Hypernormal science and its significance.

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Abstract

'Hypernormal science' has minimal potential for contestation on matters of principle and practice so that *information exchange* can be unproblematic. Sciences comprise hypernormal domains and more contestable 'normal' domains where knowledge diffusion, like acquiring linguistic fluency, depends on face-to-face interaction. Hypernormal domains belonging to molecular biology are contrasted with normal domains in gravitational wave detection physics. Sciences as a whole should not be confused with their typical domains. The analysis has immediate implications for proposed transitions out of the Covid-19 lockdown, proposed solutions to the replication crisis, and, perhaps, our understanding of the early development of social studies of science.

Key words

Hypernormal science; molecular biology; normal science; gravitational wave detection; fractal model of society; division of labor, face-to-face conferences; replication crisis

Background and Introduction

Since the 1960s and '70s, sciences have come to be understood as cultural institutions depending on trust and agreement within a background of shared assumptions.[1] Societies as a whole are made up of cascades of embedded cultural domains which can be thought of as the elements of a fractal. Each domain,

[1] Collins and Evans (2002) refer to this as 'Wave 2' of science studies, Wave 1 being the 'logic of science' view widely held up to that time, while Wave 3 justifies the pre-eminence of science consequent on its cultural properties as an institution. Thomas Kuhn's (1962) *Structure of Scientific Revolutions* could be said to be the trigger for analysis of the sciences as cultures, though this had been anticipated by Ludwik Fleck and the major shift in understanding was given philosophical depth by Wittgenstein's analysis of forms of life. Under the 'enculturational model', *information exchange* could not be the means of transferring the tacit knowledge needed for success when skilful experimentation was at stake; and there was always scope for interpretation of the validity and meaning of data. The diffusion of tacit knowledge came to be understood as much more a matter of the interpersonal interaction associated with apprenticeship and embedding in a culture and this had implications for the way replication of experiments could be differently embedded in cultural communities when they were used as a test of scientific claims (see Wittgenstein 1953; Kuhn 1962; Fleck 2008). Collins' (1974) study of the building of TEA lasers provides one of the earliest empirical case studies.

including science, is similar in so far as membership is acquired via socialization. Figure 1 is an *aide memoire* with the sciences emphasized.[2]

Scientific disciplines are made up of ever more specialized domains as we go downward in the fractal. Here we look at domains embedded within gravitational wave physics (GWP) and molecular biology (MBIOL). Domains within science differ according to the extent to which their assumptions have been developed and culturally 'sedimented'. Some domains will still be open to contestation while in what we call 'hypernormal' domains the process of cultural sedimentation will have reached the point where contestation about assumptions and practices is almost impossible. We look at certain hypernormal domains of molecular biology, contrasting them with the generally contestable domains of gravitational wave detection physics.[3] We speculate that the larger number of hypernormal

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[2] For a fuller discussion of the fractal model with wider political implications see Collins et al (forthcoming) and for an exploration in terms of methodology see Collins (2019, chap. 1). For an indication of the way the fractal continues downward into sub-specialisations in the case of the sciences see Collins (2011, fig. 5).

[3] The pioneering study of the difference between high energy physics and bioscience is Knorr-Cetina's *Epistemic Cultures* (Knorr-Cetina 1999). Collins conducted a study of the field of detection of gravitational waves from 1972 until the first detection, announced to the world in 2016 (LIGO Scientific Collaboration and Virgo Collaboration et al. 2016); he has written many papers and four books

domains in MBIOL compared to physics in general affects scientists' and perhaps other's understanding of the nature of the sciences at the disciplinary level.

[FIGURE 1 HERE]

In much of science, communication among scientists is about establishing the common culture, generating trust, and legitimating and transmitting tacit knowledge, including the acceptable background of shared assumptions. This is most striking in what Kuhn called 'revolutionary science', but normal science also continues to evolve culturally, and its scientific culture is often subject to marked contestation – Kuhn's description of normal science as 'puzzle-solving' is too simple. In *hyper*normal science, however, so much in the way of beliefs and skills has already been established and shared – so much is routine – that long immersion in such work might make the culturally evolving face of normal science hard to recognize and understand: the nature of communication across the entire range of the sciences might be thought of as a matter of information exchange rather than culture building. Indeed, it was an information exchange

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emerging from the project (see e.g. Collins 2004; 2017). For a significant part of this period, he was immersed in the field, eventually deeply enough to pass as a gravitational wave physicist in an Imitation Game (Giles 2006; Collins 2017, chap. 14). For a paper which indicates the difficult of agreeing so much as a defined measurement in this field see Collins (2001).

model that informed early research on scientific networks based on co-citations and the like and encouraged, and continues to encourage, the idea that science could be executed by computer programs.[4] Toward the end of the paper we will look at certain important consequences of these differences in understanding.

In putting forward the idea of hypernormal science we are saying that, in spite of the paradigm shift within the social studies of science from 'algorithmical' to 'enculturational' model, there are domains of science in which the old algorithmical model still works reasonably well. It still works where, as with a

[4] Prior to the 1970s most social analysts thought of scientific knowledge as information-like, reflecting the position of philosophy of science: theories gave rise to experiments and data-like experimental results were circulated among the scientific community in peer-reviewed papers for feeding back into theories. It was taken that social analysis via the study of co-authorship networks or citation networks of various kinds represented the frontiers of social understanding of science since these formal representations comprised scientific knowledge (see, for example, Garvey and Griffith 1971; Crane 1972; Sullivan, White, and Barboni 1977); these studies were based on the information exchange or 'algorithmical' model). But when science began to be understood in terms of cultural change, the algorithmical model seemed too simple; there was much more going on when ideas diffused and took root in a community than the circulation of published results, the transmission or non-transmission of tacit knowledge being a visible indicator if one knew how to look for it

traditional society, the transmitters and receivers not only share a sedimented common culture but also the set of practical skills that corresponds with it. In such settings the transfer of tacit knowledge and potential doubts about the validity of experimental results no longer create the kind of difficulties, or potential difficulties, where there are deep, though sometimes unarticulated, disagreements. Of course, scientific skills with all their tacit components, must still be acquired by socialization into a community, but, in hypernormal domains, the apprenticeship will be complete for most scientists by PhD level if not before, whereas in a domain like gravitational wave detection physics, what counts as sound procedures is still being established as the work is pushed forward at the research front and establishment and acquisition of tacit knowledge continues throughout a career.[5]

From traditional-vs-modern to a fractal model of society

According to Emile Durkheim, traditional societies are characterized by 'mechanical solidarity' resulting from the common beliefs and actions of their members. This gives rise to what we will call a 'simple division of labor' in which tasks are relatively predictable and most positions in a cooperating team can be readily allotted and swapped because everyone shares a common culture and similar beliefs and skills. In these circumstances there is very little in the way of

[5] Gravitational wave detection is rapidly sedimenting now that more are more detections are being made but the rules of what counts as an acceptable detection claim are still not clear at the margins.

specialization although, as with all societies, there will be some distinctions and hierarchies. In contrast, social cohesion in modern, industrial societies has to arise from the mutual appreciation of different, specialist roles. In these societies the division of labor is 'complex': individuals and institutions are no longer interchangeable. In complex division of labor, a shared language must be developed to provide the 'interactional expertise' needed to coordinate the varying skills which are accomplished by narrow specialists, and to establish the direction of still unfolding research frontiers.

The development of modern societies is not, however, a one-way transition from homogeneity to heterogeneity. The creation of the specialist sub-domains needed for a complex division of labor moves the specialists in the opposite direction *within* these sub-domains: division of labor within such sub-domains goes from complex to simple as the members of each narrow specialist group come to share a distinctive set of skills. The smaller and lower in the fractal is the domain, the more likely is it that, once socialized into it, all its members will share a greater proportion of understandings and skills, looking more like a traditional society.

In societies such as the UK or US, becoming a specialist expert involves becoming socialized into one of the ever smaller sub-societies illustrated in Figure 1. Below these are ever smaller domains and there can be still further cascades of sub-specialties within them, notably in the sciences.

As domains within a science move from the pioneering stage to becoming part of the taken-for-granted world they must pass through levels of increasing cultural consensus: the boundaries of legitimate criticism narrow as more and more aspects of the science become 'sedimented'.[6] Some of these stages are represented by 'a to e' on the list below.

[6] The Duhem-Quine thesis states that scientific claims rest on a network of sub-hypotheses. Claims are established by reducing the number of sub-hypotheses that may be legitimately doubted. The difference is also visible in the extent to which the experimenter's regress (Collins 1992) is likely to be a problem.

a) agreement about the conceptual structure of the science

b) agreement about the principles of experiment that bear upon the concepts pertaining to the science

c) agreement about particular types of experiment and associated apparatus that are most efficient for testing theories or making observations

d) development and establishment of a ubiquitous domain-specific 'interactional expertise', that is, fluency in a 'practice language' that enables complex division of labor among experimenters with different specialist abilities[7]

e) development of a ubiquitous domain-specific 'contributory expertise', that is, common theoretical and practical skills, allowing simple division of labor, where anyone can do anyone else's task with minimal preparation.[8]

Normal science never gets further than level 'd' but hypernormal science is found at level 'e'. At level 'e' not only conceptual agreements and interactional

[7] For the explication of relational tacit knowledge and the difference between ubiquitous tacit knowledge and specialist tacit knowledge see Collins and Evans (2007).

[8] For (a) consider the understanding of gravitational waves by about the middle of the Twentieth Century; for (b) consider gravitational wave detection by the late 1960s when the right kind of detector was still the subject of intense disagreement; for (c) consider gravitational wave detection at the point when the interferometric detectors had become dominant (though the bar detectors were still fighting a losing battle); for (d) consider the division of labour within the

expertise are ubiquitous but contributory expertise is universally shared: day-to-day practical actions can be done by anyone in the domain with only an introduction to the task, and there is no-one who is so specialized that it is difficult for another member of that domain to communicate with them about their art.[9]

Once we reach the degree of sedimentation of hypernormal science it becomes possible for the science to pass through still more stages of routinization.

f) Scientists who have completed apprenticeships in the course of their education could be employed by commercial firms who can then be subcontracted to undertake routine aspects of the scientific process since the culture of the science is distributed across the firms, via their employees, as much as the research laboratories.

g) Aspects of the process might become understood sufficiently (as a result of relational tacit knowledge becoming explicated), to allow them to be separated out and worked on by employees or sub-contractors who have some of the widely distributed skills found in the society as a whole and require only some information-like 'instruction' to know how to do their component of the

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consensual GWP interferometer-developing community. What we see in the early days of TEA-laser building (Collins 1974; 1992) is a science where the contributory expertise was not yet ubiquitous but everything else was consensual; it was not yet hypernormal (though it probably is hypernormal today).

[9] But as Lewis, Bartlett and Atkinson (2016) point out, even in molecular biology there remains a tension between informatics specialists and biologists.

overall job – in the manner of the instruction needed to work on a production-line.

h) If 'g' is accomplished, it might be possible to mechanize some of the necessary tasks – giving the impression to some that science as a whole is ready to be taken over by artificially intelligent computers and the like.

As we go down the fractal and the elements get smaller, the more agreement there is likely to be, the more likely is it that division of labor will be simple rather than complex, and the more will information exchange suffice for actors to coordinate their actions. But we have to think in terms of *likelihood* because the actual degree of consensus depends on other things as well as size and position in the fractal and longevity, such as whether scientists think they have more to gain in terms of making scientific progress, or satisfaction of one sort or another, from agreement or disagreement. At least until quite recently, mainstream gravitational wave detection physics was characterized by disagreement about methods and direction of progress, whereas molecular biology seems far less so.

There are material forces affecting whether the science reaches levels 'f-h'. Molecular biology interacts with the hugely wealthy medical industry so the push to exploit cultural consensus in the form of industrialization is strong. Bartlett, working from a labor process perspective, described the levels 'f' and 'g' that he encountered in the human genome project as 'extraordinarily normal science'. Bartlett associated extraordinarily normal science with the growth of the *big science* of biology, which is about the accretion of new facts with not much possibility or expectation of new insights or novelties. *Hypernormality*, in contrast, describes an epistemological state in which the degree of sedimentation of the culture is maximal, and it can be found in both big science and small science. As such, hypernormality provides a condition for the emergence of industrialized extraordinarily normal science though it does not provide the demand, which has more to do with applicability and market position.

Going back to Durkheim, strangely, a thoroughly established domain of science, characterized by strong agreements, is like a traditional society where the taken-for-granted reality is universal, whereas less solidly established domains are like more modern societies with their disagreements and specializations! On this account, any industrial process such as a production line, is like a traditional society in so far as it involves simple division of labor; the cultural conditions that allow for ready replacement of one laborer with another – deep sedimentation of a common culture – is already in place. At level 'g' the common culture is the ubiquitous culture of the society rather than the specialist culture of a science.

The last step to hypernormality

Consider the distinction between interactional expertise and contributory expertise. GWP and MBIOL share the overall ubiquitous expertise of science. Going downward in the fractal cascade, GWP has a number of specialist sub-groups which share their own still more local expertises, such as gravitational wave-form calculation, interferometer mirror suspension design and statistical analysis of interferometer output. As in modern societies, different specialists need to coordinate their different skills and abilities. They accomplish this through fluency in a shared practice-language – a ubiquitous language at that level of the fractal – ‘interactional expertise’.[10] This is represented in Figure 2.

[FIGURE 2 HERE]

Each of the numbered specialists has a hammer and anvil with a number representing their particular practical specialty, but the division of labor is managed by the shared practice language depicted by the bundles of waves distributed throughout the domain; the shared interactional expertise enables the specialists to coordinate their work. The sociologist immersed in the community (the stick-figure without an anvil), can also acquire interactional expertise through immersion in the spoken discourse and can pass as an expert in the domain in an Imitation Game (a Turing Test with humans) even though the sociologist has none of the practical skills –that is, no ‘contributory expertise’.[11] The importance of this model for understanding types of science is that once a normal science domain like gravitational wave detection has become big science, interactional

[10] The idea of uniformity within a domain is set out in Collins and Evans (2017). The idea of a practice language and its relation to interactional expertise is the subject of Collins (2011). For a more extensive analysis of interactional expertise see Collins and Evans (2015)

[11] For an example of an Imitation Game involving sociologists and physicists see Giles (2006).

expertise is ubiquitous among the participants, but contributory expertise is not. In contrast, within a hypernormal domain of science both interactional and contributory expertises are ubiquitous. [12] Were we to re-draw Figure 2 to represent a hypernormal domain there would be no use for numbered labels because the scientists would be indistinguishable from one another in terms of their practical expertises, beyond the dexterity born of repetition of routine tasks. Like Adam Smith's pin-makers, the scientists might choose to concentrate on specific tasks to improve efficiency, but individuals would be capable of switching roles without difficulties caused by failures of understanding or competence: there would be no difficult 'trading zones' between specialists to negotiate, there would just be 'trade'.[13]

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[12] Some inkling of this difference can be found in Knorr-Cetina's pioneering comparison of high-energy physics and biology (Knorr-Cetina 1999). Knorr-Cetina finds that high energy physicists faced with an experimental failure systematically analyse each element of the experiment in ever more detail in an attempt to fully understand the cause of the failure and its place in the physical world. One consequence is an ever more complex division of labour as new areas of specialisation occur. In contrast, biologists faced with an experimental failure respond with a strategy of 'blind variation combined with a reliance on natural selection' (p. 91) in which they vary the procedure but not the underpinning concepts or theories. We might say that physicists take their 'standard model' as a topic whereas biologists take their 'central dogma' as a resource.

[13] For the distinction see Collins, Evans and Gorman (2007).

Shrager's experience

We now illustrate the idea of hypernormal science with Shrager's experience in domains of MBIOL. From the early 2000s Shrager kept a detailed diary, published in 2004, of how he learned the art and craft of microbiology.[14]

In his diary-based article Shrager introduces himself as follows:

> In 1996, I set out to become an environmental phytoplankton molecular biologist. I began with informal study, and in the summer of 2000 1 joined a laboratory full time. My only prior formal training, aside from a typical

[14] By coincidence, Shrager and Collins met in the course of Collins's month-long sojourn at Xerox PARC, Palo Alto, in late 1987 where Shrager also became aware of the existence of social studies of science – bolstered by subsequent interactions with social psychologist Mike Gorman. But Shrager, when he was writing his diary and the subsequently published article (Shrager 2004), did not know any details of Collins's work, nor sociology of scientific knowledge in general, nor the disputes animating social studies of science: this is important for understanding his role as native informant. Shrager's early work in computer modelling of science and in social psychology had given rise to an interest in the processes of socialisation that might be relevant to the development of scientific understanding but the deeper appreciation of the *sociological* approach to the analysis of science that informs his co-authorship of the current paper is more recent and, even now, he is, to a large extent, still acting as a native informant though a pro-active one in terms of the development of the argument and the paper.

> precollege K-12 education, was in computer science (BSE, 1980, MSE, 1981) and cognitive psychology (MS, 1982, PhD, 1985). I had only a few classes in chemistry, biology, and physics in high school and college. Beginning around 1997, while commuting by train to work at a startup company building drug discovery software, I studied organic chemistry, biochemistry, and molecular biology from textbooks. In 1999, I took the Cell, Molecular Biology, and Biochemistry Graduate Record Examinations, and in the fall of 1999 1 volunteered in the molecular microbiology laboratory of Dr. Arthur Grossman at the Carnegie Institution of Washington, Department of Plant Biology, located at Stanford University. I worked half-time in Grossman's laboratory for about a year, finally joining full time in the summer of 2000. At that time, I began to keep a "cognitive diary" … in which I recorded my insights about my activities and thinking in the laboratory. Between May and December of 2000 1 logged a total of 75 entries. [p120]

How could Shrager learn enough of his subject from books to be in a position to volunteer to work in a molecular biology laboratory? One cannot imagine someone who was largely ignorant of physics putting themselves in a position to contribute to work on a physics project that demanded an understanding of something like gravitational wave detection, by reading some textbooks while on a daily commute. The difference is, perhaps, partly related to the unusually large role of text in the way MBIOL is done (at least in comparison with GWP); this again is a strong indicator of the pressure to accelerate the conversion of the tacit into the explicit. As we move on a couple of pages in the published version of the diary we find:

> I've come to rely heavily, as do all molecular biologists upon three sources: My protocol book, my lab notebook, and a set of protocols called "Molecular Cloning" by Sambrook, Fritsch, and Maniatis. If you lose your protocols, you might be okay, because they are mostly collected from other people. If you lose Maniatis, you just buy another, or borrow it from the lab next door. The protocols in [Maniatis] are so complete that people usually just read them straight out of the book. It's sort of 'The Joy of Cooking' for molecular biology. But if you lose your lab notebook, you're hosed, mainly because you'll never figure out what is in the hundreds of obscurely-labelled tubes in the various freezers in the various boxes with the various obscure markings on them. (Shrager 2004, 122, lightly edited for typos and ellipses).

We see that the procedures for doing what gets done in certain domains of molecular biology are mostly already laid out in a set of standardized protocols and what Shrager was doing on his commute was getting to the point where he could make some sense of them.

That scientists working in certain domains of molecular biology feel it is necessary and desirable to formulate its procedures in handbook-style publications, is also illustrated is in a 30/03/2021 Zoom interview conducted by Collins, Evans and Hale, with Aled Clayton, a Professor of microbiology at Cardiff University. Clayton was one of the pioneers of extracellular vesicle research, nowadays a field with a couple of thousand participants. He described one of the foundational activities of the field:

> My vesicle field is particularly interesting in this regard [it is still kinda?] a nascent field and we think it was 2013, 2012, we set up a society: 'The International Society for Extra-cellular Vesicles'.[15] … And part of the job of the society as it sees itself is to bring that consensus to the community in terms of best practices – dos and don'ts. There's a big paper, it's called MISEV2018,[16] So it's kind of an updated document on best practices for isolating and analyzing vesicles.[17]

Another aspect of the 'prepared ground' feature of domains of MBIOL is illustrated in another extract from the Shrager diary:

> [T]he other day I ordered two "primers," which are custom synthetic oligonucleotides that match the ends of the gene I'm after. You order them on the web, and they show up a couple of days later in a FedEx package that contains two apparently empty vials, which, so I was told in the enclosed materials, contains my custom primers. To make them useful, you just add water and stir.

[15] For more information see https://www.isev.org/

[16] The publication is available as Théry et al (2018)

[17] There is nothing like this in GWP – too many things are being worked out and argued out for the first time and they are too new and open to improvement to be encapsulated in a handbook.

> Literally! So, I did, and tonight I'll try to use them to amplify my gene. (Shrager 2004, 121)

Here we see that the 'ready preparedness' extends to the preparation of samples for the use of scientists in remote laboratories whose actions are foreseen by commercial concerns.

This relationship with commercial concerns is echoed in the interview with Aled Clayton. When we asked about the effect on his work of the shutdown of face-to-face meetings Clayton (AC) placed a strong emphasis on interaction with commercial firms: AC talked about the way biomedical conferences are sponsored by industry (£120K in 2016 when he organized a meeting in Rotterdam). He explained that this was a matter of *scientific* as well as financial benefit to the scientists:

> AC What the industrial guys do is that they can provide tools and methods to help us solve problems. … as academics in a discovery lab ... there is only so much we can do. So buying things pre-developed from a company is how it usually happens. It could be a big instrument that costs £120,000 or it could be kits to measure something -- that is two or three hundred pounds. There's a huge diversity there that is available on the shop floor when you go round a conference. At coffee time you bump into the same reps. 'Oh have you spoken to such and such, they've got a really cool device for measuring your problem'. So, you get some little insights and access to expertise that is outside, perhaps, of the core

> academic expertise. So I think they're quite valuable. … Systems to make things a bit quicker, a bit easier, maybe save money and time and effort. So, you're always on the lookout for these kinds of things.

Learning to do gene sequencing

Shrager's biological research included experiments with phytoplankton, also called cyanobacteria, which are single cell organisms characterized by photosynthesis. Shrager investigated the gene-related process that regulates this photosynthesis in high-light conditions, and which prevents the over-production of reactive oxygen ions that can destroy the cell from the inside. A crucial part of this work is DNA sequencing. This technique has been under continuous development since the 1970s and involves complex chemistries. Early in its development, sequencing carried out manually might have taken weeks to do something that can now be done in hours or minutes by machines called "sequencers". As a result, the critical activity of sequencing presents itself, nowadays, as a taken-for-granted operation that all molecular biologists do as regular part of their everyday work.

Shrager took a year to acquire most of the skills and understandings he needed to complete an experiment – leading to a publication (with other members of his laboratory) which revealed one of the genes involved in protecting cyanobacteria against potentially damaging oxygen ions.

This publication was not an unalloyed 'piece of information' because the interpretation was queried on the grounds that the light changes involved as part of the experiment should have been gradual to represent real-life conditions at dawn and dusk whereas Shrager's lights were either on or off – a step function. That criticism has legitimacy precisely because we cannot know if it is valid without doing a series of time-consuming replications and arguing out the correct interpretation of the results. That element of the work was more normal than hypernormal but one can see that, in terms of what it is legitimate to question, this is at a fairly general level. No-one was questioning the details or legitimacy of the manipulations and measurements. The envelope of legitimate questioning was tighter than this. As Aled Clayton put it referring to a quantity-of-protein assay involving adding a couple of liquids to the sample and then comparing that color to a sample chart.

> **HC** So let's take that as a typical result. Presumably a result like that could wind up in a publication somewhere.
>
> **AC** No – because that stuff is so routine, it's like putting your shoes on, if you're going running. It's part and parcel of a workflow …

No-one questioned Shrager's experimental virtuosity when it came to reported measurements and observations even though he had only newly picked up the necessary skills; all this part of the experiment was accepted as *merely information*. What wasn't questioned was Shrager's ability to handle and report the growth of cyanobacteria, the knocking-out of genes, and the detection of the gene responsible for the survival of the plankton *within the context of his chosen light-changing regime*. Shrager had become a partially self-taught, potentially published, molecular biologist, who could provide reliable information in respect of all those features of the experiment, in a year, the results being questioned only at the general level of the choice of light-modification protocol, a factor unique to the problem. He had not entirely escaped the experimenter's regress but doubts about experimental capacity were limited to the general levels of interpretation. This domain of molecular biology seems to be, then, a very different science to gravitational wave detection.

Another apprenticeship

Starting in 2009, Conley also undertook a partial training in molecular biology as an 'embedded humanist', which backs up the claim that manipulative abilities such as PCR can be learned quickly. Within a matter of days of being invited to learn the technique, Conley was doing her own PCR and produced results of a good enough quality to be used by the biologists as an example in lectures. This experience was gained in Canada and then used two-months later in a second laboratory placement in the UK where it was immediately recognized by a post-doc that Conley had enough skill to assist with his experiments. Conley reports that in the UK laboratory she was often mistaken for a geneticist herself and that she felt entirely comfortable in the practical aspects of laboratory work.[18] This illustrates, once more, that when a new entrant to a discipline engages with a hypernormal domain it can take only a short time to reach a useful level of participation in the science. To repeat, this is because fundamental disagreements

[18] These experiences are described at much greater length in Conley and Fisher (2019).

will not have to be understood, only readily described techniques, and in this case much of the technique has already been so formalized that it can be carried out by machines.[19]

[19] New entrants to GWP undergo a long apprenticeship to reach the frontiers of the science and unsolved problems and arguments about the optimum way forward continued even after the interferometer technology had triumphed; even now it is not always clear when a new detection has been made. We are still not sure whether a powerful signal can be said to have been detected by a single interferometer or whether two are still needed if there is no corresponding electromagnetic signature. Thus, by the time a research degree or some modicum of laboratory training has been undergone GWP scientists are just entering domains of deep dissensus, probably for the first time; those entering MBIOL may well not be thus exposed. It is, then, not that Jeff Shrager and Shannon Conley did not have to acquire a mass of tacit knowledge, just like a pioneering TEA-laser physicist, or an early measurer of the Q of sapphire, but that this happened quickly in an environment of agreement about what had to be known and how it was to be known. The need for a journey is common to all those who want to become scientific specialists but, like a short ride on a subway, the journey delivers Shrager, Conley and their equivalents, quickly to a known destination where puzzles are to be solved, whereas for a GW physicist, the journey is, or was, like embarking on a sailing voyage with an argumentative crew in search of an unknown North-West passage.

Different kinds of outsourcing

There are different ways of contributing to scientific research.[20] Likewise, 'outsourcing' to commercial firms can mean different things. For instance, there are two kinds of outsourcing found in MBIOL and discussed here and other kinds found in gravitational wave physics. Both kinds of MBIOL-related outsourcing discussed here are parasitical on hypernormality, but in different ways. In the cases described by Shrager and Aled Clayton and discussed in the last section, the industrial firms are doing real science on behalf of the research laboratories: this depends of hypernormality because it rests on the research-level understanding of the science of firms' employees, acquired during the relatively short and bounded apprenticeship undergone in the course of their scientific education to doctoral level or below; the members of the firm have to be able to invent ways of doing the scientists' work which are beneficial to the scientists. This must involve a level of contributory expertise so they must have an understanding up to around Shrager's level. Compare this with the contributory expertise of writing computer software, found in gravitational wave physics (and research physics in general); to do it properly requires a research-level understanding of the science and in GW physics it is not outsourced to software houses because that research-level understanding is a still a moving frontier located in the research institutions not commercial enterprises. As it happens, however, writing code is close to a

[20] For an extended discussion of the philosophy of the different things it means to make a contribution to a scientific project see Collins, Evans and Weinel (2016).

ubiquitous expertise within physicists' core-sets so there is no point in outsourcing even if it was possible.

Another kind of outsourcing found in MBIOL and described by Bartlett is also consequent on hypernormality but on the possibility of the routinization of tasks described at level 'f' and onward for which hypernormality provides a necessary if not sufficient condition. Bartlett reports that the first attempts made by the British team to sequence the human genome was intended to follow the model that follows from level 'e'. An interview extract explains: "And the way the original proposal was set out was that basically we would have seventeen teams of ten people doing the same thing in parallel. [...] And that team of ten was based on the lab structure that [the founding director] had built up at the [small science laboratory] to do the first worm sequence – cosmid sequencing. So, we were just going to multiply it umpteen times." ('Susan'). As it happens this did not work very well and the model that was eventually successful was more dependent on level 'g'.[21] In respect of recruitment of scientific workers to work on the Human Genome Project in the context of level 'g' Bartlett reports that John Sulston wrote, "We would recruit unskilled people [...] This group would have no need of academic qualifications. We judged them by school achievements, interview, and something by which I set great store: the pipetting test" (Sulston and Ferry 2002, 75).

[21] Bartlett (2008, 208).

This second model was a possibility because MBIOL has become so routinised that it can be broken down into small self-contained tasks. Indeed, the tools are now available to 'hobbyists', with kits and reagents available by mail order.[22] This has gone so far that it has raised the specter of biotech weapons being put together in terrorists' kitchens.[23]

These kinds of routinization and production line work are associated with a shift to 'big science' but hypernormalisation is the cultural condition even if economic pressures (such as potential impact on medical understanding) are pushing the science toward production-line type routinization so as to ease the shift to large scale work. Automation follows, but it too is not simply the embedding of tacit knowledge into machines, the conversion of tacit knowledge into explicit knowledge is required first.[24] In MBIOL then, there is no problem to be resolved by the development of a shared practice language and interactional expertise –

[22] See Delfanti (2013) for a discussion of the cultures of DIYbio/biohacking; see Nature Biotechnology (2018) for an editorial that plays down sensationalist talk of DIYbio security risks.

[23] Researchers studying DIYbio often report that agencies such as the FBI pay attention to biohacking communities (see, for example, Landrain et al. 2013). (2013). See Schmidt (2008) for an early discussion of DIYbio risks.

[24] For an exploration of this process see Collins (2010). For the difficulty of automation on already existing assembly lines see Jones (1989)

there are no epistemologically troubling *trading zones* there is just *trade*: the epistemologically untroubled exchange of items of mutually recognizable value![25] If one looks at gravitational wave physics, it too evolved into a large-scale science, but the process was different. GWP became a big science in the sense of spending the kind of money that had a big opportunity cost for other scientists; this meant visible accountability and the abandoning of pet projects so as to facilitate the completion of tasks by a deadline, but it was never a hypernormal science (though it might be on the way to becoming one with the general acceptance of a growing number of detections).

The outsourcing found in the Laser Interferometer Gravitational-Wave Detector project (LIGO), was also of a different kind to that found in MBIOL. There are industrial concerns that supply services or goods for gravitational wave detectors, but they are there not because of the needs of the science itself but demands from other enterprises that have already been funded on large enough scale to be commercially attractive (notably 'Star Wars' and optical telescopy, for which mirrors are vital, and various kinds of engineering – for instance, the firm which did the beam tube welding was *Chicago Bridge and Iron*. Outsourcing from these firms amounted to the commissioning of bespoke, technical services that were better accomplished by specialist firms who were not traditional contributors to gravitational wave science. When GWP became 'big science' it was not to repeat

[25] See Collins, Evans and Gorman (2007) for the distinction between a 'trading zone' and 'trade'.

a similar process hundreds of times but to do something at the very edges of technical accomplishment. But one would never be tempted to say that the outsourcing firms were 'doing physics' (even though with mirror polishing and coating, physics-like measurements are involved). Instead, it is a matter of building the most exquisite apparatus with processes outsourced to take advantage of industrial skills on tasks well enough defined so as not to require much in the way of understanding of the science in order to produce a project that fits the specification. For instance, the beam tubes for the interferometers had to be made to standards of cleanliness beyond normal steel fabrication practices but the welders, who worked with white gloves, could rapidly absorb the lesson;[26] interferometer mirrors must be grown as huge flawless crystals which is very much an industrial technique; and mirrors must then by polished to exact profiles which is a specialist hands-on skill.

There is, of course, the repetitive and computerized matter of the calculation of a bank of waveforms represented hundreds of thousands of inspiraling binary star scenarios (around 250,000 in the 'template bank' at the time of the first discovery). The first calculation of any waveform involved inspiraling black holes took decades of difficult normal-science style effort, but once the problem was cracked, it became a matter of routine, but still carried out by the scientists with the computing skills. These domains never provided a hypernormal flavor to the science as a whole because there was always the possibility that pursuing them

[26] See the description in Collins (2004, 530 ff).

represented a huge mistake in the context of the understanding of both astrophysics and interferometry and this remained the case at least until the confirmation of the first discovery.

Molecular Biology and hypernormal science

That there are hypernormal domains in molecular biology is strongly indicated by the enthusiasm for the successful application of automation and outsourcing, long documented in the history and sociology of science literature. Unlike other domains, according to this analysis, the initial optimism about automation has been realized to some extent because the conditions for routinization of the demand is there.[27] But, crucially, this does not mean that molecular biology is nothing but a hypernormal science. In another sense, all sciences are the same, they comprise a jig saw of sedimented hypernormal domains and still contestable culture-building domains. That is why it is important that the success of and visible domination of hypernormal domains in molecular biology does not lead to the mistaken view that there is nothing else to it. Various aspects of molecular

[27] See, for example, Keating, Limoges and Cambrosio (1999); Bartlett (2008); Arribas-Ayllon, Bartlett and Lewis (2019). The enthusiasm for automation seems to have transmitted itself beyond the science itself (see, for example, Wykstra 2016; Alkhateeb 2017). Wykstra, revealingly, considers that automation could resolve the replicability crisis, having no sense of the way the experimenter's regress and and cultural nature of science render replicability far more than a mechanical process.

biology still exhibit the stresses, strains, disagreements, and other aspects of culture-building akin to that found in gravitational wave physics.

Missing this other face of even hypernormal science is dangerous. The director of a research center explained:

> "I've been very worried about outsourcing for a while because I didn't… you know some groups and my competitors in the States are really… they really do not have a lab. I mean they might have a technician who can aliquot DNA and a freezer, but they don't, you know just everything is outsourced and I never wanted that, I wanted us to do some biology afterwards and do some of the sort of, the smaller scale experiments which often are the most interesting ones." (Arribas-Ayllon, Bartlett, and Lewis 2019, 116).

That the hypernormal as a form of science is a cause for concern is also evident from some accounts of the Human Genome Project, with Service (2001, 1182) describing fears that the HGP would be a "mindless factory project that no scientists in their right minds would join" that would destroy "the cottage industry culture of biology in the process". However, that there has been some excited anticipation that massive data sets of molecular biology will allow computers to move us into an era of 'hypothesis-free science', and this suggests that, for some, hypernomality is a progressive advancement in the maturation of a science.[28]

On the other hand, all sciences develop routine domains as one goes down the fractal and the specialisms become narrower and more sedimented, but this seems

[28] See Wiley (2008) for a discussion of this, and Stevens (2013 esp. pp 66-67) for a discussion of the way in which this kind of terminology has been subject to contestation within MBIOL.

to happen more easily in molecular biology than in sciences like gravitational physics. We might speculate that this is because of inventions such as PCR – the polymerase chain reaction – which enables multiple copies of genetic material to be produced in an hour or two. Likewise, the development of CRISPR as a technology for gene editing has often been described as 'revolutionary'. But it is important to note that CRISPR was not the first gene editing technology. CRISPR was not revolutionary so much in the sense of allowing biologists to do something entirely new, but in the sense that it transformed a particular aspect of laboratory work from something that was "expensive, technically challenging, and time-consuming" into a process that "is cheap, easy to use, and does not require expert knowhow" (Caplan et al. 2015, 1421).[29] In molecular biology this seems to mean that there are many domains where the difficulty of the task can be, and is, the size of the task in terms of numbers of repetitions required, not the basic technique: consider the sequencing of genomes as an iconic example.[30]

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[29] We might say that MBIOL is more LEGO than LIGO, more assembling components than inventing a single material and conceptual apparatus. It would be a better joke if the LEGO metaphor had not already been deployed within biology (see, for example, Winston 2012)

[30] Rabinow (1996) describes the invention of PCR in its context, not least the start-up companies and venture capital investors who provided industrial support for the rapid transformation to hypernormal science. He also describes the ethical obstacles that might have stymied the rapid progress, such as concern over the

But if our argument that the move to hypernormality rests on a cultural change that includes widespread agreement in respect of theories, methods and practices, it might be important to note that both PCR and CRISPR are tools built from already existing ‘biological machinery’ refined and proved by the process of evolution: they depend on enzymes found in bacteria. It may be that this discourages the kind of disputes over fundamentals that typify the invention of new techniques in other sciences. To deploy these tools invented by ‘Nature’ might have initially required ‘green fingers’ but no-one could argue that they could not work since they obviously work within bacteria. In contrast, in gravitational wave physics, techniques were always under dispute to the extent that important scientists resigned from the project because they did not believe that interferometry was well-enough understood to justify the building of the large-scale devices.

Significant implications of the hypernormal

A recurring mistake in today’s technically advanced societies is to think that high level performance in certain small domains indicates something far more significant in terms of the mechanizability of human abilities. For instance, world beating performance by machines at games like Chess or Go, or successful performances in solving mathematical problems which are just part of the overall

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safety of recombinant DNA and their speedy resolution by internal regulation. Here we are trying to resolve an epistemological problem but our resolution, if it makes any sense, depended on these contextual factors.

process of scientific discovery, or even mediocre performance in a restricted game-like version of the Turing Test, are described as signifying a high level of machine general intelligence;[31] likewise any extraction of previously unrecognized patterns from huge bodies of data. But general intelligence, of which science is one exemplar, requires cultural understanding in unanticipated circumstances and this is where machines fail when the test is properly designed.[32] The early days of automation of production lines (see footnote 24) as well as all machine learning up to recent times also illustrate the point. It is important, then, not to confuse the relative ease or automatability in the kind of narrow hypernormal domains in science reported here, with science as an institution: science as a whole is still a cultural enterprise, even if there are domains within it which can be described formulaically. We have described the existence of hypernormal domains because they are fascinating exceptions to the 'cultural revolution' in the analysis of science, not because they represent the future of science as an enterprise, even though the exponential growth of the power of computers will provide more and more opportunities for hypernormality.

Attitude to conferences and other face-to-face meetings

One pressing example of the difference between hypernormal domains and science as a whole can be found in the attitude to scientific conferences. We can now see that in the case of a hypernormal scientific domain, the cultures and the

[31] See, for example, Collins (2018)

[32] See, for example, Collins (1990; 2018)

trust are rapidly established in the course of early apprenticeship and that much of the subsequent communication among scientists is for the purpose of *simple division of labor*. In other words, it is a matter of *information exchange* not the establishment of a culture or the building and maintenance of trust. Information exchange can be done via various forms of remote communication. There are previously documented examples such as the design of a new rocket engine, where, it seems, the science and technology were well enough established to have allowed efficient design with only remote communication between firms.[33]
The Covid-19 pandemic has given impetus to the replacement of the scientific conference circuit with remote communication and the various benefits of platforms like Zoom have become clear. The disbenefits of the conference circuit are also clear – a high carbon footprint and the need for time-consuming and expensive travel and time spent away from home possibly exaggerating existing inegalitarian forces within science. MBIOL and GWP are both sciences and both aspire to generate uniform cultures across national diversity but for hypernormal domains this can be accomplished without face-to-face meetings. If it is true that hypernormal domains are larger and more readily found in MBIOL, as appears to be the case, this could explain why they seem to give rise to a much more vocal

[33] Management science is another domain where these matters are of concern (Purvanova 2014). Malhotra et al (2001) documents the case of the new rocket engine.

championing of the abolition of the conference circuit than is found in, say, physics.[34]

In sum, at one level, all human activity is a matter of culture: as culture changes or expands, whether this is specialist scientific culture or ubiquitous community culture, the frontiers are likely to be ill-apprehended and contested, but as time goes by, cultures sediment, meanings lose their ambiguity, and what was once new and dangerous becomes the taken-for-granted. As in the domains of molecular biology we have described, the difficult job of using language to transmit *meaning* becomes the easy job of *information* exchange. We are proposing that molecular biology scientists might be more ready to sacrifice face to face communication than gravitational wave physicists because a higher proportion of them are engaged in hypernormal domains and there is some slight evidence for this. The enthusiasm we encounter for the automation of the procedures of molecular biology and the readiness with which aspects of the science can be outsourced to commercial concerns backs this up.

To think that information exchange is *all* there is to MBIOL is to risk serious misunderstanding and we have stressed throughout that hypernormality pertains to *domains* within the discipline, though we believe there are a high proportion of such domains and a high proportion of scientists who work in them in MBIOL. It

[34] See for example, Sarabipour, et al (2021). Contrast Collins, Leonard-Clarke and Mason-Wilkes (n.d., under submission) for the case of gravitational wave physics and Collins, Barnes and Sapienza (2020) for the case of photonics.

is all too easy to think that the skills of modern MBIOL can be completely replaced by automation because it is obvious that quite a few of them can. The difficulties are being experienced in those parts of the specialty characterized by Bartlett as 'extraordinarily normal science' – that part that engages in large scale outsourcing and, large scale, *simple* division of labor. The danger reported is that the basic skills of the science may be being lost. Arribas-Ayllon et al quote a scientist interviewed during their investigation of a large consortium of biological laboratories and outsourcers:

> [W]hen we spent time in the field between 2008 and 2010 we heard complaints that consortium-based GWAS was eroding the excellence of experimental, laboratory biology … A significant problem was the 'frustrating' reliance on outsourcing. While outsourcing reduced the costs of genotyping and removed much of the skilled work at the bench, it also played a role in the displacement of a laboratory style of reasoning: The problem in 2010 we had is that a lot of the data was being sent away and there was less work to do in the laboratory. I think, over the last five or six years we've seen that as a bit of a problem …
>
> [being in the Consortium] helps people who are more biostatistical than it does people who are more laboratory based. So it's more of a tool really for the biostatisticians and the biomathematicians rather than the laboratory. (Professor of molecular genetics) (Arribas-Ayllon, Bartlett, and Lewis 2019, 208).

To mistake all science for hypernormal science and draw the conclusion that the permanent shutdown of the conference circuit would be an unallied benefit to all would be to invite a disaster for the majority of sciences that have not yet reached an advanced stage of sedimentation in their cultural development not to mention those facets of MBIOL that are still developing.[35]

The replication crisis

That there is a replication crisis in sciences that turn on low levels of statistical confirmation should have come as no surprise: physics had long discovered that results based on statistical inference could not be taken to be reliable unless the improbability of their being due to chance was at least equivalent to 5 standard deviations. This was because there are all kinds of systematic errors in experimentation that have nothing to do with random error. Physics moved to 5-sigma to try to swamp these other known, or sometimes unknown, errors. In spite of this, many other sciences are ready to report results supported by only a 2-sigma level and John Ioannidis in his 2005 paper, strikingly entitled 'Why most published research findings are false', pointed out the lack of care and consciousness of systematic errors that characterized research in medical science (Ioannidis 2005). The scandal subsequently expanded to psychology, where certain very well-known experiments proved to be unreplicable. As the

[35] The consequences are worked out at length in the context of fieldwork among physicists, in Collins, Leonard-Clarke, and Mason-Wilkes (n.d., under submission).

experience of physics shows, however, the problem is bound to show itself wherever a weal level of statistical inference is the main support of a scientific result.[36]

It was medical science where Ioannidis first located the problem. But in medical science one might well be looking for very small effects of drugs on relatively few humans each different in consequence of the huge variation of psychological state and past encounters with the physical and biological world. This is why the statistically analyzed double-blind, control trial is the so-called 'gold standard' in biomedicine. This is also why Ioannidis's suggested solution to the replication crisis – improved statistical meta-analysis of many experiments to improve the statistical odds – makes some sense. This could work where the fundamental science is hypernormal so that experimental protocols can be described and understood by information exchange across many laboratories. Under such circumstances there is some chance that the many experiments done in different laboratories are sufficiently alike in protocol that their results can be sensibly aggregated in a statistical meta-analysis. But, importantly, *meta-analysis makes no sense outside of the hypernormal domain* because ill-definition and disagreement means that it is impossible to know whether an experiment is worth adding to an aggregative exercise. One can go through the motions, but scientists will draw entirely different conclusions from the outcome, some happy with a positive result and some declaring 'rubbish in rubbish out'. This is completely

[36] The issues are discussed in Collins (2019, chap. 9).

obvious in a case like parapsychological experiments but also clear in, say, the 1970s arguments about the initial claims to have detected gravitational waves. Therefore, before a statistical solution to the replication crisis is embraced, it is vital to do a meta-meta-analysis of the nature of the science and its history. It could be that Ioannidis, coming from medical biology, has mostly encountered hypernormal science and this has influenced his preference for statistical solutions, whereas in most sciences another kind of solution is needed.

The sociology of scientific knowledge and what came after

Finally, what we have found out allows us to confirm certain features of the early years of the sociology of scientific knowledge. There was a continuing argument in those years, which has almost certainly shaped much of the history of what became known as 'science and technology studies', over the formation of scientific findings. On the one hand were the 'controversy studies' which took their purpose to be to understand how scientific knowledge was made by studying the way disputes were settled among the world-wide set of scientific laboratories which were home to the 'core-set' of scientific knowledge-makers in the case of a disputed domain. Characteristically, controversy studies involved fieldwork which depended on travel between all, or nearly all, of the scientific laboratories involved in the controversies and travel to the conference and other venues where scientists meet to argue out their differences. The influence of that legacy on this paper should be clear. [37]

[37] See, for example, Collins (1974; 1975) and Pinch (1981)

A different approach was taken by the 'laboratory studies', inspired by anthropology and semiotics, where fieldwork involved a long sojourn in a single laboratory watching the process by which knowledge was made at the bench. The most famous and influential of these laboratory studies was Latour's sojourn at the Salk Institute, which was published in 1979 as Latour and Woolgar's *Laboratory Life*.[38] Latour's boast was that in spite of his time at The Salk, he never understood any of the science nor tried to. This was hugely influential because it legitimated a large body of studies emerging from the arts and humanities in which the researchers felt no need to understand the science they were reporting.[39]

Retrospectively, it is obvious that Latour was going to prefer to describe knowledge as made within a single laboratory because he had no access to the places were the output of the single laboratory was consumed, and he was going to place a great deal of stress on the large amount of writing, or 'inscription' that he observed taking place – he was acting as a semiotician, not a scientist, nor did he wish to become a scientist, and the same applied to his large body of followers. Those studying scientific controversies, in contrast, tried to understand the science, hence the centrality of spoken language and interactional expertise in the analyses that followed.[40]

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[38] Latour and Woolgar (1979)

[39] See Collins (2012) for more on this

[40] See Giles (2006) for an example of the importance attached to this.

The difference between the two approaches can be seen in the role of Latour's iconic term, 'immutable mobile'. Latour considered that the output of scientists were inscriptions and the end-product of a passage of laboratory work – a publication – represented the sedimentation of scientific knowledge. Once the output was inscribed it was immutable and could travel the world without change. Those for whom controversies between multiple laboratories were more salient argued, in contrast, that in scientific controversies, nothing coming out of a laboratory was trusted even if it had been published – everything was mutable! Shrager published his diary in 2004 without knowing of the ongoing debate in social studies of science, but stressed that writing was absolutely essential for keeping track of the transparent liquids that were being pipetted, and what they all meant, but he also stressed that this was not the essence of the science,

> [C]onsider Latour and Woolgar's (1979/1986) famous study, "Laboratory Life." These anthropologists undertook an extended participant-observer study of a molecular biology laboratory; one of them worked as a technician in the laboratory for 21 months. They became fascinated by the process of *inscription,* noting that the members of the laboratory are "compulsive and almost manic writers" (p. 48), including the writing of scientific papers, filling out of tracking forms, labeling of tubes and materials, and entering information into notebooks of various sorts. Latour and Woolgar understand the laboratory as "a factory where facts [are] produced on an assembly line" (p. 236), implying that science involves the production of "facts" through the manipulation of inscriptions-scientific

> facts and theories being thus reduced to a kind of inscription. (Shrager 2004, 121)

But Shrager claimed that this could be thought to be the essence of the science only by someone who was intent on observing it from the outside. Shrager explained that he, by contrast, in trying to become a biologist, was concerned with cells, and DNA and the like, not inscriptions.

> Because Latour and Woolgar's (1979/1986) observer did not have the goal to actually become a molecular biologist, he did not find himself compelled to reason about the relevant objects: proteins, DNA, and so on. (Shrager 2004, 124)

But now we can understand the ready acceptability in certain quarters of what seemed to Shrager (and to Collins et al), Latour and Woolgar's incorrect conclusions.[41] Latour and Woolgar's claim that inscriptions were immutable mobiles continues to appeal in much of modern MBIOL because in many domains of MBIOL they *are* immutable mobiles – it is an information exchange science[42]. What they are describing is a particular kind of science – hypernormal

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[41] Not that to draw these conclusions was not a hugely successful strategy but let us assume that, like other scientists, they were more concerned with the truth of the matter than strategy, and that they thought they had located it.

[42] Lewis and Bartlett (2013) describe the kind of high-thoughput MBIOL that emerged in the wake of the HGP as involving the production of mass inscription; codes, sequences, structures etc. deposited in standardised forms in databases

science – in which most of the difficult knowledge has already been hammered out and sedimented. Whether this is because the Salk Institute of the time was engaged in hypernormal science, or whether it was because Latour stayed in only one location and did not experience the controversy associated with domains that are not hypernormal, we do not know, and cannot know because, as Latour himself explained, his method did not involve gaining an understanding the science. It is a shame that the method, or perhaps, the domain, into which Latour and Woolgar stumbled, led them and many others to accord their claims far more metaphysical significance than they have; they had studied and described the surface activities of a controversy-free information exchange-based science! They thought they had seen the process of sedimentation of science – the production of immutable mobiles – but they had put the cart before the horse. In much of modern molecular biology mobiles are mostly treated as immutable because they are treated as information – and that is because the science has already reached the hypernormal state.

Conclusion

This new division of the sciences into normal, depending on the continuing consolidation of culture and trust, and the hypernormal that work through information exchange, arose from our attempt to 'square' the universal presence

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accessible to the wider scientific community, a process which implies the erasure of any judgements of experimental virtuosity, any contestation over whether an experiment has been done correctly.

of tacit knowledge in scientific research, and its relationship to the building of scientific cultures – as more than adequately demonstrated in studies of physics – with the enthusiasm for and actuality of automatability in much of bioscience. In our account hypernormal domains are much more widely distributed, larger, and more influential in bioscience than in physics so that science can function via information exchange more readily in the bioscience area than in physics. All sciences are essentially the same but the rapidity and extent to which they sediment is different. This is profoundly important for the theoretical understanding of the nature of science and has many practical implications for science's future, some immediate.

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Figure 1 and caption

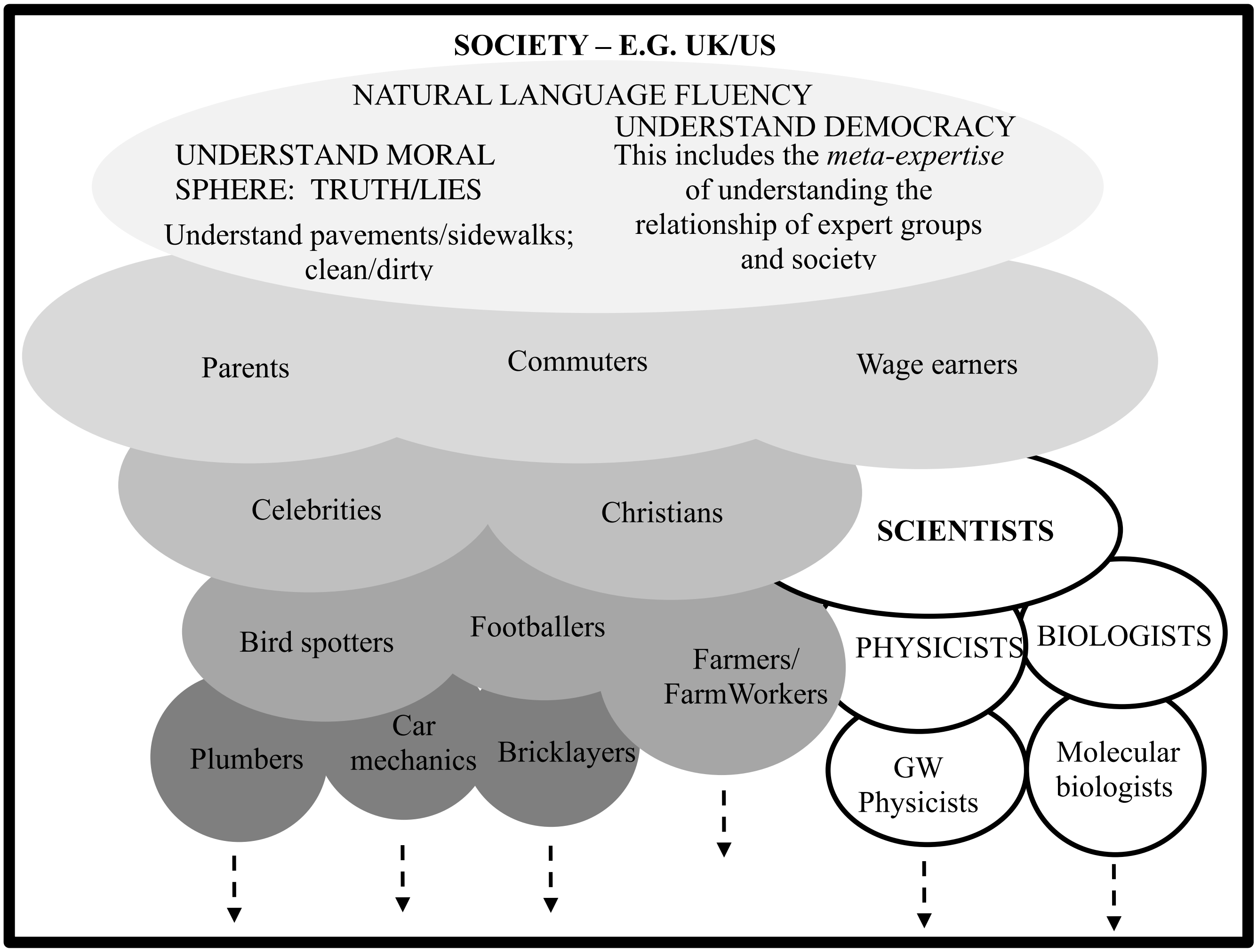

Figure 1: A simplified fractal model of society (science emphasized). The cascades continue as indicated by the dashed arrows. The actual complexity of the mutual embedding, being many dimensional, has to be imagined.

Figure 2 and caption

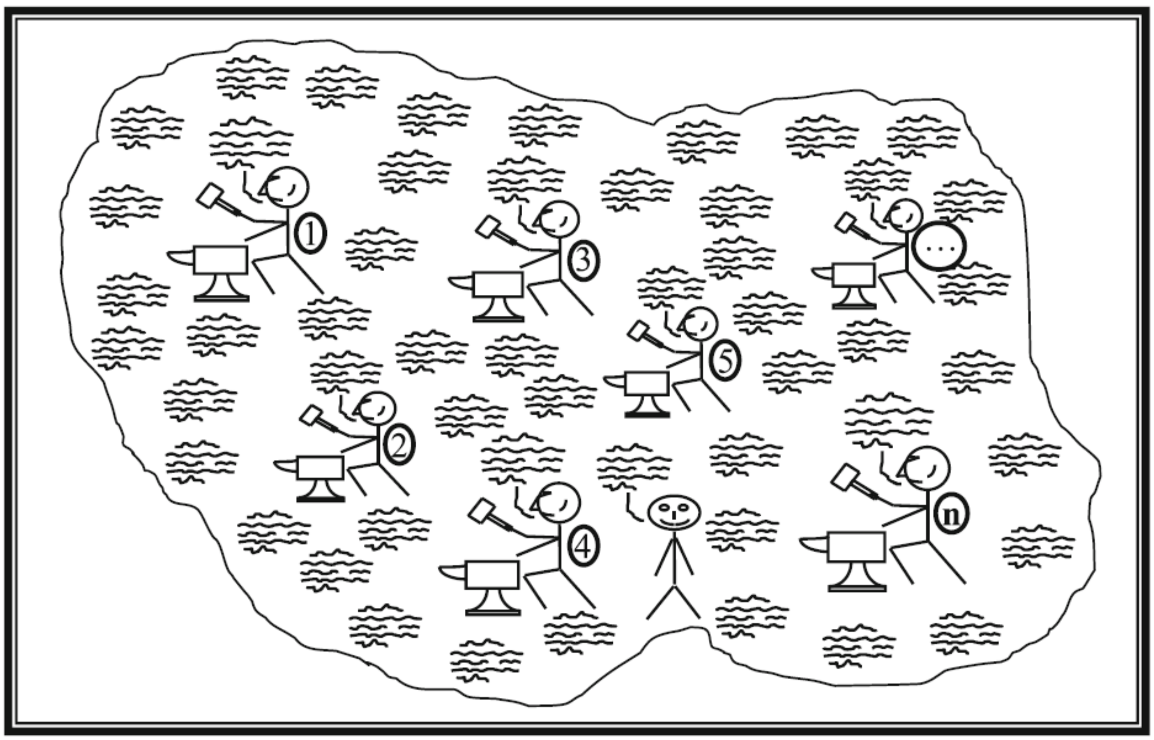

Figure 2: A practice domain – inspired by GW physics (originally Collins 2011, fig. 2)