Aspects of Informal Mathematics

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This is a transcript of my intended talk at the GAP.6-workshop "Towards a New Epistemology of Mathematics" (14-16 Sep 2006). Motto of the talk was to be Otto Neurath's dictum "Confidence cannot be intellectualized" (1921).

1 Introduction

The point of departure is the contrast between the *received view* in the philosophy of mathematics, as the study of logico-mathematical questions of a foundational nature, and what Aspray and Kitcher have at a point called a *maverick tradition*, arising the last few decades, where the focus is rather on mathematical practice, i.e., on mathematics as a research activity, as a human affair important aspects of which are also to be found outside the realm of published results and their formal justification.

Quite rightfully, this contrast is called into attention now and then. A few years ago, David Corfield did so, introducing the concept of 'foundational filter' in the philosophy of mathematics, in his book *Towards a Philosophy of Real Mathematics* (CUP, 2003). Most recently the historians of mathematics José Ferreiros and Jeremy Gray did so, in the commendable introduction to their reader *The architecture of modern mathematics* (OUP, 2006).

Let us approach this matter by asking ourselves 'what kind of man or woman is a mathematician'?

Observation: The major schools in the philosophy of mathematics, especially during the past century, have typically associated themselves with a particular type of (either godlike or human) *ideal* mathematician.

E.g.

Platonists (methodological or not): someone with perfect intuition of the mathematical realm.

Empiricists: the ultimate scientist, studying the very essence of existence within spacetime.

Logicists: a fully rational agent.

Formalists and conventionalists: an entirely free agent.

In general:

> Ideal mathematicians are usually assumed to be infallible, eternal or atemporal, unlimited in memory or complexity, isolated from other mathematicians, mental beings without contexts. [. . .] In so far as actual mathematicians err, they *fail* to approximate ideal mathematicians and so are of no concern to philosophy. (Thomas Tymoczko, 1986)

Contrary to this, mathematical humanists are prepared to grant a considerable, if not central, role to *actual* mathematicians. That is: human beings inherently 'cursed' with loads of personal (ideosyncratic) and cultural (shared) traits that inevitably turn them into contextually constrained instead of godlike epistemic agents.

If the real mathematicians are actual people, rather than ideal constructions, then philosophical reflection looses it *a priori* normative character, for in order to properly evaluate, it must first feed itself with empirical material describing the very practice it is about to judge. A form of epistemological naturalism enters the picture here.

Another consequence for philosophical inquiry, if it is to concern itself with mathematics as it is found, is the externalist move. This implies the view that:

> The philosophy of mathematics is neither mathematics nor a subset of mathematics. It is a field of study which reflects on mathematics from the outside. (Paul Ernest, 1998)

So, the question is: What is the philosophy of mathematics, *really*? A question that is central to the concerns of the PhiMSAMP-group the organizers of this workshop are among the founding members of.

What I shall do from here, is look into the matter of the formal limits of *real life* mathematics, that is look into a number of arguments as to the inevitability of a minimally humanist philosophical course. The first part will remain general, the second part will particularly deal with the social character of mathematical inquiry (as you might have noticed an important topic of future inquiry within the PhiMSAMP-network). In this respect, here, I particularly want to forward a number of views on the concept of 'scientific genius'.

2 Formal limits - The human mathematician

Following the lead of the naturalists, as we do, philosophers should not just consider detached mathematical theory, but also –not only, but preferably in the first place– mathematicians and what these actually do, which includes theorizing only as one among many other things.

About ten years ago, the Canadian philosopher (and specialist in Category Theory) Jean-Pierre Marquis has proposed what I think to be a very interesting and useful sixfold subdivision of different ways in which to conceive of the notion 'foundations of mathematics'. As only one (or at most a few) of these coincide with the traditional conception, Marquis has sought to liberalize the notion in question.

1) Logical foundations consist of a deductive system, including primitive concepts, properties and inference rules, on the basis of which entire mathematical theories might be (re)constructed.

2) Epistemological foundations delimit, usually according to some virtuous property of ideal cognizers, what are acceptable pieces of mathematical knowledge and how these should be arrived at.

3) Ontological foundations specify what are genuine mathematical objects and their properties.

4) Semantical foundations provide with models interpreting the logical foundations and linking these with the ontological ones (via reference relations).

5) Cognitive foundations, whether strongly or weakly interpreted, establish the sufficient or necessary conditions for humans to successfully discover new (heuristics) and/or get passed on existing mathematical knowledge (pedagogy).

6) Finally, methodological foundations lay out the principles and methods that when used by mathematicians lead to legitimate constructions.

Contemplating the road to the foundational crisis, it should be clear that it was through late nineteenth century *intra-mathematical* problems such as coping with non-Euclidean geometry or manifestations of the infinitely small, that epistemological foundations became the most important topic of explicit philosophical interest.

Despite the ensuing mathematical creativity and philosophical flexibility, the worst was yet to come. By the early twentieth century, the paradoxes of set-theory apparently led foundational theory into true impasse, and then, by the 1930s, Gödel dismissed its very principle.

In the meantime however, starting with Frege, an irreversible process of formalization had taken hold of philosophy of mathematics. Indeed in the hands of the big post-Fregean schools then, past Gödel, philosophical focus gradually shifted to reductionist logical foundations.

From this period, although it actually became one of its highly specialized branches, philosophy (aka foundations) of mathematics definitely got out of touch with the rest, i.e., the bulk, of mathematics.

That is, after having been exposed as a dead end in principle, also in practice did it more and more turn out a hopeless *and* pointless affair of trying to squeeze all of 'extraordinarily diverse' mathematics into one single formal framework.

> Mathematics consists of a great diversity of theories. There is, for example, the theory of computability, the theory of games, the theory of groups, the theory of probability and so on virtually endlessly. It is very difficult to see how these could all be derived from the same small initial set of mathematically unproblematic propositions. [...] The enterprise of finding an intra-mathematical foundation for all of mathematics is about as dead as any enterprise can be. (E.E. Sleinis, 1983)

As a result of all this, when *mathematicians* are using the term 'foundations' today, they commonly do so in the methodological sense. More particularly, the possibility of transferring particular tools between (sub)fields would be referred to. Another, related sense often appealed to is that of (non-reductive) organizational power. While some of the foundational ways discerned by Marquis, such as heuristics, might still be getting their complementary place under the sun, it must be clear that at least logical foundations can be no issue at all here.

In the present circumstances, following Marquis, at least one supplementary philosophical task other than the devising of proper foundations (of whatever type) themselves can be readily pointed at, viz., the task of ordering all above types according to their importance. The consequence will be that some of the foundational ways to have high, and others low status, relative to one another.

For mathematical humanists, it is clear that any such exercise should put the formal realm, their main target of critical scrutiny, in due perspective. To serve this end, drawing upon the sociologist Erving Goffman (1922-1982), the 'maverick' mathematician Reuben Hersh has famously developed the analogy between two distinct realms of mathematical activity and as many different regions in restaurants or theatres. At this type of places, servants or actors alternately find themselves 'on stage' and 'backstage', exercising different occupations and –correspondingly– behaving differently.

> Compared to 'backstage' mathematics, 'front' mathematics is formal, precise, ordered and abstract. It is separated clearly into definitions, theorems, and remarks. To every question there is an answer, or at least, a conspicuous label: 'open question'. The goal is stated at the beginning of each chapter, and attained at the end. Compared to 'front' mathematics, mathematics 'in back' is fragmentary,

> informal, intuitive, tentative. We try this or that, we say 'maybe' or 'it looks like'. (Reuben Hersh, 1991)

The purpose of this separation between front and back in mathematics, says Hersh, "is not just to keep the customers from interfering with the cooking; it is also to keep the customers from knowing too much about the cooking".

An important consequence hereof is that what meets the eye is, strictly conceived and to a certain extent, a myth, for it conceals the 'ugly truth' of the back office. Concerning mathematical knowledge, the myth (a term also having been used in this respect, if I am not mistaken, by Philip Kitcher), i.e. the covering up of what is *really* going on, displays elements such as unity, objectivity, universality, certainty; elements to be found in all the 'great' philosophies of the discipline, from Euclid to Bourbaki. So, whenever public presentation is intended, these hallmarks are to be safeguarded come what may. Consequently, the various philosophical schools have developed the specific styles they thought would suit that end best.

The alternative picture is one of demystifying foundational scaffolds as being *ex post* rather than *ex ante* constructions. The latter image aptly summarizes the view of the mathematical naturalist, according to whom the 'true' foundations of mathematics are found in the back, where, as noted above, frontstage myths are not taken naively at all.

Now, in recent decades, there seems to have occurred a dramatic increase in (the awareness of) the 'informality' of loads of mathematical results, provisional or definitive, with a rising number of extremely long, complicated, digital, specialized, experimental or otherwise elusive proofs putting to the test the limits of our human (mechanical or intuitive) mathematical powers.

There's the powerful example of experimental mathematics (broadly conceived), dealt with during this conference by Alan Baker. I'll cite a number of additional cases in point further on. Philip J. Davis, in the late 1960's indeed for himself arrived at a rough sketch of the alternative picture that "mathematics, in some of its aspects, takes on the nature of an experimental science". According to him, empiricizing ideas like these should in fact have gained momentum from the late 1960s on, once computers had gradually become available as more or less reliable assistants.

But, as Reuben Hersh noted only a few years ago, "mathematicians themselves seldom discuss the philosophical issues surrounding mathematics; they assume that someone else has taken care of this job. We leave it to the professionals", the latter of course supposing to be philosophers.

Looking back on an entire career in mathematics, in his memoirs, Davis confirms and specifies: "Most practicing mathematicians care very little about discussing the philosophy of their subject, but they work unconsciously with a philosophy of Platonism. [...] If the shortcomings of Platonism are pointed out, mathematicians usually fall back on formalism". With this he echoed the following classic lines from his famous joint book with Hersh, *The Mathematical Experience* (TME):

> Most writers on the subject seem to agree that the typical working mathematician is a Platonist on weekdays and a formalist on Sundays. That is, when he is doing mathematics he is convinced that he is dealing with an objective reality whose properties he is attempting to determine. But then, when challenged to give a philosophical account of this reality, he finds it easiest to pretend that he does not believe in it after all". (Davis and Hersh, 1983)

However, when it comes to challenging this careless and superfluous attitude, it seems "the professional philosopher, with hardly any exception, has little to say to the professional mathematician. Indeed, he has only a remote and inadequate notion of what the professional mathematician is doing".

This obvious neglect might be linked to the –philosophically unhealthy– preoccupation, throughout the twentieth century, with shaken foundations, i.e., the 'aftermath' of the famous foundational crisis, which has been a matter of hardly any immediate relevance to practicing mathematicians.

> People noticed that in their normal everyday work as mathematicians you don't really find results that state that they themselves are unprovable. And so mathematicians carried on their work as before, ignoring Gödel. The places where you get into trouble seemed too remote, too strange, too atypical to matter. (Gregory Chaitin, 1999)

Therefore, the mathematical humanist would claim with Yehuda Rav, that "it's important to remember that mathematics is not an edifice which risks collapse unless it is seated on solid and eternal foundations that are supplied by some logical, philosophical, or extra-mathematical construction". This idea was magnificently captured in the following legendary extract of contemporary philosophy of mathematics, by the hand of the late Morris Kline:

> The developments in this [twentieth] century bearing on the foundations of mathematics are best summarized in a story. On the banks of the Rhine, a beautiful castle has been standing for centuries. In the cellar of the castle, an intricate network of webbing had been constructed by industrious spiders who lived there. One day a strong wind sprang up and destroyed the web. Frantically the spiders worked to repair the damage. They thought it was their webbing that was holding up the castle. (Morris Kline, 1980)

When Davis and Hersh wrote their aforementioned joint book, TME, the straightforward point of departure was the sheer confusion of mathematicians who decide to lecture on the philosophy of their discipline, to realize they cannot even begin to explicate their proper opinion about crucial matters like the nature of numbers, sets or proofs.

More recently, Hersh has diagnozed this confusion quite aptly.

> When you're a student, professors and books claim to prove things. But they don't know what's meant by 'prove'. You have to catch on. Watch what the professor does, then do the same thing. Then you become professor, and pass on the same 'know-how' without 'know what' that your professor taught you. (Reuben Hersh, 1997)

The joint book TME subsequently reflected the quest, from within a mathematician's working reality, for answers to this type of questions, calling into attention their importance.

In 1995 though, Hersh still judged it necessary to lament the absence, so far, in the philosophy of mathematics, of inventive figures like Karl Popper, Thomas Kuhn, Imre Lakatos or Paul Feyerabend. Philosophers, that is, who would 'liberate' mathematics, in the way the ones mentioned, despite their dramatic departures, had done for science in general: on the common assumption that "philosophers of science could think about what scientists actually do, not bring presuppositions and instructions for scientists to ignore". This would arguably break the deadlock between "philosophers of mathematics [who] ignore mathematics and mathematicians, and mathematicians [who] find nothing of interest in philosophy of mathematics".

The suggestion, I guess, is to acknowledge that mathematical proofs do not consist of or develop via deductive procedures alone. Indeed it could be argued that formal proof does not exhaust the category of rigorous, i.e., mathematically accepted, argument. We have already pointed to a number of *inductive* or experimental methods applied in the course of both the process of discovery and justification in mathematical practice. But this methodological type can be further broadened, so as to include not 'just' the preparative or informal stages on the road to formal proof, but also the ways these proofs get supplemented or framed, in order for the intended audiences to grasp their content and judge their quality.

William P. Thurston, when involving in a famous debate on the specificity of mathematical inquiry, has addressed the matter of proof *vs* progress in mathematics. What exactly are they, what is their role in mathematical practice and how do people relate to them? Questions like these are bound to take the philosopher of mathematics way beyond the matter of whether a newly proposed piece of mathematics, a proof, adds to the collection of established truths or not. More particularly, it raises the issue of mathematical *understanding*, and that of the conceptual dynamics facilitating it. As we have noted, the rise of computer proof has brought this matter to the fore very strongly. If we really want to get a grip on this topic, Thurston complains, then the traditional DTP or Definition-Theorem-Proof-model of mathematical practice should be put to the test, and some of its *psychological and social dimensions* should become the object of consideration as well. Thurston illustrates this point by listing various ways of 'conceiving of' the derivative of a function, and then distinguishes a number of alternative but non-exclusive modes of mathematical thought contributing to a grasp on mathematical units (propositions or proofs) in general, among which the linguistic, visual, logical, and dynamical ones. At least one of these modes seems to have a distinctively non-formal ring about it. As Thurston notes:

> People have amazing facilities for sensing something without knowing where it comes from (intuition); for sensing that some phenomenon or situation or object is like something else (association); and for building and testing connections and comparisons, holding two things in mind at the same time (metaphor). These facilities are quite important for mathematics. (William P. Thurston, 1994)

Rigorous but non-formal arguments often come in the format of proof-outlines: sketches of the characteristic global shape of a proof, e.g., infinite descent, splitting in cases, or *reductio* (Van Bendegem, 2000). Unlike many of their formal counterparts, these typically have the virtues of surveyability and robustness (MacKenzie, 1998). The reason is simple: they are concise but powerful ways of grasping the upshot or meaning of proofs, their overall structure being what counts, not so much meticulous detail. Because of this, they facilitate the transfer of particular proof techniques to other contexts, for example more general ones; i.e., the association facility referred to by Thurston above.

3 Individual limits - The social mathematician

Retaking an earlier quote (and further concentrating on part of it),

> Ideal mathematicians are usually assumed to be infallible, eternal or atemporal, unlimited in memory or complexity, *isolated from other mathematicians*, mental beings without contexts. [...] In so far as actual mathematicians err, they *fail* to approximate ideal mathematicians and so are of no concern to philosophy. (Thomas Tymoczko, 1986)

However, contrary to this, Tymoczko continues, "it is the community of mathematicians, not a single isolated mathematician that is central to philosophical concerns". In order to make his case for mathematics as a public affair, against individualist epistemology, Thomas Tymoczko has been particularly connecting with both elements of cognitive foundations as discerned above by Marquis, viz., pedagogy and heuristics. The first facet touches upon the issue of the persistence of the mathematical community through time, which constantly requires new mathematicians to be initiated (from elementary to advanced levels).

From this simple observation, it appears that procedures for doing so will have an important bearing on how the community's continuity will take shape, which is of significant philosophical relevance. For example, it seems that from an educational point of view, a quite different selection of intra-mathematical foundational theories is bound to be made, as instead of logic and/or set theory, one would rather come up with algebra, geometry, calculus and computer science, these unifying not abstract mathematical theories, but the basic mathematical experience to be confronted by students and teachers. Also, the pedagogical perspective draws full attention to the context of discovery, which has been totally left out of sight by traditional individualist epistemologies, for whom verification and justification of the mathematical finished product is all that matters, as opposed to the road(s) towards it. Conversely, a perspective favourable to learning processes could make room for embracing some of the 'nasty' properties of mathematical practice, such as error. In order to establish the philosophical instead of –or in addition to– the merely sociological importance of the educational, one should move to the other, second level: heuristics, from where (s)he might try to render plausible (not more than that, this matter essentially remaining one of (dis)belief to an extent) that the social actually does or can constitute mathematical knowledge, that "the private act derives from the public act and not vice-versa".

The bottom line of any argument in favour of this is that mathematical progress, through (self-)criticism and the recognition of error, is only possible within the context of communities: mathematical activity consists of processes whereby results are *being* proved, and the results themselves are not to be detached from this creating process. Indeed, note that in the end, what convinces other mathematicians of the legitimacy of this or that result, is never the paper proof by itself, but an entire set of complementary unilateral messages and reciprocal discussions surrounding it. Consequently, as the informal is apparently at work in the context of justification also, proofs transcend formal systems, and proving remains an open-ended process.

> Informal proofs *are* the actual proofs of mathematical practice; the issue is how to *interpret* these proofs. (Thomas Tymoczko, 1986)

The broadly Lakatosian heritage contains a stock of concepts available for attempting at carrying through the latter task: conjecture, proof-idea, lemma, criticism, counterexample, modified proof, equivalent versions, generalization, application, reinterpretation and aesthetic considerations.

> As a proposal, this way of looking at proofs would have the effect of decentralizing the philosophy of mathematics. It implies that a great deal of philosophical understanding must be gained on the local level. (Thomas Tymoczko, 1986)

Let me here invoke just two concrete, and radically different examples of cases where the social (i.e., the limits of the individual) inevitably enters the picture. The first is that of Fermat's Last Theorem, where socially laden issues about reviewing, specialization, authority, and priority should be addressed (e.g., the matter of the extremely limited number of experts able to deal with the purported proof). The second is that of the Classification Theorem of Finite Simple Groups, where, as the full proof seems to consist of about 15.000 pages in print, especially question about division of labour, both in place and time, force themselves. In both these cases, there are ready philosophical doubts about whether it is feasible for *any* indivual to grasp the theorems in question and their elaborate proofs.

Mathematical Genius: Individual Talent or Social Grace?

In search of support for the social primacy thesis in mathematics, one should no doubt at some point deal with one of the most fascinating and appealing phenomena of cultural history: that of 'genius' or 'exceptional individual'. Tackling this subject, the anthropologist **Leslie White**, midway the former century, has defended the view that mathematical truth does not ultimately reside in an abstract or ideal realm, either of objective (Platonism) or subjective making (intuitionism), but within cultures.

Here,

> the contradiction between the view [...] that mathematical truths are discovered rather than manmade is thus resolved by the concept of culture. They are both; they are discovered but they are also man-made. They are the product of the mind of the human species. But they are encountered or discovered by each individual in the mathematical culture in which he grows up. [...] Mathematics is the psychosomatic response to the mathematical culture. (Leslie White, 1949)

White thereby rules out the significance of the individual quasi completely, that is, except for its being the biological vehicle or 'neural locus' for conceiving and passing on mathematical knowledge. This knowledge itself as well as its growth, White holds, are constituted by the community. As for the individual contribution to crucial breakthroughs and related phenomena, while conceding that some sets of brains indeed turn out to be better suited for 'carrying' mathematical knowledge than others, this is merely comparable to some types of wire being better electrical conductors than others. “If the proper cultural elements are lacking, superior brains will be of no avail”.

> There were brains as good as Newton’s in aboriginal America or in Darkest Africa. But the calculus was not invented in these other times and places because the requisite cultural elements were lacking. Contrariwise, when the cultural elements are present, the discovery or invention becomes so inevitable that it takes place independently in two or three nervous systems at once. Had Newton been reared as a sheep herder, the mathematical culture of England would have found other brains in which to achieve its new synthesis. One man’s brains may be better than another’s, just as his hearing may be more [keen] or his feet larger. But just as a ‘brilliant’ general is one whose armies are victorious, so a genius, mathematical or otherwise, is a person in whose nervous system an important cultural synthesis takes place; he is the neural locus of an epochal event in cultural history. (Leslie White, 1949)

However, this generally passes unnoticed: although the weight of the tradition on most individual minds is overwhelming, only seldom does it get recognized for what it is.

One may notice that scientists or artists very often speak of a divine inspirational instance guiding them with seemingly compelling force. But that so-called external intervention need not have anything mysterious about it, if what is felt is just the tradition’s powerful embrace, which usually will choke excessive creativity or at least leave at a normalized level, but also at occasions inspires its most sensitive and energetic minds to freshly synthesize and then –indeed– apparently transcend the whole of it.

One of the most outstanding (non-mathematical) examples ever of this kind of ‘genius’ must have been Wolfgang Amadeus Mozart (1756-1791). The Spartan musical education he received from his father Leopold, in combination with his remarkable talent and sensitivity, allowed him to brilliantly redefine the prevailing musical canon. However, for practical but also artistic reasons (both of course being intimately related), he could not do so but from within, then at and eventually –presumably to his ruin– past the borders of the tradition of the day (pre-romanticism).

Well then, equally so in mathematics, the present theory goes. At its earliest stages, the initial conception of its most rudimentary ideas surely must have been the work of individuals. However, without a culture facilitating their exchange and confrontation (e.g., through symbolic representation), the human race would have never passed beyond this phase, neither in its entirety nor in any of its later individuals. Compare how elementary mathematical skills are now being discovered in hominoids as in babies. Yet, other than human infants, the former do not succeed in extending these ‘innate’ capacities, presumably for lack of brain power, but –evolutionary related to that– also because they find no 'independently' developing stock of more elaborate mathematical concepts in which they are further immersed. Instead, with every generation, they simply have to start again from scratch.

Complementary to this, the pragmatist philosopher and psychologist **William James** (1842-1910), in his famous lecture *Great men and their environment* (1880), has likened social and biological evolution, suggesting a form of social Darwinism to be applicable to the field of scientific research. In this eloquent text, he dismisses predestination or determinism in earthly affairs, but even upon its acceptance deems full knowledge about its working, i.e., omniscience, as being outside the realm of the human mind, as the latter

> has no such power of universal intuition. Its finitiness obliges it to see but two or three things at a time. If it wishes to take wider sweeps it has to use 'general ideas', as they are called, and in doing so to drop all concrete truths. (William James, 1880)

Instead, having specific purposes in mind most of the time, man usually prefers to narrow his scope, picking out a few objects of close attention at most, largely ignoring the rest.

At his time, James considered the neglect of infinitesimals, being real but superfluous quantities, a good mathematical example. As he argues, this general attitude has genuine survival power: a captain navigating a vessel through a battle should not attend to as many factors bearing upon his situation as possible, but rather concentrate on the couple of crucial parameters, if he wants his crew to stand a chance.

Now James applies the Darwinian lesson to science, viz., the relation between individual and social dimensions in research. That is, the genius exhibited by individuals he considers a physiological accident (the result of random variation in the population, which is however not at odds with any natural laws), while it is entirely up to the social environment to preserve or destroy these particular talents.

The physiological and social processes, says James, belong to different 'cycles' that have no bearing upon each other.

> The same parents, living in the same environing conditions, may at one birth produce a genius, at the next an idiot or a monster. The visible external conditions are therefore not direct determinants of this cycle. (William James, 1880)

In short, what James claims is that geniuses are not socially produced, but are instead given as the results of spontaneous variation. Genius can be destroyed or left undeveloped under external forces, for sure, but not be passed on.

Although one should of course note that this was the end of the nineteenth century only, James has gone rather crudely about the matter. For one, to boldly transfer biological Darwinism to the intellectual domain passes over many a purportedly important disanalogy between both domains. E.g., the latter realm comparatively allows for much more flexible, i.e., creative and fast, reactions to the limitations imposed by the social environment (which stretches the array of available adaptational techniques). For another, James fails to say anything significant about the ways of genius in a *positive* manner, that is, apart from the alleged selecting away of unfit candidates.

Take the brilliant Indian mathematical autodidact Srinivasa Ramanujan (1887-1920), who made a good number of stupefying discoveries and booked several amazing results in a quasi purely intuitive way, while on the other hand, about half of what he came up with appears to have been near to complete nonsense (at least to the modern Western mathematical mind). One might think that, had he enjoyed formal education, that would arguably have developed his talent, so as to substantially diminish the share of ludicrous ponderings.

However, alternatively, it could have as easily choked his talent. With reference to a previous example, it was one of the remarkable features of Mozart to have survived (i.e., digested) as a child prodigy the overload of impulses directed towards him, especially by (or at least under the influence of) his father. Then again, this strength might also be considered as one of the determining aspects of 'true' genius, of course.

So while on the face of it, the awarding of this title 'genius' surely is not supposed to be a matter of mere degree, education (or socialization in general) *has* a possible influence on the moving individual people up (or down) the scale, when the ability to produce splendid connections or wonderful flash insights is concerned. To build upon one of James's examples: one who is born a natural idiot in one family might in the end find himself higher on the intellectual ladder than someone else being born a natural genius in another family. Comparatively, genius cannot just be destroyed, it can also be cultivated.

Leaving behind James, in the middle of the previous century, more fine-grained analyses have been offered by Michael Polanyi and Robert Merton. For **Michael Polanyi**, on the one hand, genius appears as a gift, on the other, it seems to have to maintain itself through strenuous efforts. Great discoveries might sometimes appear to be falling from the sky, but inspiration rarely calls upon one without prior respiration. Notions like 'intuitive' or 'spontaneous' are likely to be invoked at this point.

> Our imagination, thrusting towards a desired result induces in us an integration of parts over which we have no direct control. We do not *perform* this integration: we *cause it to happen.* The effort of our imagination evokes its own implementation. (Michael Polanyi, 1974)

Discovery, for Polanyi, requires three essential faculties: the sighting of a good problem, the quest for a solution, and the drawing of a conclusion. The first is a guessing or estimating game, but as for example the occurrence of multiples shows (calculus, non-Euclidean geometry, informational complexity), scientists are usually good at it, and the anticipation powers of mathematicians are no exception to this. As for the second, Polanyi sees a parallel to vision, we humans being able, out of fragmentary clues, to constitute a coherent picture somehow carrying the promise of, or suggesting, a solution lying behind it (although in actual sight the image is certainly more vivid, the difference is said to be one of degree). An increasing sense of coherence reduces the indeterminacy of the enquiry, and subsequent experimenting further narrows down the scope, until (seem to) present themselves concrete ideas that will eventually lead into a definite solution (the third facet).

Two things spring from this. First, instead of severing the formulation of hypotheses and their testing/proving as two distinct, resp non-rational and rational phases, both are intimately connected: scientific discovery, for Polanyi, starts long before subsequent testing/proving (sometimes the latter is even left out all together), or consists in proposing a new interpretation of data already available.

Second, non-tangible personal powers of the mind are central to the account. Polanyi adheres to a *Gestalt*-psychological explanation of scientific theory, thinking of the latter as a cognitive equilibration effort of various pieces of experiential material, an effort which first reveals a hitherto hidden reality. Contrary to Kuhn, Polanyi does not primarily see this as a collective or paradigmatic, but instead as highly individualized thing, which adds up to the inexactness of all science: essential tacit elements remain hidden in each person's individual appreciation. Human knowledge is initially tacit to a large extent, i.e., exceeding the expressible, and then undergoes consecutive emergent restructuring of its cognitive hierarchy (which reminds of Piaget), which gradually comes to resemble that of the objects of knowledge.

Contrary to James and Polanyi, then, **Robert Merton**, the champion of early sociology of science, has conceived of scientific genius in utterly social terms. "In this enlarged sociological conception, scientists of genius are precisely those whose work in the end would be eventually rediscovered", he claims. That is, individual geniuses are taken as the functional equivalents of arrays of scientists with varying talent. So, while it might take more individuals and moreover their coordinated effort over a longer (or shorter!) period of time, the individual genius, at least in the sense of being *hors catégorie*, as a subject is not quite indispensable, despite clearly outranking his peers qualitatively. There is in the end but a difference in degree.

Merton finds support for his theory in brilliant scientists having invariably been involved in important discoveries in general, and in multiples specifically (e.g., he points to thirty-two such instances for Lord Kelvin alone), at least to an extent considerably superior to that of their 'ordinary' colleagues. Still, he maintains, indeed they seem not to have been strictly indispensable for any of these.

> The sociological theory of scientific discovery has no need, therefore, to retain the false disjunction between the cumulative development of science and the distinctive role of scientific genius. (Robert Merton, 1973)

While clearly being at the opposite end of the spectre, as compared to James, Merton adds that while made discoveries for him could have been reached with quite another mix of individual genius and collective effort as well, he will not have it that particular discoveries are or have been inevitable, any which way.

Interestingly, the mathematician **Raymond Wilder**, near the end of his career, has also touched upon this topic, for the specific case of mathematics. He confirms the minimal requirement of a suitable social environment, when considering the scientist ahead of his time, thus ignored and later rediscovered, thereby meriting himself the label of 'genius'.

> We can accept that he was undoubtedly an unusual person, perhaps a genius, but without some motivation and something to base his ideas upon, his creative faculty could not have progressed far. (Raymond Wilder, 1981)

Therefore, Wilder has wondered how some allegedly misunderstood geniuses actually related to their area of interest as well as more general culture. A classic case in point would be the Moravian monk Gregor Mendel (1822-1884), pioneer in genetics, well known for his pea-experiments, but only rediscovered and acknowledged as such in 1900.

For mathematics, Wilder proposes the names of Bernhard Bolzano (1781-1848), for his original work in real analysis, as well as the French military architect and engineer Gérard Desargues (1593-1662), for his visionary anticipation of projective geometry, in *Brouillon projet d'une atteinte aux événements des rencontres d'un cone avec un plan* (Proposed draft of an attempt to deal with the events of the meeting of cone with a plane; 1639), which according to Struik remained unrecognized until the nineteenth century.

If "Desargues' work was fully as brilliant as anything to be found in the contemporary analysis", Wilder wonders, then why was it abandoned? Well, to begin with, his book was limited to fifty copies only, although that would not have been exceptional in times when it was customary to distribute one's work oneself through informal channels. Also, it contained some strange terminology, however again not to the extent of frightening the attentive and interested reader. Consequently, the major causes for the actual preference which was given to (new) analytic geometry, Wilder thinks, are rather to be found elsewhere, viz., in the general mathematical climate of the seventeenth century and the internal nature of Desargues's theory, as compared to prevailing standards.

As for the former component, Desargues' proposals could not readily be received within the existing conceptual framework, for this would have required the notion of 'free geometry' (i.e., unrestricted by Euclid). "Not until the nineteenth century was it possible even to *think* of a geometry founded on the concept of transformation and invariant (finally culminating in Klein's *Programm*)". Another cultural aspect is the lack of institutionalization. Desargues was not a teacher, could therefore not gather disciples, which was (is) not a *sine qua non* but surely of great help in disseminating and developing a theoretical framework.

As for the second dimension, its internal structure, despite having been the work of one isolated man only, the book nevertheless attracted the attention of some of the finest contemporary spirits, such as Fermat and Pascal. But everybody, up to and including Desargues himself, considered its capacities to remain fundamentally Euclidean in nature, thus *not* sowing the seeds of a new geometrical approach at all. As such, it did not pose a challenge to the existing tradition. Its inherent conceptual stress, which would lead into the concept of line at infinity, was not yet felt strong enough in order for the framework to be turned into a new tradition *outside* Euclidean geometry, viz., based on the concepts of transformation and invariant.

It might have survived, though, had it at the time been consolidated with the rest of mathematics (the way its successor, projective geometry, did consolidate with algebra and analysis in the nineteenth century, through the work of Monge and Poncelet, among others), or had concepts from other branches been injected into it. In conclusion, what in general characterizes the mathematical precursor/genius, for Wilder, is that (s)he is basically a loner coming up with terminological and conceptual novelties which the existing culture is not ready for, and consequently are not accepted and disseminated, or remain unnoticed altogether.

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