their own decision making about what they should do next, and in plotting their paths of exploration and navigation of the chemical space. In doing so, they treat possible compounds as agents with ascribed properties such as beauty, economic value, patentability, etc.; and their associated narratives are aligned to how they think about their own agency - as navigators in searching the space. Significantly, they also produce retrospective narratives in the form of maps or visualizations of these choices and the paths they have taken that look like tree diagrams, such as the one shown here. These retrospectively produced maps constrain the vast, initially featureless chemical space in ways which allow them in this second narrative to single out their starting points, and tell a narrative of the steps (successful and dead ends) from initial screening hits to the successful candidate compound. More importantly, these narratives help them to correct and to improve their decisions over time. Narratives in drug discovery therefore effectively serve chemists as navigation tools through chemical space.

Sabine Baier





*Nodes:* represent compounds

*Edges:* represent that changes have been made to a compound in order to improve its SAR

*Direction of time:* running from the bottom of the diagram to the top

*Screening Hit:* initially developed compounds at the beginning of a drug search that are perfected over time to increase their SAR, screening hits differ in various degrees in their core structure

*Terminated candidate:* screening hits that are no longer explored due to inferior SAR

*SAR:* Structure Activity Relationship between the chemical structure of the screening hit and its biological activity, i.e. its ability to tackle the target: the goal is to perfect the SAR (efficiency, potency, low toxicity, etc.) over time

*SAR Transfer:* transfer of desired properties between two similar screening hits, i.e. adapting the chemical structure of one screening hit to another to get the best out of both screening hits

1. SAR Development exploring the SAR of different variations of a single initial screening hit

2: comparative phase where SARs of different screening hits are reviewed; may lead to a SAR Transfer

3: full case study outlining the success story of a single screening hit all the way to a drug candidate entering the clinical testing phase.



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