
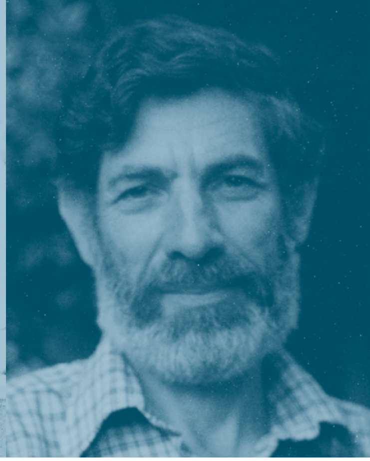


Bharath Sriraman Editor

Humanizing Mathematics and its Philosophy

Essays Celebrating the 90th Birthday of Reuben Hersh

$$\frac{dM}{ds}(s,t) = M(s,t)V(x(t))$$

$$u(t) = \int_{-\infty}^{\infty} \sum \hat{g}_j(t,s)\, T_A(s)\, u_j\, ds$$

$$\frac{\partial^2 u}{\partial t^2} = c \frac{\partial^2 u}{\partial x^2}$$

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Essays Celebrating the 90th Birthday of Reuben Hersh

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Preface – *Reuben Hersh: Humanizing Mathematics and Its Philosophy*

It is difficult to find words to succinctly capture a mathematician who has systematically deconstructed the cold edifice of the institution of mathematics. Reuben Hersh turns 90 on December 9, 2017, and his nine-decade journey to the present moment offers glimpses into the world of mathematics as well as the changing nature of the American landscape. Born in the Bronx to working-class immigrant parents, Reuben embraced the ideals of the "working class" in spite of graduating from Harvard at the age of 19. After a stint at *Scientific American*, Reuben spent the 1950s working as a machinist. When I asked Reuben about this, he said "Being so young and naive, I just felt frustrated at his (Svirsky's) constant dissatisfaction with my work. Then there were political reasons too. I was deluded into thinking that only the working class could save the world, so I ought to be part of the working class. Learning to run a lathe was interesting and in a way gratifying work" (see interview in "An Interview with Reuben Hersh").

One could say that becoming a machinist created a dual identity, namely, that of a "working man" as well as a "thinking man" and one that seems *different* with the way professions are structured today, particularly if one looks at the ivory tower of academia. However, being able to do many things was the hallmark of learned people for centuries. Gauss's "day job" was that of a surveyor; Euler worked as an engineer; von Helmholtz started out as a physician but went on to make astonishing contributions to mathematical physics.

Reuben went into mathematics after obtaining a degree in literature and then working as a machinist. He chose mathematics because he always enjoyed it. It should not be surprising then to know that he went on to complete a PhD under Peter Lax and had a fruitful career as a mathematician for many decades with research in partial differential equations, random evolutions, and operator equations. If one imagines these decades of his life as that of a "working mathematician" with literary leanings analogous to the "working machinist" with literary leanings, then these leanings came into full force when he started to write about the nature of mathematics, what it means to be a mathematician, the social nature of mathematics, the burden of proof, and what it means to question the status of mathematics. His first expository book *The Mathematical Experience*, cowritten with Phil Davis, won a National Book Award in Science in 1983. His subsequent book *What Is Mathematics, Really?* picked up where the Courant-Robbins classic ended, with a rhetorical question, but provided a more detailed exposition on what it means to be a mathematician, how mathematicians think about their work (as opposed to "science" or "craft" or "art"), and philosophical problems that arise when doing mathematics. Many of us can "do" things, i.e., do mathematics, do physics, do biology or do garden work, do woodwork, do cooking, etc., but very few of us are able to articulate what it really means to do something in a way that would appeal and interest a layperson. Reuben's expository books have been very impactful to many of us, and his position of mathematics as a human endeavor or humanistic allows the foot soldiers and the lay mathematicians a doorway through which they can examine their own mathematical endeavors. A well-known mathematician once told me that there are very few that can leap from one area of research mathematics to another and then be able to clearly articulate the connections that led them to make these "creative leaps." Reuben's journey as writer, machinist, mathematician, and philosopher over the nine decades of his life contains many such leaps, not simply within mathematics but between disciplines and not simply between disciplines but between completely different "working lives." His corpus of writings that range from technical mathematics to reviews to expository and philosophical writings offer clear glimpses of Reuben's many leaps.

I conferred with Reuben about putting together a Festschrift for his 90th birthday, and he agreed to it based on some conditions of course, namely, being able to shape this in order to break convention and be *different*! So he sent me a list of colleagues/friends/scholars whose work he has been influenced by, and I invited them to contribute to this book. When I asked Reuben what he would like contributors to address, he said the following:

> Forty years ago, Paul Cohen enraged me by predicting that (at some unspecified future time) mathematicians would be replaced by computers. So I now ask you, 1. Can practicing mathematicians, as such, contribute anything to philosophy of math? Can or should philosophers of math, as such, say anything to practicing mathematicians? 2. 20 or 50 years from now, what will be similar, and what will, or could, or should be altogether different: About the philosophy of math? About math education? About math research institutions? About data processing and scientific computing?

The colorful and eclectic essays in this Festschrift from numerous well-known mathematicians, philosophers, logicians, and linguists offer in part his colleagues' attempts at answering Reuben's questions and also in part glimpses into Reuben's fertile mind and his influence on many generations and decades of mathematical life. In his 90th year, he continues to produce mathematics and writings about it that is accessible to us all. Reuben Hersh epitomizes the phrase "humanist mathematician and philosopher," and I hope this Festschrift celebrates his many accomplishments and contributions to the field. I am deeply honored to be able to edit this collection and join the authors in this book to wish him a happy birthday, and I hope for another decade of contributions from Reuben.

Missoula, MT, USA Bharath Sriraman

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An Interview with Reuben Hersh

Bharath Sriraman

Question: Describe your formative experiences in school that resulted in you graduating from high school at the age of 15. Were there any teachers or books that influenced you?

When I was a kid in New York, it was customary to "skip" a kid in elementary school who was too far ahead of the rest of the class. I taught myself to read by watching as my Mom (Mildred, originally Malke) read to me; so in kindergarten I got a bad grade because I preferred reading to myself rather than playing with others. I got skipped twice in elementary school. Skipping 3rd grade put me into a class where they were already doing division. So I asked my Mom to explain it to me. She did.

Then in junior high school (now of course renamed middle school), they had a thing called RA,RB,RC,RD which did 7th, 8th, 9th grades in 2 years.

Then we moved from the Bronx to Mount Vernon, in Westchester County, right next to the Bronx, and so I went to Mt. Vernon High, which was called A.B. Davis High School. I was desperately eager to get away from my father (Phillip, originally Fishel); so I took extra classes plus went to summer school, got my diploma at age 14. I had absolutely no one to advise me what to do next; so I just stayed home another year, going to A.B. Davis High as a "postgraduate." By age 15, I was ready to flee.

Question: Did religion play a role in your childhood?

In a backwards way, my father had grown up in Poland with a Chassidic father, went to cheder every day from age 3, learned nothing but Torah. When he ran away from Poland to Palestine at age 18 to escape being drafted into Pilsudski's army, he realized that he had learned nothing of any use or value; so he became ever after hostile to all rabbis. The only Judaic aspect of my childhood was being kept home

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from school on Rosh Hashanah and Yom Kippur, because it wouldn't look nice for me or my sister to be in school. What we did on those days at home was nothing special, just whatever we pleased. So I was totally free of any belief in God, Heaven, an afterlife or immortality, angels, devils, commandments, Chosen People, or any other such nonsense and trifles.

Question: Which year did your parents emigrate to the USA? And did you grow up learning more than one language?

1920. Not really, picked up a little "kitchen Yiddish."

Question: What were your parent's views of education, a university education? Was education seen as a means of advancement?

They both had a religious home education in the old country, zero schooling here. It was always taken for granted that my sister and I would go to college.

Question: What led you to Harvard? And what led you to pursue literature in Harvard?

I decided to apply to three good colleges and one bad one, in case all three turned me down: Columbia, Yale, Harvard, and University of Southern California. Columbia and Yale scheduled interviews and naturally, after meeting me face to face, rejected me. Harvard and USC let me in; so I went to Harvard. I knew it was a snob school; so I went with a determination to maintain my Bronx accent.

I wanted to become a writer, to make a difference in the world of writing. There was no writing major; so lit was the closest thing. Math was fun, but not useful or, so far as I knew, important. I took beginning calculus, but David Widder, my teacher in 2nd year calculus, made it infinitely boring, without the slightest amount of motivation. So I didn't take any more math after that.

Question: Could you elaborate on what you mean by "and naturally, after meeting me face to face, rejected me?"

At that age I was totally asocial and unsocialized. It never occurred to me to give any thought to what impression I was making on anyone, least of all some middle-aged guy from Yale asking dumb questions.

Question: Was there any literary work or writing in particular that appealed to you during your undergraduate degree?

Blake's poetry

Question: What were the reasons for not pursuing graduate work immediately after Harvard?

I looked at a couple of journals on English lit. Not at all appealing! Felt a great need to get away from school. School was all I had known so far.

Question: What do you feel is extremely beautiful/beautiful? Please give some examples. Can you order these examples according to their aesthetic appeal on you?

Beethoven's music, the short poems of William Blake, many of the local sunsets out here

Question: What led you into becoming a machinist? Was it a skill you already had, or was it something you were interested in?

Why did I switch from editorial assistant at Scientific American to second-class machinist at various machine shops? Well, it was a combination of things. I only learned much later that my boss at Scientific American, Leon Svirsky, was impossible to satisfy and drove one of my successors there to psychotherapy. Being so young and naive, I just felt frustrated at his constant dissatisfaction with my work. Then there were political reasons too. I was deluded into thinking that only the working class could save the world; so I ought to be part of the working class. Learning to run a lathe was interesting and in a way gratifying work.

Question: You mentioned your sister earlier. Did she also go to college and was she grade skipped like you? And what did she end up studying? I am asking this as women were not encouraged at that time period to go into higher education.

Deena got a degree at University of Wisconsin, I don't know her major, but it was not science, maybe psych. Something human, I am sure. She married a very aggressive ambitious law librarian who became very big at University of Texas law school and had three daughters with him. Then in her forties went to grad school in social work and spent years at UT as coordinator of practical work of social work grad students. She is smart and is interested in a lot of things but never was drawn to math.

Question: You were in college when WWII was raging. Do you remember what civilian life was like during this time, particularly in terms of the stories/media that filtered through to the common man?

I was an undergrad at Harvard. The Harvard Crimson had been discontinued, replaced by something called the Service News (Harvard was hosting a Navy V12 program). The schedule had been made a three-semester per year. I was active in the Harvard Liberal Union, supporting the fight against fascism and the liberalism of FDR. I even canvassed in Cambridge for him in 1944, although still only 16 years old. Some Harvard students started a postwar union, to be ahead of the game in planning the postwar world.

Question: And what were these plans for the postwar world? Do you see a return to Fascism in the way extreme right-wing governments are increasingly being considered by the electorate and the alt-right movement in the USA? There is even a webpage where "leftist" professors are listed—harking back to the lists of the McCarthy era. What do you think of this trend in higher education?

I never paid attention to any of that. All that mattered was defeating Hitler!!!!!

The past does not repeat. It is not past, of course; it is still here inside us. But it does not return; Hitler will not rise out of the grave. We have to pay attention to what is really going on here and now, not to a fear fantasy of the past returning. The NY Times the other day had an article telling the history of such lists going back to the 1930s.

Question: Why did you pursue mathematics as a graduate degree?

At age 29 with a family to support and scared to continue risking any more fingers at machine shop work, I was at a loss for months after losing half my right thumb to a band saw. Then it came back to me; I used to like math! Maybe that could be a way to earn a living.

Question: Was it difficult to pick up academic work (i.e., being a student) after a time lag from your undergraduate work?

No, it turned out to be second nature.

Question: Describe the academic culture (as seen by you through a student's eyes) at the Courant Institute?

There were insiders and outsiders. Insiders were buddying with faculty, making themselves at home in the building. Outsiders were taking just one or two courses while working full time at companies in the New York area. I started as an outsider, an overage under-educated nervous newcomer. After I was hired as a research assistant, I turned into an insider who still was aware of the greater number of outsiders, who of course resented the privileged insiders. Much later I was fascinated to read, in a memoir by Courant of the advent of Nazism at Gottingen, how surprised faculty members were at all these little known outsider students who showed up as Nazis.

Question: Did you find Peter Lax, or did Peter Lax "find" you?

I was taking his real variables course. Hadn't given any thought at all to a thesis or an adviser. Tried to sit in the back of the classroom and avoid being noticed. However, he just walked up to me and asked if I'd like to be his student. I assume my homework must have been above average.

Question: How did you arrive at your dissertation topic? Did Peter assign you a problem, or was it through working together? And what was the topic of your dissertation?

He showed me a list of problems. After giving up on two of them, I tried to find the most general correct boundary condition for a first-order hyperbolic constant coefficient system of PDEs in a half-space. You can read more in my bio of Peter. The simple idea of taking the Laplace transform made it quite straightforward, and, in fact, I got free of charge any well-posed system, not necessarily hyperbolic, not just first order!!! That is what I mean by looking at it sideways instead of forward, i.e., turning it into just a space problem, getting rid of the time variable.

Question: Can you describe a learning or a teaching moment when you experienced a sense of beauty for the subject matter? If so did it result in pursuit of research (in other words in a creative process)?

When I realized that that the method I used in my thesis, for hyperbolic problems, was equally applicable to all well-posed initial value half-space problems, without restriction as to type. Also I realized that Kac's example of a Poisson process controlling a particle sliding on the x-axis was really about any abstract group switching direction at random. Both ideas led to years of work on many examples and applications.

Question: Did you ever consider doing a PhD in pure mathematics instead of applied mathematics?

In fact, I have always been a pure mathematician, even though working in an area of pure mathematics that is "applicable." PDE is a branch of analysis. ODE is also a branch of analysis, often nowadays called "dynamical Systems." It is the simplest subarea of PDE.

Question: Is there a pecking order in the world of mathematics/mathematicians?

Sure. Only, not a well-ordering, it's a partial order.

Question: Is arrogance inversely proportional to the level of achievement in the cases of eminent mathematicians [the movers and shakers of the field]?

No.

Question: To rephrase the previous question, are eminent mathematicians more likely to be humble? Is there a cultural component to this question?

I can't deal with likelihoods. It's a matter of personalities.

Question: You've met a veritable who's who of mathematicians in the course of your career—can you recall any personalities?

Fritz John was waiting at a plenary meeting of the AMS to deliver a named lecture. I modestly approached him and asked him something friendly. He answered something like he'd rather be any other place in the world. Terribly nervous.

George Polya visited UNM to give a math talk, volunteered also to give a talk on math ed. Very funny. We took him to a Mexican restaurant for lunch, and he ordered chile relleno. My wife warned him not to eat the seeds, but he just said Hungarians are used to spicy food. We saw his eyes fill with tears and his cheeks turn red. But he managed to smile and say it was good.

Mark Kac spoke at a meeting I chaired in Edmonton. Low-priced housing was available on campus; all the other participants were there, but he just looked and asked what else we had. After trying a fancier place on campus, we went into town to a commercial hotel; they had some kind of bridal suite or presidential suite. That was what suited him. (People who knew him had warned me he wouldn't stay in the student housing.) Speakers were required to submit a ms for publication, but he wouldn't. I and another participant had to write his article for him, based on a videotape and his scrappy lecture notes. Yet he was very friendly and likable, just not ashamed or embarrassed at being treated like a big shot.

I found Hormander polite and courteous, but my Swedish friend told me that he was quite otherwise to ordinary Swedes.

Paul Cohen was very competitive and combative. That worked if you were strong and self-confident enough to stand up to him.

Erdos and Lax were the friendliest gentlemen, Varadhan as well.

When I first met Stan Ulam, I tactlessly mentioned having seen him called Father of the H bomb. He stuck out his chest, looked up at the ceiling, and blurted out, “But it’s true!”.

Einar Hille was always happy to take whatever we handed him and insert it into the proceedings of the national academies of science.

David Mumford free of any pretensions at all

Richard Courant thought it hilariously funny that I, his copy editing assistant, presumed to advise him on rewriting. He started out his lecture course by whispering faintly that if anyone had difficulty hearing him please speak up; he then went on to whisper about generalized functions; no one spoke up.

Rota was an amazing blend of totally natural and straightforward and totally hidden and masked.

Question: Was anti-Semitism prevalent in the institution of mathematics you experienced?

My only academic experience of anti-Semitism was overhearing a conversation between John Searle, a big shot Berkeley philosopher, and a local English professor at UNM.

Question: What led you to develop a humanist perspective to the philosophy of mathematics?

As I explain in the intro to What is Math, Really? it all started by teaching an undergrad course in Foundations of Math and discovering that there were three standard philosophies, all of which were obviously wrong, but I had no idea what my own philosophy was.

So it seemed natural and obvious to look carefully at what I and my friends, collaborators, and admired exemplars were actually doing and saying they were doing and basing my understanding of what math is on this information. (Not on before the fact philosophical requirements!!!)

Question: Suppose a Fields Medalist peer says: I don’t actually know what “philosophy of math” means nor do I think I know any “philosophers of math” (maybe you could name a few leaders of this “field”?). How would you answer this question?

Alphabetically,

Byers, Cellucci, Dehaene, Friend, Grosholz, Hacking, Kitcher

Question: What is the difference between practicing mathematics and philosophizing about practicing mathematics?

Same as the difference between playing the piano and writing music crit

Question: Does your response implicitly suggest that sometimes the latter cannot do the former? In other words, is the former a prerequisite to being the latter?

Critics are important. There have been many great literary critics who did not produce much poetry or fiction themselves. The pianist can learn something from a good music critic.

Question: Why do you reject the Platonist view of mathematics?

Because I think there is only one universe. I don't believe in a spiritual realm separate or independent of material reality.

Question: One often hears that history of mathematics should be relegated to the category of "popularizing" mathematics. In other words history of mathematics is not serious mathematics but more a Segway into nostalgia—what do you think of this view?

Nonsense. History is a deep scholarly endeavor, not mere nostalgia. No, it is not serious math; it is history of serious math.

Question: I like this response. My question stemmed from the fact that I have met some mathematicians who when told the history of a "serious" dispute on a problem in their field simply blow it away as being trivial. Are there any contemporary "disputes" in any area of mathematics that you are aware of that might eventually make it into history of math books?

A serious dispute can seem trivial if a new discovery makes it obsolete. Digging a deep hole may be made obsolete if someone finds a shortcut sideways into the hill. There were disputes about the right definition of the integral; now they don't seem interesting or important anymore. Voevodsky's new foundations are controversial; he is bringing back constructivism in an even more complete and detailed way but purely as a practical method of formalizing proof, without the philosophical crusading of Brouwer or Bishop.

Question: Are there different styles of doing mathematics, with some being more instructive (or transferable to teaching students)? This question stems from the fact that a lot of literature reports on the schism between how mathematicians practice mathematics versus how they teach mathematics.

Yes, the dogmatic formal-logical style and the heuristic, problem-solving style like Polya. The latter is much better for students and teaching.

Question: What is your opinion of the style of experimental mathematics promoted by the Borweins?

Very impressive. An incredible body of fascinating discovery

Question: Can you elaborate? I always thought they should receive more popular press, but for some reason this has not happened. What do you think is the reason for this?

Borwein never looked for headlines or controversies. While he produced amazing heuristic discoveries, he continued to demand deductive proof. He was really conservative philosophically while daring and audacious in computing.

Question: Tell us about your collaboration with P. Davis on The Mathematical Experience. How was the experience of co-writing?

I had been blocked for years, struggling to get beyond just making notes in notebooks. Phil actually offered to listen to me talk and then write it up! I was astounded. We enjoyed very much talking math and finding out how much we agreed. I went to Brown on sabbatical, with the plan of writing philosophy of math while he worked on a different project, popularization and exposition. I produced

100 pages of handwritten ms and then had a breakdown, couldn't continue. Handed it over to him and Hadassah to do with whatever they could.

Question: Have your views on the nature of mathematics changed in the time period after The Mathematical Experience?

They have developed very considerably, as you can see from "What Is Math, Really?" and "Experiencing Math."

Question: Dirac wrote about Schrödinger and himself: "It was a sort of act of faith with us that any questions which describe fundamental laws of nature must have great mathematical beauty in them." Can you agree with the similar statement: "It was a sort of act of faith in my work that any fundamental mathematical truth must have great beauty in it."

I would put it in terms of what the researcher is motivated to commit to. We humans are motivated and affected by whether something is beautiful or ugly. In physics, that might be translated into a belief about nature herself. I think it is a property of the human mind that is being projected onto nature. The quadratic formula is an ugly thing that everybody has to know; it's pretty fundamental (example from Morris Kline). Hardy and Halmos called applied math ugly; Peter Lax answered that in my biography of him. Topologists and algebraists used to call PDE ugly. Then when solitons popped up, everybody said, "OH, beautiful." It's common for the first proof of a tough theorem to be pretty mean and unattractive, and later on people try and may succeed in beautifying it.

I think the Erdos-Selberg proof of the prime number theorem is not claimed to be a joy forever.

Question: The institution of mathematics is often portrayed in a lot of "critical education literature" as being a racist, gendered, privileged, patriarchal subject matter. An inference of this anthropomorphizing is mathematics being blamed for societal inequalities and viewed as promoting the "industrial military" agenda. What are your views on this?

I didn't have the impression that this kind of talk was still going on; I thought it came and went decades ago.

Question: Why is mathematics not taught as an elective like art?

It is, in courses derisively called Math for Poets, as another way to fulfill a math requirement without repeating high school.

Question: What I meant is, why should mathematics be a requirement in high school, when say physics or biology is not?

Kids won't take it if they think they don't need it; then they find out it's required for the career they want and they have to learn high school math in college. It's too bad people are so dumb sometimes you have to force them to do something for their own good, like wearing helmets on a bike.

Question: Did you experience some mathematical objects as beautiful? Which ones? Can you compare those feelings of mathematical beauty with the aesthetic appeal of the examples you gave above?

My own quasi-new proof of the Heron area formula and of the Faulhaber polynomial identity that I included in a couple of philosophical places and of the two simple ideas (bring in the Laplace transform and replace d/dx by any group generator) which generated most of my research career.

But you don't mean just in my own work but more generally. Certainly the complex integral of an analytic function—Cauchy's integral theorem and formula—would be at the top of the list and the five platonic solids and most of what's in Courant-Robbins.

Question: Does success/failure in a creative problem-solving process influence the former aesthetic appeal of the problem? Does success enhance the appeal of the problem the way that it leads you to other related problems?

I think my previous answers cover this. Look at the twin prime problem. Recently there was huge excitement when an unknown researcher using known ideas was able to prove the analogous statement where "2" is replaced by some huge integer in the hundreds of thousands or millions. No beauty there, sheer tough slogging where everybody else had not tried that hard

Question: Is mathematics objective?

It is objective from the viewpoint of the person who must learn it or apply it. It is however internal to culture and society; so in that global sense of humanity as a whole, it can be called subjective. Its objectivity is in its transpersonal intersubjective societal status.

Question: Some mathematicians insist on paper-pencil mathematics being the real core of doing mathematics.

That's silly, typing on a computer is much better.

Question: I meant paper-pencil mathematics as that which does not rely on computers to generate a proof.

I have trouble understanding the concept or term "real core of math" well enough to argue about it. I am familiar with the critique of the computer proof of the four-color theorem, and I suppose the critique of other computer-assisted proofs is not entirely different from that old one.

Halmos said "we learn nothing from it."

Others said computers can make mistakes.

But any mathematical result will be interesting to those who are interested in it, not so interesting to those uninterested in it. What else is new? As with anything else, you are free to take it or leave it.

If the question is whether the four-color theorem should be considered "proved," i.e., accepted as established and available for use in future proofs, that was a decision that was available for the mathematical community to make, and it seems that it has been accepted as established. It is true that there is some variation in the credibility or certainty of established theorems. Some are rock solid, others not so much. So there probably still is disagreement on whether the four-color theorem is just as solid as this or that comparable theorem, nothing disturbing or worrisome about that.

I consider the work of Jon Borwein exemplary. The work of Voevodsky et al. is in progress; it will be seen whether it transforms math or becomes one more sidestream or special topic.

In general I don't support judgmental remarks about any kind of mathematical work.

Some don't consider numerical analysis to be real math. Some don't consider set theory in the highest most remote ranges of hyperinfinity to be real math.

Let everybody do what they like, and as time goes on, it will get sorted out. Meanwhile, be liberal in what can be published and/or taught.

Question: Can this view be challenged with new media and increasingly different ways of accessing mathematics through the internet?

Of course

Question: What do you see as the future of university mathematics, particularly mathematics departments that justify their existence by doing "service courses"?

Vestigial organs and activities can persist for centuries. Look at Latin as the language of scholarship or penmanship as required in my elementary school days.

Question: In your tenure as a teacher at UNM, did you think that courses became watered down over time due to the changing background of entering undergraduates?

I wouldn't say watered down. There were changes meant to take into account the needs and skills of actual students.

Question: Why would you suggest mathematics as a field of study to someone today?

Same as poetry or dance, only go into it if you simply can't help it.

Question: Will pure mathematics eventually become obsolete?

No. It is too much fun.

Nine Decades

Bharath Sriraman

A Photographic Passage

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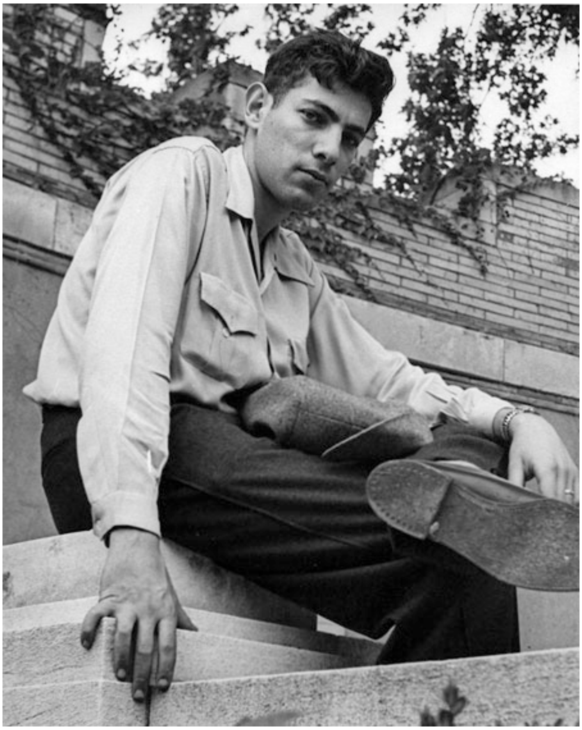
As a child with Mildred (mother), Phillip (father), and Deena (sister)

Reuben and Deena (early 1930s)

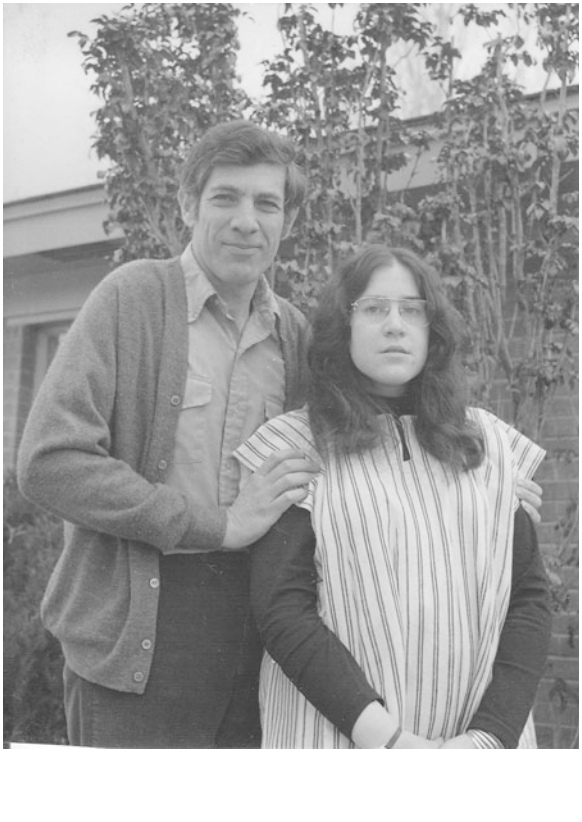
In a New York park while employed at *Scientific American* (early 1950s)



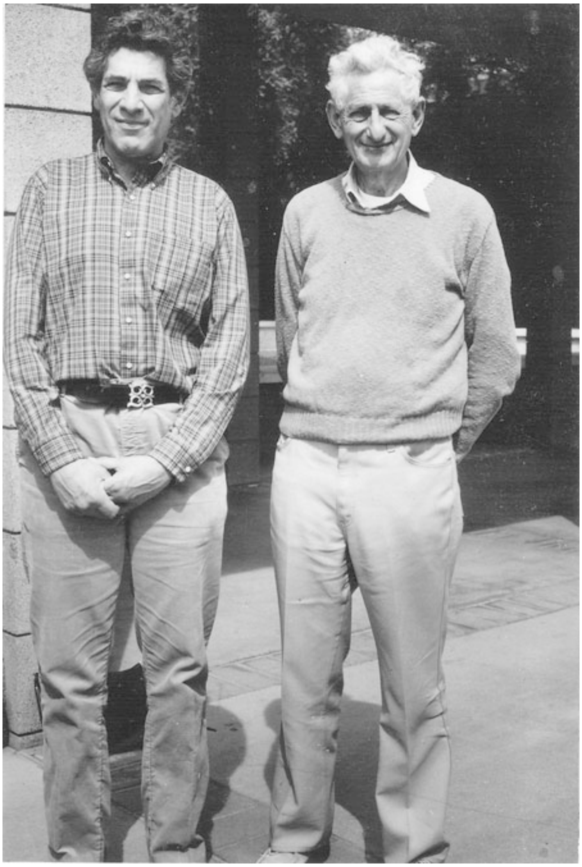
Playing chopstick with two fingers, in the Cape Cod Cabin of Chandler Davis's family (around 1950)

With children: Daniel and Eva (1961)

With Eva (1970s)



With Phil Davis



With all his descendants: Daniel and Eva, grandchildren David and JJ (1998)

With Mildred (1998)

With JJ (2005)

Wall of remembrance at Jewish Cemetery in Santa Fe (2016) (Reuben is standing next to two blocks he had put in a wall of remembrance in honor of his mother’s parents and his father’s extended family, all of whom died in the Holocaust.)

Reuben and Vera, at the cemetery (2016)

Pluralism as Modeling and as Confusion

Reuben Hersh

We consider three levels of pluralism: (1) within mathematics, (2) with regard to philosophy of mathematics, and (3) personal pluralism.

(1) Pluralism means peaceful coexistence, of several contradictory theories. Within mathematics, such coexistence is taken for granted. A normal probability distribution contradicts a uniform probability distribution. They are "parallel" concepts, a simple example of peaceful coexistence, of pluralism. Then there are the L_p spaces of functional analysis. If $p = 2$, we have Hilbert space. For p greater or equal to 1 but not equal to 2, we have Banach space. For two different values of p, we don't perceive a contradiction but rather a choice. That's mathematics. Pluralistic!

Most familiar is the coexistence of Euclidean and non-Euclidean geometry. Euclidean geometry accepts Euclid's parallel postulate; non-Euclidean negates it. No problem! They are just two separate theories. Pick the one you prefer at present. This peaceful contradiction within mathematics has never interfered with anybody's keynote speech praising the unity of mathematics.

If you have read a bit of Riemannian geometry, you know how to look at the two geometries as two surfaces, one with constant curvature zero and one with constant negative curvature. But even without such a formal reconciliation, we know how to keep them in two separate boxes, like the hammer and screwdriver that sit side by side in my tool shed, along with some nails and some wood screws. They are

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contradictory but also entirely compatible. A contradiction arises if I foolishly try to hammer in a screw or to screw in a nail.

(2) *Although in mathematics complete consensus is the norm, in philosophy of mathematics, I have been told, it's just the opposite way; you never convince anybody to give up their position.*

Philosophers of mathematics commonly present themselves as choosing and defending a "position," as if on a battlefield. I quote Stewart Shapiro's *Oxford Handbook of Philosophy of Mathematics and Logic* as a respected representative.

"I now present sketches of some main positions in the philosophy of mathematics," he writes. Six positions are listed in the table of contents. Five of them get two chapters, pro and con. Between chapters expounding logicism, intuitionism, naturalism, nominalism, and structuralism, are chapters reconsidering structuralism, nominalism, naturalism, intuitionism, and logicism.

"One of these chapters is sympathetic to at least one variation on the view in question, and the other 'reconsiders'." Formalism gets only one chapter, evidently it didn't need to be reconsidered.

"A survey of the recent literature shows that there is no consensus on the logical connections between the two realist theses or their negations. Each of the four possible positions is articulated and defended by established philosophers of mathematics."

The practice of philosophy of mathematics seems to be to a large extent choosing a position and fighting for it. The fighting, or arguing, is what philosophy of mathematics is all about. There is no question of convincing anyone to change positions. And there is no need to get interested in actual mathematics, or the opinions of actual mathematicians. The game is all about arguing with your fellow philosophers of mathematics, each with his or her position to defend and advocate. (Thanks to Brendan Larvor for pointing out that some authors are nuanced and recognize some merit in the position of an adversary.)

So the proposal of peaceful coexistence, pluralism, in philosophy of mathematics, is a radical new idea and, in my opinion, a great idea. I accept and support pluralism as a metaphilosophy. In fact, this paper proposes a rationale and justification for pluralism. To do so, I want to take a higher or more inclusive standpoint than merely that of metaphilosophy—overlooking or surveying various philosophical "positions."

The scholarly or scientific study of any phenomenon, whether physical, biological, or social, implicitly or explicitly uses a model of that phenomenon. A physicist studying heat conduction models heat conduction as a fluid flow, or as propagation of kinetic energy of molecules, or as a relativistic or quantum mechanical action. Different models serve different purposes. Setting up a model involves focusing on features of the phenomenon that are compatible with the methodology being proposed and neglecting features that are not compatible with it.

By talking of a model rather than a theory, one acknowledges that the phenomenon could also be studied other ways. One's model merits consideration if it provides an insight that isn't better provided by some other model.

Philosophers taking a "position" of formalism, structuralism, or logicism, for example, are advocating different models of mathematics, which is a social-historical phenomenon subject to empirical study, both in its content as well as in its human interactions. Mathematical knowledge and activity are observable phenomena, already present in the world before philosophers proceed to study them.

In philosophy of mathematics, mathematics is the thing being modeled, even though much of mathematics already is a model of a physical action. Arithmetic, for instance, models the action of counting.

Philosophy of mathematics, when studying the "positions" of formalism, constructivism, platonism, and so on, is studying models of mathematics, It studies second-order models! (Other critical fields like literary and art criticism are also studying models of models.) Being a study of second-order models, philosophy of mathematics constitutes still a higher order of modeling—a third-order model.

Let's look separately at the math-studying disciplines and their models.

Mathematical activity (in contrast to mathematical content) is modeled by neuroscience, by logic, by history of mathematics, by psychology of mathematics, by anthropology, and by sociology. These use verbal modeling for phenomena that are not quantifiable—the familiar psychological and interpersonal variables of daily life, including mathematical life.

History, logic, neuroscience, psychology, and other sciences offer different models of mathematics, each focusing on aspects accessible to its method of investigation. Different studies of mathematical life overlap, and they have interconnections, but, still, each works to its own special standards and criteria. Historians are historians first of all and likewise educators, neuroscientists, and so on. Each special field studying math has its own model of mathematics.

Philosophy of mathematics of course is an important, respected kind of theorizing about mathematics. So is logic, which is a distinct specialty allied to philosophy of mathematics. So is history of mathematics. So are computing and computer science. So is the new field of cognitive neuroscience. Also of great interest are anthropology, psychology, and sociology of mathematics.

The positions actually are complementary models.

If we set philosophy of mathematics among the whole group of studies of mathematics, both humanistic and scientific, we can see the significance of formalism, logicism, nominalism, structuralism, naturalism, and even social constructivism, in a new light. Each of them is in fact a model of mathematics. In that way, each is legitimate, and none prevents another from carrying on its work.

I am just a mathematician, but I dare to hope that some philosophers will be tempted to give up the customary back and forth in favor of peaceful coexistence.

Pluralism would be a blessing. However, it is not a position *in* the philosophy of mathematics, for it says nothing about mathematics itself, neither its ontology, its epistemology, nor its practice. It is a "position" in a loftier field, which should be called the metaphilosophy of mathematics. Pluralism is a position about the philosophy of mathematics. It is a philosophy of the philosophy of mathematics.

"Taking a position" on the nature of mathematics looks very much like the vice of essentialism—claiming that some description of a phenomenon captures what that phenomenon "really is" and then trying to force observations of that phenomenon to fit into that claimed essence. Rival essentialisms can argue for a very long time; there is no way either can force the other to capitulate.

Such is the story of mathematical Platonism and mathematical anti-Platonism. Mark Balaguer has even published a book proving that neither of those two can ever be proven or unproven. "He concludes by arguing that it is not simply that we do not currently have any good arguments for or against Platonism but that we could never have such an argument." Balaguer's conclusion is correct. It is impossible in principle to prove *any* model of *any* phenomenon, for the phenomenon itself is prior to, independent of, our formalization and cannot be regarded as or reduced to a term in a formal argument.

The model of mathematics as a formal axiomatic structure is an immense success. It is a branch of mathematics while simultaneously being a model of mathematics, so it possesses a fascinating and bewildering reflexivity. Enjoying these benefits doesn't require one to be a formalist—to claim that mathematics *is* an axiomatic structure in a formal language. Bill Thurston testified to the confusion and disorientation which that formalist claim causes to beginners in mathematical research.

Frege's logicism expelled psychologism and historicism from respectable philosophy of mathematics. Nevertheless, it is undeniable that mathematics is a historical entity and that mathematical work and activity are mental work and activity. Its history and its psychology are essential features of mathematics. We cannot hope to understand mathematical activity while forbidding attention to the mathematician's mind.

As ideologies, historicism or psychologism is one sided and incomplete, as was logicism's reduction of mathematics to logic. We value and admire logic without succumbing to logicism. We can see the need for the history of mathematics and the psychology of mathematics, without committing historicism or psychologism.

Another prominent position is "structuralism." Already in the 1930's the leading French mathematicians "Bourbaki" used structure of structures as a model for mathematics. It is easy to see both the merits and the defects of such a model. Pursuing the merits has paid off, in benefiting research. This is a different matter from *being* a structuralist—taking the position that mathematics *is* structure. A chapter in Burgess (2015) contains a helpful analysis and critique of structuralism.

Burgess and Rosen (1997) surveyed nominalist models of mathematics and found them instructive, even though, they concluded, nominalism is not tenable as a philosophical position.

A natural and appealing philosophical tendency for modeling mathematics is phenomenology. The phenomenological investigations of Merleau-Ponty looked at outer perception, especially vision. A phenomenological approach to mathematical behavior would try to capture an inner perception, the mathematician's encounter with her own mathematical entity.

If we looked at these theories as models rather than as theories, it would hardly be necessary to argue that one of them falls short of capturing all the major properties of mathematics, for no model of any empirical phenomenon can claim to do that. The test for models is not whether they are complete or final but whether they are useful or illuminating.

Different models are both competitive and complementary. Their standing will depend on their benefits in practice. If philosophy of mathematics was seen as modeling rather than as taking positions, it might consider paying attention to mathematics research and mathematics teaching as testing grounds for its models.

Can we imagine these rival schools settling for the status of alternative models, each dealing with its own part of the phenomenon of interest and each aspiring to offer some insight and understanding? The structuralist, Platonist, and nominalist could accept that in the content of mathematics, even more than in heat conduction or electric currents, no single model is complete. Progress would be facilitated by encouraging each in his own contribution, noticing how different models overlap and connect, and proposing when a new model may be needed. A modeling paradigm would substitute competition for conflict. One philosophical modeler would allow the other modeler his or her model. By their fruits would they be judged.

Philosophy of mathematics constructs models, comparable to those of other math-studying fields.

Just as philosophy of mathematics attempts to describe mathematics using methods of philosophy, so do the history of mathematics and computer science and cognitive neuroscience. Each one offers its own model of mathematics. Philosophical "positions" are also models. These different models of the same empirical phenomenon are logically contradictory but scientifically complementary.

The argument between fictionalists, Platonists, and structuralists seems to suppose that some such theory could be or should be the actual truth. But mathematics is too complex, varied, and elaborate to be encompassed in any model. An all-inclusive model would be like the map in the famous story by Borges—perfect and inclusive because it was identical to the territory it was mapping.

Formalists, logicists, constructivists, and so on can each try to provide understanding without discrediting each other, any more than the continuum model of fluids contradicts or interferes with the kinetic model.

If a philosopher of mathematics regarded his preferred "position" as a model rather than a theory, he might coexist and interact more easily. Structuralism, intuitionism, naturalism, nominalism-fictionalism, and realism-Platonism each have strengths and weaknesses as a model for mathematics.

Pluralism in philosophy of mathematics would follow as a consequence of viewing philosophical positions on mathematics as models of mathematics. There would be peaceful coexistence, rather than conflict and opposition. Different models would supplement each other, even fit together, as kinetic theory and continuum theory fit together in the physics of fluids. It would be okay for the Harvard Philosopher yesterday a Quine follower to be today a pragmatist. No longer would the charge of "Chameleon!" be thrown.

Research into mathematical behavior from the logical, the historical, the cognitive and neuroscientific, the anthropological, the psychological, and the sociological perspectives or viewpoints all have yielded and to this day are yielding interesting, important insights about the nature of mathematics.

The far greater part of ongoing mathematical activity is schooling. Teachers and educators must be included in any future comprehensive mathematics studies. They observe a lot and have a lot to say about it. Paul Ernest, a theorist of math education, in his book *Social Constructivism as a Philosophy of Mathematics*, follows Lakatos and Wittgenstein in building his social constructivist model.

Mathematics education has urgent questions to answer. What should be the goals of math education? What methods could be more effective than the present disastrously flawed ones? Mathematics educators carry on research to answer these questions. Their efforts would be greatly facilitated by a well-established overall study of the nature of mathematics.

Among existing models of mathematics, the giant branch of applied logic called formalized mathematics stands out. Being at once a model of mathematics and a branch of mathematics, it has a fascinating self-reflexivity. Its famous achievements are at the height of mathematical depth, settling Hilbert's first and tenth problems and providing tools for mathematics like nonstandard analysis. Proudly and justifiably, it excludes the psychological, the historical, the personal, the contingent, or the transitory aspects of mathematics.

Related to but distinct from theoretical formalizations is the recent actual formalization of mathematical proof in actual code running on an actual machine. Such programs come close to guaranteeing that a proof is complete and correct.

Logic sees mathematics as a collection of virtual inscriptions—declarative sentences that could in principle be written down. On the basis of that vision, it offers a model: formal deductions from formal axioms to formal conclusions—formalized mathematics. Sol Feferman constructed a refined logic model of mathematics: the smallest system of logic big enough to support classical mathematics.

Mathematical logic is a branch of mathematics, and whatever it's saying about mathematics, it is saying about itself—self-reference. This powerful model does not have to resemble what real mathematicians really do. That project can be left to others. The logician's view of mathematics can be briefly stated (perhaps oversimplified) as "a branch of applied logic."

The competition between category theory and set theory, for the position of "foundation," is a competition within logic, for two alternative logical foundations. They provide us ordinary working mathematicians with two alternative models, either of which we may choose, as what seems best for our purpose.

The work of neuroscientists like Stanislas Dehaene models mathematics as an activity of the nervous system. It looks at electrochemical processes in the nervous system of the mathematician and there seeks for correlates of her mathematical process. As localization in the brain becomes more accurate, it may become possible to observe specific brain processes synchronized with conscious mathematical thought. Already, Jean-Pierre Changeux argues that mathematics is nothing but a brain process.

The neuroscientist's model of mathematics can be summarized (a bit oversimplified) as "a certain kind of activity of the brain, the sense organs and sensory nerves."

History of mathematics is done by mathematicians as well as historians. History models mathematics as a segment of the ongoing story of human culture Mathematicians ask, "Was it important? natural? deep? surprising? elegant?" The historian sees mathematics as interwoven with finance and technology and with war and peace. Today's mathematics culminates all that has happened before, yet in time it will be viewed as a brief, outmoded stage of the past.

One natural model for mathematics is as story or narrative. Robert Thomas suggested such a model. (Thomas has also suggested litigation and playing a game as models for mathematical activity.) The collection *Circles Disturbed* contains various attempts to use narrative as a model of mathematics. Thinking of mathematical proofs as a kind of story has both obvious merits and defects. Pursuing its merits might have payoffs in research, or in teaching. That would be different from being a fictionalist—taking the position that mathematics is fiction.

The collaboration between philosopher Mark Johnson and linguist George Lakoff is exemplary. *Where Mathematics Comes From*, by Lakoff and Rafael Nunez, is a major contribution to our understanding of the nature of mathematics.

Each of these fields has its particular definition of mathematics. Rival definitions could provoke disagreement, even conflict. Disagreement and conflict are sometimes fruitful or instructive, but often they are unproductive and futile.

Philosophers also propose models of mathematics, although without situating their work in the context of modeling. Some of these models combine philosophy of mathematics and history of mathematics. Imre Lakatos wrote that without philosophy history is lame and without history, philosophy is blind. Or maybe it was the other way around. Lakatos' *Proofs and Refutations* presents a classroom drama about the Descartes-Euler formula. The problem is to find the correct definition of "polyhedron," to make the Descartes-Euler formula applicable. The successive refinement by examples and counter-examples is implicitly being suggested as a model for mathematical research in general. Lakatos argued that his rational reconstruction was more instructive than history itself! This is amusing or outrageous, depending on how seriously you take these matters. It is a clear example of violating the zeroth law of modeling, which never confused or identified the model with the phenomenon!

Some other important philosophers of mathematics who joined philosophical and historical analysis include Philip Kitcher, who identifies five driving forces that generate new mathematical entities; Emily Grosholz, who focuses on "ampliative" moves in mathematical research; and Carlo Cellucci, who argues that plausible reasoning rather than deductive reasoning is the essential mathematical activity.

Logic models mathematics as a web of formal inscriptions. Neuroscience models it as electrochemical activity in the nervous system. History models it as part of the ongoing evolving fabric of human society (including science, technology, war, government, and education). None of them is wrong. None of them is complete. Actual mathematics or mathematical life, like any other major aspect of humankind, is too varied, complex, ever evolving, and enriching itself, to be captured by any single theoretical framework or model. It is never necessary to argue that any model is incomplete or partial, because it is in the very nature of modeling to be incomplete and partial, just as any map of any region of the Earth is incomplete as a description of that region. It is the very purpose of a model or a map to be incomplete, to select certain features of its subject to represent and theorize. A complete description would be just a duplicate of the original and therefore useless.

Models are naturally pluralistic.

What do we gain by seeing formalism, logicism, etc. as models of actual mathematics (what people do when they do mathematics)? We see immediately that they are pluralistic and they naturally and automatically do fall under a pluralistic metaphilosophy.

Study of any complex empirical phenomenon, whether physical, biological, or social, ordinarily is done by a variety of tools, models, or methodologies, which supplement each other, according to the particular aspect of that phenomenon appropriate for that model or methodology. There is ordinarily a mutual tolerance or coexistence of models, which can rightly be called "pluralism." Classical Newtonian physics coexists with quantum mechanics and general relativity; the science of cosmology disposes of whichever model works best for the problem or phenomenon under consideration.

Mathematics, a universal attribute of our species, is modeled separately by logicians, historians, neuroscientists, and others. These models could be integrated into "mathematics studies," a coherent, many-faceted branch of empirical science. Philosophers could facilitate that unification. Their competing "positions" on the nature of mathematics would more productively serve as models of mathematics. This reconception of the philosophy of mathematics would render it a pluralist enterprise.

(3) *Personal pluralism*

In addition to the normality of pluralism in mathematics itself and the desirability of pluralism in philosophy of mathematics, I must also confront pluralism of the ordinary everyday mathematician, the unphilosophical one who is so uninterested in philosophy that he feels no need to know his own philosophical views. (I am dropping the tedious he/she shibboleth.)

It was somewhat shocking that Bourbaki himself (the mask donned by a super prestigious quasi-Parisian clique that came to epitomize fashionable abstraction) was self-avowed to be such a one. Jean Dieudonne, the scribe and captain of the group, wrote: "On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say, 'Mathematics is just a combination of meaningless symbols' and then we bring out Chapter 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. This sensation is probably an illusion, but it is very convenient. That is Bourbaki's attitude toward foundations."

As if to confirm Dieudonne's report, Paul Cohen, an American famed for contributions to fields spurned by Bourbaki (set theory and Fourier analysis), wrote: “The Realist position is probably the one which most mathematicians would prefer to take. It is not until he becomes aware of some of the difficulties in set theory that he would even begin to question it. If these difficulties particularly upset him, he will rush to the shelter of formalism, while his normal position will be somewhere between the two, trying to enjoy the best of two worlds.”

I summarized these descriptions: “The typical mathematician is a Platonist during the week and a formalist on Sundays.” I took this philosophical opportunism or insincerity to be undesirable, even unacceptable. But now I see a way to make it okay. Just call it pluralism! (Or perhaps “dualism,” since the number of alternative philosophies seems to be only two.) William Byers has introduced ambiguity as an essential aspect of mathematics, a driving force that leads to the creation of new mathematics.

However, back in 1979, this simple way out did not occur to me. Rather than just label the contradictory notions “plural” to make the inconsistency okay, I offered my own way out: accept our mental models of mathematical entities as real! They actually exist and have properties. This is “realism,” but not Platonic realism. It is sociocultural realism. It agrees with the claim of formalism that mathematics is created by us, and it agrees with the claim of realism that mathematical entities exist and have definite properties that we may be able to ascertain or determine. These two claims are not inconsistent, once we locate mathematical entities in human culture and human thought.

This is a verbal, descriptive model. Like any model, it focuses on certain specific features of the situation, and by attending to those features, it seeks to explain a pair of undeniable facts. We want answers, not just freedom to choose this answer today and that one tomorrow. I say that mathematics is just something people do, especially those people called “mathematicians.” To decide what mathematics is, one must study mathematical practice—including watching how and what we ourselves do and think, when we do mathematics. And mathematical entities, from numbers up to categories, are just ideas in our heads—shared socially validated ideas, to be sure. That is my viewpoint. It is a viewpoint—a position! It has not secured many endorsements or adoptions by philosophers. Mathematicians and mathematics educators do not seem to see anything wrong with it. Maybe philosophers who decide to become pluralists will include my humanistic philosophy among the ones they are willing to tolerate.

I see an analogy to cognitive pluralism in psychology, which recognizes visual, kinetic, and interpersonal intelligence along with the traditional test-taking math/verbal kind of intelligence (Work of Howard Gardner and Vera John-Steiner).

Proposal

I support Professor Friend’s campaign for a pluralist attitude in philosophy of mathematics. I think it will be strengthened by thinking of the various “positions” as models and as pictures of mathematics focusing on one side of mathematics or another. The philosopher will then be readier to interact with the historian or the cognitive scientist.

Why not seek for a unified, distinct scholarly activity of mathematics studies, the study of mathematical activity and behavior, by all possible methods? An interdisciplinary field, a friendly companion to mathematics itself, not competing but supplementing it. Mathematics studies could be comparable to linguistics as the study of language behavior, by all possible methods. It would not interfere or compete with mathematics departments, any more than linguistics departments compete or interfere with long-established departments of English literature, French literature, Russian literature, and so on.

Rather than disdain the aspect of mathematics as an ongoing activity of actual people, philosophers could seek to deepen and unify it. How do different models fit together? How do they fail to fit together? What are their contributions and their shortcomings? What is still missing? This role for philosophy of mathematics would be higher than the one usually assigned to it.

A coherent inclusive study of the nature of mathematics would contribute to our understanding of problem-solving in general. Solving problems is how progress is made in all of science and technology. The synthesizing energy to achieve such a result would be a worthy and inspiring task for philosophy.

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"Now" Has an Infinitesimal Positive Duration

Reuben Hersh

Authors writing about Time have struggled to choose between a scientific instantaneous Now, with zero duration, or an experiential Now with some undefined small positive duration. The difficulty is resolved by the infinitesimal of Abraham Robinson. This article offers the nonstandard or "hyperreal" line as a model for Time, thereby to resolve a persistent controversy of the meaning of "Now." As a "monad" in the Leibnizian time axis, "Now" is a time interval shorter than any standard positive interval, yet longer than any infinitesimal.

I start out with Aristotle's *Physics*: "Aristotle's Physics A Guided Study" Joe Sachs, Rutgers U Press 1998 page 120–124 Aristotle chapters 10–12.

...though time is composite, part of it has happened and part is going to be, while none of it is. The now is no part of it. For the part measures the whole, and the whole must be composed of the parts, but time does not seem to be composed of nows .... If there were no now, there would be no time.

The now is manifest as the thing carried along, like a unit of number, But in addition [unlike number], time is continuous by means of the now and is divided by the now. And the now marks off the motion into a before and an after and this it does in a manner corresponding to that of the point.

But the now is always other through the moving of the thing carried along. Insofar as the now is a limit, it is not time but an attribute of time; but insofar as it numbers, it is a number.

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Chapter 13

The now is a connection of time, as was said, for it connects the past time and the future. And it is a boundary of time, for it is the beginning of one part and the end of another. But this is not as clear as with the point, which stands still. And the now divides, potentially; and insofar as it is a division, the now is always different, but insofar as it binds together, it is always the same, as with mathematical lines. Thus also, the now is in one way a division of time, potentially, and in another a common boundary and union of its parts. And the dividing and the uniting are the same act of the same thing, but the being of them is not the same.

To paraphrase Aristotle briefly:

In order to separate the past and the future, a single point "now" suffices. (This automatically invokes an image, the "straight line," and brings in the mathematics of Euclid, as modernized of course.)

On the other hand, in order to be faithful to the unbroken connectedness of experience, we require a "now" that overlaps the past and future, a positive duration for what is happening "right now."

Automatically and uncritically we think of time as a straight line, with a positive direction, infinitely long in both directions. It's split between the future, a ray stretching to the right, and the past, a ray stretching to the left. This pair of disjoint half lines is separated or joined by a single point, "the present," "the Now."

This image, this time line, is not *Time*. Time itself is not a line, it is the general phenomenon of change. The time line is just a *model* of Time, a mathematical model. It helps us to conceptualize or visualize experience. Like any model, it is not *identical* to the object being modeled, which is the Time we live through, either in ordinary life or in experimental science.

"Now" is a primitive of human language, as when I yell at you, "Do it now!" It is not a problem in common conversation.

But when Logic sticks in its head, a problem does arise. The book edited by Durie presents arguments about the Now by distinguished authors, including Henri Bergson and Gaston Bachelard. Bergson tells us the Present must have a duration. For anything at all to happen, it takes at least a little bit of time. In zero time (without duration), we'd be with Zeno, stuck in a "place," unable to move. But things are happening; we are living and breathing, now! For Bergson, "now" holds together the past and the future, a duration where the future becomes the past.

To Bachelard, "Now" is merely a point separating the past and the future.

Bachelard claims to settle the argument by pointing to Albert Einstein. The "event" in Relativity Theory has a time coordinate, a number. End of argument.

And in this regard, not just Einstein but all of experimental science can be cited. Isaac Newton wrote that he was calculating velocity *at the very instant* when the falling stone hits the ground. Since the work of Newton, observable, measurable motions have been modeled as "functions" of time. We record observations using two lines. One is a time axis. The other is an axis of distance, temperature, pressure, or some other "state variable." The state variable is called a function of time, which means merely that a point on the state variable axis corresponds to some point on the time axis. The use of points or "instants" is taken for granted.

This methodology is so universal, so well established for centuries, that it is taken as an objective reality, as a real truth, not just a successful model. But it doesn't fit well with our subjective time sense, like how long we've been waiting in the doctor's office. Without bringing in any mathematics, we know which things have already happened ("are in the past"), which ones are going on now ("in the present"), and which ones haven't happened yet (they're "in the future"). That kind of "experiential time" is not quite the same as Newton's "everywhere always equable flow." Yet they are not unrelated. They have to be compatible in practice. We manage to shift back and forth between the "independent variable" of physics and the past, present, and future of daily life.

There is the objective, scientific duration or time, the independent coordinate of mechanics. And over against it to be reconciled with it is an internal, subjective sense of time, a consciousness of time passing either slowly or quickly, the flow of time that we feel even with our eyes closed in a silent room, that seems entirely different from the time by which we describe the rising and setting of the sun and the moon.

We seem to be facing a conflict between our lived Now, a brief duration where Past and Future seamlessly merges, and our scientific practice, which evaluates an observable at any given instant.

But this opposition is delusional. It is based on the delusion of immaterial consciousness. Our minds are embedded in our bodies; they are activities or functions of the brains and nervous systems which are not free floating; they are in our flesh, our muscles, and guts. My heart is beating. My breath goes in and out. My stomach empties and needs to be refilled. My buttocks weary from long sitting. All this information available to my brain is somehow combined or condensed into a sense of time passing. It is time to get up, or to lie down, or to find a snack.

The coming and going of daylight, the coming and going of the seasons of the year, and the aging of ourselves and others impose their own measure of lived experience, before or without any intervention of clocks. Muscle fatigue, hunger, heartbeats, and breaths in and out, all are different clocks sending signals to our brain where they are compared, coordinated, and synthesized to give us a subjective sense of time passing. How long have I been sitting here waiting? How long has it been since I have heard from so and so? As I stand I feel the passage of time in my muscle fatigue. Or as I sit, I feel the passage of time in my buttocks and back. Maybe I even get a backache. All the while of course, my breath is entering and leaving my lungs, and my heartbeat is continuing, perhaps at a steady pace, perhaps with speedups and slowdowns. Fatigue not only of the muscles but of the nervous system. The detached observer is a myth. We ourselves, as real observers, are embodied, with stomachs and brains.

There is keeping time, marking time, making time, losing time, but that doesn't mean that there is an actual thing that you are marking, or keeping, or losing, or making, as the case may be.

The "Now" is an old problem in experimental psychology. How long must a sound or a flash of light persist, in order to be perceived? William James called it "the specious present" and reported experiments in Germany. The empirical reality of the "specious present" is the number of milliseconds that a sight or a sound must endure in order to be perceived.

The empirical Now shrinks as we invent more powerful observing instruments.

On my desk is a calendar with illustrations from high-speed photography. A bullet is shown in midair, just as it has passed through an apple. It was in that position for "an instant." But the camera shutter had to stay open long enough for the photographic chemical process to take place. No photography can be instantaneous. If Now takes some time, then how much? Milliseconds? Nanoseconds? Microseconds?

This empirical and experiential fact seems to ignore a certain logic of past and future. That little interval on the line, the Durational Now, will have boundary points where it meets the Future and where it meets the Past. The perceptive time interval must extend between two instants, a beginning and an end, and we are still left with the question, what should we mean by "now"? When does the Now begin? How long does it last? When does it end? In trying to answer these questions, we seem forced to narrow the Now back down to a mere point.

To someone not indoctrinated in modern mathematics, not ashamed to use the word "infinitesimal," it would be tempting to propose a duration of "Now" that is neither zero nor positive. Something in between. "Infinitesimal."

The standard "real line" taught in school is subject to the Axiom of Archimedes:

"Any interval, no matter how short, will become longer than any other interval, no matter how long, if added to itself sufficiently many times."

This axiom amounts to saying, "There is no infinitesimal," if by "infinitesimal" we mean something so short that no matter how many times we add it to itself, the result will always be less than one unit (inch, mile, whatever you want to choose as your unit).

Is this Axiom *true*? It is true of the standard real line, because it is part of the definition of the standard real line. Circular reasoning, irrefutable. Is there any other real line that is not the standard one? Yes! It is called the Nonstandard line! (Also called the "hyperreal" line.) It has been recognized and accepted into established mathematics since 1966, when the logician Abraham Robinson published his famous book *Nonstandard Analysis.*

Already at the foundation of the calculus, in the 17th century, Leibniz and his predecessors Cavalieri and others used infinitesimals to calculate areas and volumes. To do so required a certain finesse, because they were unable to explain exactly the meaning of the word "infinitesimal." Roughly speaking, "An infinitesimal is greater than zero, but smaller than every positive number." This definition tells us that an infinitesimal is smaller than itself! Blatantly self-contradictory. Leibniz explained that although infinitesimals do not actually exist, still we can think about them as if they did exist. In the 19th century, Cauchy, Dedekind, and Weierstrass showed how to do calculus without saying "infinitesimal." We use a couple of little extra variables, usually called epsilon and delta. Against the rules of mathematics, geometers, physicists, and engineers continue to think and talk with infinitesimals.

Robinson the logician was experienced in applied mathematics. He used logic to construct a continuum, a "line," where every standard number is surrounded by a little cloud of infinitely close nonstandard numbers. How did he get away with it? He had a new tool—modern formal logic, thanks to Frege, Whitehead, and Russell. Mathematical logic describes mathematics formally with precisely stated symbols, grammar, and deductions. The "standard real line," that is, the set of standard numbers, subject to the Axiom of Archimedes, becomes expressed in a definite formula. Then it makes sense to talk about nonstandard numbers. An infinitesimal is a number greater than zero but smaller than every *standard* positive number. No more self-contradiction!

I propose the hyperreal or Leibnizian line as a suitable mathematical model of time.

Nonstandard analysis is no longer a startling novelty. Half a century has passed. Nonstandard analysis is in elementary calculus books by Howard Jerome Keisler and others that have been used successfully in courses at several universities. It is a powerful research tool. Although most people go through grad school without meeting it, it isn't controversial any more. It is a well-established part of mathematics and a respected research methodology.

In Leibniz's calculus, velocity was a ratio, an infinitesimal distance divided by an infinitesimal time interval. Robinson uses this nonstandard ratio and obtains as a standard velocity the unique standard number infinitely close to this nonstandard ratio. In this calculation, time is being modeled by a nonstandard real line. In the present paper, I point out that this model of time is useful, not only for calculating velocity but also for endowing the Present, the Now, with a positive duration, thereby relieving the tension between the experiential now, which has duration, and the scientific now, which must have a definite location.

The nonstandard real line has a zero, which is a standard real number, and around it a set of infinitesimals, both positive and negative. Robinson took a word from Leibniz and named the cloud of nonstandard numbers infinitely close to any standard real number a "monad."

In a hyperreal model of time, we could choose to say, without contradiction, that past and future overlap or intersect in the present. The present would be a monad; it would be all the nonstandard points infinitely close to some standard point. "Now" would be defined as the intersection of past and future. It would have a finite positive duration. That duration would not equal any number, either standard or nonstandard, for it would be smaller than any standard number, yet greater than any infinitesimal.

The subjective "present" does not have any definite beginning or end. This feature is shared by the nonstandard monad, which does not have a first or last element.

One of the difficulties of describing time phenomenologically is the impossibility of assigning any number to the duration of the instant. The monad does not possess a numerical magnitude. As a model of any instant, including the present instant, it has positive duration, greater than any infinitesimal, yet still less than any standard. This paradoxical feature of the hyperreal model fits nicely with phenomenological introspection.

Intuitively, it makes more sense to think of past and future overlapping than to think of them as disjoint, separated by an instant (the present) through which they can never touch each other. How can the future flow into the past, as tomorrow turns into yesterday, without tomorrow ever being allowed to touch yesterday? The mystery about the future flowing into the past is an artifact of the standard mathematical model. It falls away in the hyperreal model. The future and the past overlap infinitesimally. The infinitesimal overlap of future and past is the region of actuality, of things happening and changing.

Of the three pieces of time—past, present, and future—it is the now that is really real. "Be here now!" That reality requires duration.

Miller presented an exposition of the *phenomenological now*, according to Edmund Husserl. Consider how we hear a melody. The melody is a sequence of tones. We hear one tone at a time, yet we listen to the melody as a whole, not as a succession of unrelated tones. We remember the tones we have just heard, and we anticipate the next tone which we expect to come soon. Before the "primal impression," then, we have a "protention," and in passing it leaves behind a "retention." The connections between these experiential aspects of time permit us to have a connected experience of the whole melody. In fact, the same analysis applies even to our experience of each separate tone of the melody, for even a single tone has a duration, and a "primal experience," a "protention" and a "retention."

This analysis of our experience depends on our already accepting the objective fact—that one tone does really precede or follow another. This amounts to accepting the objective reality of Time, accepting that things do happen out in the World, that one happening does precede or follow another.

The difficulty is with our picture of time as a Euclidean or Newtonian line. These lines carry along with them the Archimedean axiom: "Any interval, no matter how short, can serve to count any epoch of time, no matter how long." There is another kind of line that people have thought with, where infinitesimal intervals are allowed—"infinitely short"—so to speak. They were part of calculus when calculus was first invented. Velocity at an instant was just the ratio of the infinitesimal distance traveled to the time elapsed in an infinitesimal time interval.

This made sense, it worked, it was understood, but it was not Aristotelian. It did not fit into the "yes or no" framework of Logic. With effort, it was abandoned for an Archimedean framework that relied on inequalities of two small variables called epsilon and delta. In modern times, a way has been found to marry the Infinitesimal to Aristotle. The key was mathematical logic. Once logic became precise and powerful enough to have its own theorems, one could talk about language and logic themselves as part of mathematics. Then we could refer to the Archimedean number system of Euclid and Newton and call it Standard. We can talk about another kind of number, a nonstandard one, that is smaller than every standard one, and still positive. An infinitesimal. That is Robinson's nonstandard analysis, which is now established as part of pure mathematics and tool of applied math. In contrast to the standard Archimedean or Newtonian line, we call it Leibnizian, because nonstandard analysis is a rigorous reestablishment of Leibniz's infinitesimal calculus.

The nonstandard real line is a better model for Time than the standard one. We can still think of the present instant as the zero on our time axis. We can associate to it a duration which is less than any standard positive number no matter how small, yet still greater than any infinitesimal. This would be what Robinson called the monad of zero, the set of positive and negative infinitesimals.

It satisfies the phenomenological requirement for a Now that is associated with an instant in time yet has a duration greater than zero. The seeming contradiction is not in the experience of Time; it is only in the attempt to model time by the standard line of Euclid and Newton. Robinson's hyperreal or nonstandard line is a better model, a better fit or description of time as experienced.

For the purpose of representing physical processes, we can choose either version of the line. The infinitesimal is closer to naive intuition and was never really expelled from applied mathematics.

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Review of *How Humans Learn to Think Mathematically: Exploring the Three Worlds of Mathematics*

Reuben Hersh

Educational theory is not my area of expertise. This is an important book on educational theory and practice, deserving an expert and thorough review. So why did I agree to review it? In answer, I quote a friend here at the University of New Mexico. "You write a book review because you want to say something about the subject of the book. But you are required first of all actually to tell about the book itself."

I will tell you about David Tall's impressive new book about how humans learn to think mathematically. I will also tell you my own thoughts, about how humans think mathematically, to discover and prove new theorems. It turns out that these two questions—how does one learn to think mathematically and how does one actually do research in mathematics—are almost the same question! I will shamelessly advertise my own newest book, *Experiencing Mathematics* (American Mathematical Society, 2014).

To answer either of the two questions I have just stated, one is forced, implicitly or explicitly, to confront the basic dilemma: what is the nature of any mathematical entity, be it a theorem, an algorithm, a counterexample, a conjecture, or even a whole theory, a complex combination of all four?

Does it reside in some other worldly reality of "abstracta," neither here, there, nor anywhere else? (See "platonism" or "realism" in the philosophical literature.)

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Is it only a fiction, not even claiming the right to "exist"? (See "fictionalism" or "nominalism" in the appropriate counter-literature.) Or does it actually exist, real and down here, close to us, accessible to us? (See *Experiencing Mathematics*.)

These are questions I have been fighting with for a long time. They are my reason for reviewing the book. However, I am duty bound to postpone them to the last half of this review. First, let's look at the book itself.

There is something called "thinking mathematically" that some humans learn. But *how* do they learn it? A big question. It takes a big book to tackle it.

David Tall has written two PhD theses—one in algebraic topology, under Sir Michael Atiyah, and one in cognitive psychology, under the psychologist Richard Skemp. He has taught in many classrooms—primary, secondary, tertiary, and postgraduate. The first half of his book is about "school mathematics," and the second half is on university and postgraduate mathematics. Different people attain different levels of "thinking mathematically." Interesting concrete problems enliven the generalities on learning and teaching.

Tall explores "three worlds of mathematics: the embodied, the symbolic, and the formal," using social, cognitive, and computational perspectives. He recognizes the contributions of cognitive neuroscience. He talks about the embodiment, compression, connection, and blending of mathematical ideas.

He describes and analyzes the development of mathematical thinking, from the young child to the sophisticated adult. At each level, mathematical learning is broken down into stages: recognition, description, definition, Euclidean proof, and formal rigor.

He is fully at home in the mental world of research mathematicians and tries to make it available and comprehensible to readers who work at the primary and secondary levels. A mathematical concept that is fully developed, solid, permanent, seemingly independent, and eternal is named "crystalline."

The book omits important aspects of learning mathematics. By whom? For what purpose? The words "gender," "ethnic," or "class" do not appear. It discusses emotion, but not the emotional interactions between learners, or between learner and teacher. It doesn't mention well-known, important contributions *in practice* (not theory) by Uri Treisman, Clarence Stephens, or Robert Lee Moore.

Tall includes many examples of "wrong answers" to test questions. What is behind the wrong answer? What misconception or misunderstanding misled the student? He finds many ways students go wrong by trying to follow rules or methods "met before" that are not applicable.

Tall recognizes that great obstacle to communication between learner and instructor—the disparity between how mathematical content is seen by the learner encountering it for the first time and by an instructor who knows it all by heart. The difficulty in learning new mathematics often comes from conflict with older concepts. Progress in school requires successively learning three or four different mathematical structures. At each stage, the rules change. What was previously forbidden now becomes possible. Tall's account is different from the standard one. He doesn't imagine that each step forward requires nothing more than a clear explanation of new rules and laws. On the contrary, it requires serious mental effort and flexibility. For example, in the natural numbers, the pupil could only "take away" an amount smaller than what she already had. But in the integers, any number can be subtracted from any other, regardless of which is greater. In a similar way, it takes mental effort to move from the integers, where division is possible only if the divisor "goes into" the dividend, to the rationals, where anything can be divided by anything else—except zero! Each step into a new, bigger number system is a cognitive leap; it is *intrinsically* difficult and demanding. Tall coins the terms "met-befores" and "set-befores" to discuss these complications. His continued focus on the learner, not just the teacher, is most welcomed.

A central theme, which goes back to the fundamental contributions of Jean Piaget, is the transformation of "knowing how" to "knowing that." For example, a pupil may calculate correctly, without yet knowing how to talk about *the calculation itself*. The operation of subtracting, "taking away," is readily understood for natural numbers, when what's "taken away" is smaller than what's available. When she moves to the integers, the operation of "taking away" is *reified* (Anna Sfard's term) into something new and strange—a "negative number." A verb is transformed into a noun! To calculate correctly with these negative numbers, she must learn new rules that violate some of the rules she learned with the natural numbers.

Later on, functions take a similar leap. To begin with, any particular function is just an operation that associates one number to another. But later on a whole class of functions becomes a *thing* that you operate *on*—differentiate or integrate, add or multiply. This leap is intrinsically difficult for many students. For the instructor it's an obvious triviality.

To the big question, "How do humans learn mathematical thinking?" a first simple answer might be, "They go to school." But standard schooling focuses on calculating—which is but the first step to "thinking mathematically."

Classroom teaching and the design of curriculum and assessments all depend on a "theory": What must pupils at each stage of education be able to do? What mathematical content ought to be learned and taught? Tall works within the assumptions about these matters that are presently standard worldwide. "Thinking mathematically" begins, as usual, with basic arithmetical calculation and algebraic manipulation and simple problem-solving in geometry, algebra, and arithmetic including "real-world" problems, and goes on from there to the top level, discovering and creating new mathematics. He does pay some attention to statistics. He has his own method of teaching introductory calculus. Computer graphics create a "microscope" in which the graph of an algebraic or a trig function is seen to be "locally straight" (one might say "infinitesimally straight"). Then the slope of this infinitesimal tangent line segment can serve as *the definition* of the derivative, and the expression dy/dx is actually a fraction, the rise of this infinitesimal line segment divided by its run.

Tall is eclectic. He writes "I spent years comparing and analyzing the conflicting elements of different theoretical frameworks until I realized that greater progress can be made by seeing each alternative in a sympathetic light in its own appropriate context and seeking to blend together different viewpoints to evolve new insights." (page 412) Among the writers who have influenced him are three of my old friends—Shlomo Vinner, Anna Sfard of Israel, and Ed Dubinsky of the United States. I was delighted to learn about Vinner's expression, introduced long ago, of the "concept image." It is very similar to the "mental model" I write about in explaining how mathematicians' proofs work to compel agreement.

Explicating what is meant by "thinking mathematically" is a nontrivial undertaking. The attempt to understand how humans learn to think mathematically requires theorists to produce models of mathematics itself. We need not demand the complete Truth from any one model of mathematics. We can ask of it merely, what can we get out of this model to help understand how mathematics works?

The mathematics we teach in school is part of *established mathematics*. Established mathematics is a social-cultural-historical phenomenon. Of course it actually "exists"! Its representations exist in books and computer memories, of course, and most importantly, in the minds of the people who have learned it—in particular, qualified teachers of mathematics. The teacher works to guide and assist the learner in acquiring these concepts—that is to say, in re-creating them in the mind of the learner. Education presupposes that *our minds are real, our thoughts are real*.

Some nominalists deny that numbers and other mathematical entities exist at all. The "prince of nominalists," William of Ockham, was more careful. He wrote, "No thing outside the mind is universal [...] It is just as great an impossibility that some thing outside the mind be in any way universal [...] as it is an impossibility that a man be an ass." If numbers and so on exist only in the mind, we need to clarify *whose* mind we are talking about, and just how, in what way, mathematical entities exist there. People working in either education or research look for answers that are compatible with their work experience.

David Tall's theory of mathematics learning implicitly presupposes a theory of mathematical reality. It is incompatible with either the fictionalist or the platonist theories currently in academic Anglophone philosophy of math (see Bonnie Gold's *Proofs and other dilemmas* published by the MAA, or Stewart Shapiro's *Handbook of Philosophy of Mathematics and Logic*).

The activities of learning and teaching mathematics are about actions and concepts or processes and concepts that are *taken for granted as real entities*. Could these concepts and processes be mere fictions, no-things, or non-existent? Such a notion is deeply hostile to, if not incompatible, with mathematics education.

Or alternatively, are they real entities "out there"—independent of human apprehension—"abstract" in that sense? Such an attitude is alien to the learner-centered humanistic math education advocated by Tall and others who are focusing on how best to serve math students at all levels. The fictionalist and platonist narratives are remote both from living education in mathematics and from living research in mathematics.

Mathematics is a collection of practices and concepts of human beings. Mathematics education and mathematics research are human activities in the same realm, and it's natural that their philosophical foundations are compatible. The successful student reconstructs what is already known by others, to acquire his own mental model of a math concept. The researcher constructs something new, by inspecting and manipulating her own mental models of mathematical concepts.

The philosophical viewpoint of the teacher and student (as implicit in *How Humans Learn to Think Mathematically*) and that of the researcher (as explicated in *Experiencing Mathematics*) fit together.

Mathematical entities or concepts are equivalence classes of mental models. Each person who grasps or possesses a concept has a mental model, a representative or sample of that concept. Different representatives possessed by different people are required to be equivalent, in the sense of giving the same answers to test questions. This equivalence is what makes possible successful communication about a mathematical concept. Mathematics education consists on the one hand of reconstruction by the learner of the desired mental model (with the assistance of a teacher or a textbook) and on the other hand of testing and grading (called "assessment") which continually and repeatedly require the student's mental model to be congruent to the teachers', that is to say, to the community of mathematics. Thus, the practice of mathematics education accords with the notion of mathematical reality offered in *Experiencing Mathematics*. A student is accepted as competent by the mathematical community if, by passing the tests, she has demonstrated that her mental model of the concept is congruent to the standard one of the community.

Tall's book is an impressive achievement. He has spent a lifetime thinking deeply and observing carefully how students learn mathematics or fail to learn it, and he has synthesized the whole pathway from the beginning up to the climax.

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Can You Say What Mathematics Is?

William Byers

A more complete discussion of some of these ideas can be found in William Byers (2015). *Deep Thinking: What Mathematics Can Teach Us about the Mind,* World Scientific.

Introduction

Is mathematics something that can be pinned down? Does it have an objective meaning? Is it even objective at all? The questions that are the theme of this collection depend on what we mean by "objective." We distinguish between *weak* and *strong* objectivity. This leads to another way to talk about mathematics, the philosophy of mathematics, and the theme of this collection. It also leads to consideration of the relationship between mathematics and artificial intelligence. The discussion highlights the significance of Reuben Hersh's writing about mathematics as a deconstruction of the normal, naïve belief that mathematics is objective in any absolute sense.

What Is Mathematics?

What is mathematics? Putting the question in this way doesn't supply an obvious point of entry. Compare it to Bill Thurston's question, "How do mathematicians advance human understanding of mathematics?"[1] Though seemingly obscure,

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Thurston's way of putting the question gives us a productive way to look at the nature of mathematics. It has (at least) two advantages. The first is, strangely enough, that it is circular. Circularity is irritating to mathematicians because we think that the way to progress toward capturing the essence of mathematics is similar to trying to prove a conjecture within mathematics and this begins by examining it at arms' length. Actually we feel that mathematics *is* at arms' length and that it is objective and timeless. But Thurston's question brings the objectivity of mathematics into question and tells us that a different approach is called for. Attempting to pin down the essence of mathematics is not the same kind of activity as trying to determine whether the equation $x^n + y^n = z^n$ has any integer solutions.

The second strength of Thurston's question is a variation of the first. It highlights the fact that mathematics is a process and not just a collection of things: definitions, axioms, theorems, and algorithms. Not only is mathematics an activity, but also it is a *human* activity that involves intelligence: learning, thinking, understanding, and creating. That is, mathematics, to be mathematics, involves the mind. To turn the last statement around—the nature of mathematics is not fully captured by saying that it is formal or deductive or algorithmic or even logical. This is something that Reuben Hersh has been saying for many years now. Mathematics is not even "objective" unless you are very careful about the meaning that you give to the word.

Asking and answering questions about the nature of mathematics are philosophy and not mathematics. Subscribing to a philosophy of mathematics does more than describe what is going on. It creates a context that gives meaning to mathematics. For example, formalism tells you that mathematics consists of proving theorems and setting up deductive systems. Doing mathematics is not really possible without a philosophy that tells you what mathematics is, even if this philosophy is not consciously embraced but is only implicit in what you do. Normally in mathematics and science, you only become aware of the assumptions that frame your work during periods when the assumptions are in question. I suspect that today may be such a time for mathematics because of the growth of computer technology and the techniques of artificial intelligence. So it's a good time to ask some fundamental questions.

What Is Number?

Let's start with a question that looks easier than "what is mathematics," namely, "what is number." I'm not asking about the definition of particular kinds of numbers—for example, the reals or the rationals. I'm asking about the nature of "number" in general. What is a number? The answer is not at all obvious to me. It's a hard question, and in moment, I'll take up the precise way in which it is "hard." However, if I ask you what a real number is, then the answer is more straightforward. It's also hard but not hard in the same way. The nature of mathematics is hard in the same way that the nature of number is hard. "Number" is hard because it is an informal idea (maybe pre-formal is a better term), and this informal nature needs to be pinned down by defining some particular class of numbers before the activity of (what we normally call) mathematics can proceed. We replace the informal idea by the formal one and basically forget about the former. I propose to pay more attention to the informal idea of number because it is an inexhaustible source of new mathematics.

A discussion of the nature of mathematics also lives at this informal level. Can we talk about the informal in a useful manner? Does mathematics (and number, randomness, infinity, continuity, and almost all other really basic notions in mathematics) mean something *before* it is formalized?

"What is a number?" is a hard question precisely because the generalized notion of number is not an objective entity in the same way that a real or a rational number is. It is the process of pinning it down that makes it objective. On the other hand, "number" (in general) means something; it is not arbitrary. Nevertheless these two entities live in different universes—one that is more elementary, informal, and implicit and the other formal and quite explicit. These different universes have different rules and procedures. We usually only address the nature of "pinned down" mathematics, but the crucial step in the process involves the pinning, that is, making number into an objective entity. It's not that the informal notion of number isn't objective, but it's not objective in the same way as the numbers in some explicit number system.

Is mathematics objectively true? Does it live in an objective world at all? Well in one way, it does and in another it does not. It's objective in the sense that mathematicians no matter what their race, creed, gender, and culture tend to agree about the facts of the case such as the chaotic dynamics of the function $4x(1-x)$ or the sum of the angles of a planar triangle in Euclidean geometry. When you are doing mathematics, it feels objective because you have the feeling that you are discovering autonomous facts. You feel that doing mathematics is a process of *discovery*.

On the other hand, mathematics is clearly nonobjective (subjective?) because (human) mathematicians bring it into existence. It is not objective because it is *invented* and set firmly in place by individuals, groups of mathematicians, and cultures as a whole. There is a tension in this ambiguity that lies at the heart of mathematics: objectivity and subjectivity or discovery and invention. But let's not be too quick to resolve this tension by saying that one side is right and the other wrong since this tension is the generating heart of mathematics and the reason why it works so well.

What Is Objectivity?

From a naïve point of view, "objective" means "out there," usually in the natural world but possibly in the Platonic realm that many mathematicians hold so dear. "Subjective," on the other hand, refers to being "in here," residing within the body or the mind. We sometimes say that some phenomenon is "merely" subjective meaning that it comes from personal prejudice or idiosyncratic opinion.

Thus, we would object to a mathematical theorem or scientific experiment being influenced by religion, race, or gender. Objective means that such matters, as well as many other cultural factors, do not influence the result. Of course some people consider mathematics itself to be a culture, but within mathematics, the criterion of independence from arbitrary opinion might serve as a minimal way to differentiate subjectivity and objectivity—we could call this *weak* objectivity or subjectivity.

However, there is a stronger meaning associated with the expression “out there.” Something is objective if it does not depend on mind. You could say that it is objective in this sense if it would continue to be true even if there were no human beings around. This must have been the idea behind putting a diagram of the Pythagorean theorem on Voyager 1 in the hope that the truths of Euclidean geometry were universal and would be recognized by any intelligent being. Is the Newtonian theory of gravity objectively true? Most people would say that it is even if that truth is only an approximate one. Is the relativistic theory of gravity objectively true? Is it an eternal truth? Maybe it is, but the jury is (necessarily) still out. At any rate, a scientific theory is objective in this sense if the theory exists independent of all the scientists who formulate or study it. Some people believe that the scientist *discovers* what is already there and that the rules of the universe are built-in, so to speak. Let’s call this *strong* objectivity.

Many people believe that mathematics is strongly objective and that mathematics captures some built-in truth about the cosmos; for example, the electron *is* its mathematical description. Though this belief captures something about mathematics that cannot be ignored, I believe that it also misses something. What is missing is that indispensable aspect of mathematics pointed to by the observation that what you find in a research article or a textbook is not mathematics. It’s not mathematics in the same way that a musical score is not identical to music. Music must be heard (even a deaf person like Beethoven may be able to “hear” music). Some sort of equivalent statement is true for mathematics. What you find in a paper or a textbook is only potentially mathematics. The reader has to add context, meaning, and understanding. No one can understand a paper for you. Something has to click in your own mind to make this potential mathematics into real mathematics. This is what I mean when I say that mathematics has an irreducible element of mind. It is strongly subjective.

So the answer to the question about whether or not mathematics is objective is that it is objective in the weak sense but not in the strong—free from prejudice and arbitrary opinion but not independent of intelligence. It’s obvious that mathematics is mind based, that it deals with mental constructs, and that its origins lie in the mind and brain’s cognitive and perceptive organs. Doing mathematics involves thinking. Its elementary structures are not axioms but concepts, which get grouped together into conceptual systems. Because our thoughts are transient and impermanent, human beings need to solidify these concepts and, in so doing, give them a quasi-permanent status. The reality of mathematics is that it changes; the mythology is that it is objective, permanent, and so unchanging.

Only the formal traces of mathematics—what is written down—are objective and last for a long time. Real mathematics has different characteristics. It is a way of understanding the world that comes into being through acts of creativity. But it is a human way of understanding. Either it is alive or else it is trivial. Initially the mathematician who creates it brings it to life, but then the mathematical community recreates it through acts of comprehension. So real mathematics is impermanent. It comes into being and then falls into irrelevance and disappears. Those who view mathematics (and science) as permanent and unchanging are doomed to disappointment. There was a time when people spent a lot of time proving metrization theorems in general topology. Today no one cares. Even the axioms and other foundational elements of mathematics will change. There is nothing that you can count on. Logic itself will change. There is no characteristic of mathematics that you can point to and say this is permanent and won't change. But this doesn't mean that mathematics isn't real. Music has changed radically from the time of Bach, and some people who are attached to the old forms will say that what you hear today is not "real" music. But there will always be music because music is a fundamental way in which human beings interact with the world. Similarly there will always be mathematics because mathematics is an essential way in which human beings understand the world. By emphasizing the sociocultural aspects of mathematics, Reuben Hersh knocks mathematics off its Platonic/formalist pedestal and brings it back into real life.

So mathematics is not strongly objective, and yet it opens a window that looks out at what is real and true. The mind manages to access what is real. In physics, this correspondence between the mind and the natural world amazed Einstein and was the source of his spiritual sensibility. This correspondence is also behind what has been called the "unreasonable effectiveness" of mathematics in the natural sciences. If you eliminate mind, the mystery of how we humans are able to see so deeply into the natural world disappears but at a terrible price—the loss of the feelings of mystery and wonder that make doing science so rewarding.

Nevertheless strong objectivity (no mind) remains a goal for many mathematicians, and one of the motivations for pursuing it is to establish mathematics as a domain independent of human intelligence, that is, to reverse the natural order of things, that human beings create mathematics, and incorporate mind into mathematics or computer science, which would be tantamount to eliminating mind altogether. However, in order to get strong objectivity, you need more than mathematics; you also need some philosophy or paradigm such as formalism, Platonism, or algorithmic intelligence. Mathematics, without such a philosophy, is ambiguous, both objective and subjective. You can explain the role of certain philosophies of mathematics as attempts to remove this ambiguity from the description of the subject.[2]

I mentioned Platonism in mathematics. Platonism posits an ideal domain where the truths of mathematics reside. This belief makes mathematics strongly objective. On the other hand, just where is this Platonic world? Maybe it only reflects the psychological need that human being have for certainty. If so, it would depend on mind. However, there may indeed be a deeper truth to Platonism if we understand the

Platonic world as the world of mind, that is, consciousness or even intelligence in the most general sense. Perhaps it is possible to have a more subtle form of Platonism that includes mind. Maybe that is what I am reaching for in this article.

Reuben Hersh seems to think of Platonism as a kind of myth because he wants to highlight the sociocultural dimension of mathematics. His work is a kind of deconstruction of the naïve, absolute, Platonic objectivity of mathematics. Of course most working mathematicians want nothing to do with this view. They prefer to continue proving theorems, and for this activity, it is convenient to assume a kind of working Platonism. Be that as it may, we can observe that Platonism "solves" the problem of mathematical objectivity.

Conceptual Systems

Mathematics is not objective in the strong sense (and in the same way, but more controversially, neither is science). In order to properly come to grip with what is going on, it is useful to discuss some concrete situations in mathematics. Consider conceptual systems in mathematics. What is a conceptual system (CS)? A CS is a mathematical structure, like the real numbers or topological spaces, looked at from the inside (so to speak). Actually a better way to think about the nature of a CS is to turn the previous sentence around and say that what we usually think of as mathematics, say the real numbers as a complete ordered field, arises by removing mind from the conceptual system of the real numbers. The CS is of primary interest, and what we usually consider to be mathematics is only of interest to the extent that we restore it to life and make it a CS instead of an abstract structure, by applying our intelligence to the system. It is worth noting that computers work with the formal structure and not the CS.

The difference between a CS and the formal structure that represents it is analogous to the difference between a concept and a definition. Continuity, for example, is a concept, whereas the usual ε-δ statement is a definition. The latter is a complex logical statement but even having some familiarity with the subtleties of the definition doesn't ensure that one understands continuity. Students have said to me, "I follow it but I don't understand it." This statement makes perfect sense and describes many students' lived experience with mathematics. It also isolates the problems that student often have with formal proofs. Concepts need to be understood. Definitions can be followed logically which might mean that you can understand the words and symbols that make up the definition and how they are logically related to one another.

You can have a way of understanding continuity without necessarily being capable of using the definition. The concept stands behind the definition,[3] that is, the definition is a way of formalizing the concept (there are usually more than one). "Concept" is informal and implicit whereas "definition" is formal and explicit. Of course we use the expression "understanding the definition" when we really mean understanding the concept behind the definition. Concepts must necessarily be understood. On the other hand, definitions are formal entities that can merely be verified either by a person or by a machine. You can get to the concept through the definition, a series of generic examples, geometric diagrams, or in some other way or combination of ways. The definition is formal, and you make it your own by adding understanding and intuition. On the other hand, it is not a concept (for you) if these subjective elements are not included.

A conceptual system contains a family of concepts that are linked together in an organic manner with each concept influencing the others. It shares a great deal with the idea of a paradigm in science. The CS of the real numbers also contains the concepts of infinity, series, limits and continuity, order, linearity, cardinality, and many more. Conceptual systems in mathematics, even the simplest ones like the counting numbers, are potentially infinite because there are a limitless number of results that are potentially accessible within the system. Nevertheless all conceptual systems have boundaries, that is, they are intrinsically limited because there are problems that can be stated in the language of the system that cannot be solved within the system. Think of the reason why the radius of convergence for the power series representation of the function $1/(1 + x^2)$ is 1. The statement is made within the real numbers, but it is best understood by moving up to the complex numbers.

A conceptual system is not (strongly) objective because a conceptual system needs to be understood. To work with it at all requires fluency with its language and concepts. You don't look at a conceptual system from the outside so much as you explore a conceptual system from the inside. An interesting conceptual system is a universe, and you can spend your lifetime exploring that universe. On the other hand, it is certainly objective in a weak sense, so you can prove results and discuss them with others.

The conceptual system itself may not be objective in a strong sense, but once inside the system, there is a sense in which things are strongly objective. The conceptual system conveys a strong objectivity to its properties and theorems. Within the conceptual system of Euclidean geometry, Pythagoras' theorem is true, and the sum of the interior angles of a triangle is two right angles. But it is only within a conceptual system that you can ask whether something is objectively true or false, and even that question is made subtle by Gödel. So even if it is reasonable to ask whether some conjecture is true or false within a CS, asking whether the system itself is objectively true is just not a very good question.

A CS has an irreducible subjective element, which you can call intelligence, understanding, or mind. Because of this, everyone has his or her own take on a given conceptual system. So there is not a unique conceptual system that we all share in common. On the other hand, a CS is not arbitrary and neither is an individual's grasp of the system. Let me elaborate a little on the last statement. I have been told that when biologists scan a specimen with an electron microscope, they first stain the specimen in order to be able to distinguish between the various features on the slide. In other words, the stain highlights certain features that interest the scientist. By analogy, a conceptual system brings out certain features of the mathematical situation and omits others.

Conceptual systems are not arbitrary. They are best thought of as insights into some aspect of the mathematical phenomena that is being studied. The CS of the rational numbers was developed in order to make a ratio into a number. In the process of giving an expanded definition of number, it gave new insights into other things like music, for example, and even provided the ideal of rationality that pervaded Greek civilization. Even the CS of the counting numbers, the first number system to be learned by children, highlights things like linear order, arithmetic, prime numbers, and so on.

An alternate metaphor (to the one that says that a CS is a stain) is that a CS is a window on a certain mathematical situation or a situation that has the potential to be described in mathematical terms. CSs are necessarily incomplete, and yet we cannot do mathematics without them—without a window you wouldn't see much of anything because things would not be differentiated enough to work with.

The role of CSs will be further clarified by returning to our discussion of number. "Number" in its most general, pre-formal sense does not live in a CS. "Number" generates conceptual systems, namely, the various number systems. "Number" in general reflects some aspect of the world that is so profound that it seems to be built into our brains as infants not to speak of the brains of other animals. There is even evidence from psychologists that infants (say, age six months) even have (at least) two rudimentary conceptual systems for number.[4] Why would the development of such systems be so basic? The answer may be that "number" is a very basic aspect of the world of experience, a way of answering the questions, how much? and how many?, and thus getting hold of the vital idea of quantity. However having a built-in propensity for number is not enough.

In order to work with the idea of number, to do mathematics with it, you have to place it in a conceptual system, the counting numbers, the Greek measuring numbers, or some other number system. At this stage, it is objective in a strong sense, and you can prove things about these specific kinds of numbers. But different number systems highlight different properties of number. Furthermore because every CS is incomplete, we are forced back to the primordial question of what a number really is and solve the problems that arise by defining number in increasingly complex and sophisticated ways.

Different Conceptual Systems Are Incommensurable

Thomas Kuhn in his famous work[5] on scientific paradigms made the point that different paradigms are incommensurate (his word, taken, presumably, from mathematics) with one another. We can see this incommensurability at work if we consider the counting numbers and the fractions as different CSs for number. Ask a child, how many numbers there are between 2 and 3, and his or her answer will tell you which CS they are currently living in. If they live in the counting numbers, they will answer "none," but if they live in the rationals, they will say that there are many. The two answers are contradictory, but each is correct in its own way.

It is in this sense that different CSs are incompatible with one another. The reason for this is that a CS is not just a description of some pre-existing objective domain. It *defines* that domain, that is, it brings that domain into existence. In this case, it tells you what a number is. But somehow we have the feeling that number can't be two different things, in this case that it is *either* an integer *or* a fraction, not both. This is why learning the fractions is hard. The child has the feeling that they already know what a number is, namely, a counting number, so how could 2/3 be a number? The answer is that it is and it isn't; that you have to expand your working definition of number and that is hard. Think of the Pythagoreans and the root of two or of the advent of non-Euclidean geometry.

The Evolution of Conceptual Systems

Conceptual systems are incomplete since they do not provide an adequate context for the resolution of all the problems that can be formulated within the system. For example, you can't trisect an angle or square a circle within the Greek system of geometric measuring numbers. So even though you can spend your lifetime working within a given conceptual system, it is also true that conceptual systems break down and are replaced by new systems within which there may be embedded an isomorphic image of the original system. So Newtonian physics lives within relativity, and the rationals are embedded in the reals. This forces a necessary ambiguity on the new CS since; for example, once the real numbers come into existence, a rational number can be thought of as both a ratio of integers and as a certain kind of decimal.

The rules that govern working within a CS are not the same rules that govern moving from one system to a more complex one. For example, the rules of logic work best *within* a CS where you can use logic and algorithms as tools to prove new results. Things are objectively true or false (more or less) within a CS. The results of the system grow in a continuous manner. If the computer has a use in mathematics (and it does), it is to explore the terrain and the limitations of a given conceptual system.

Moving from one system to another (think of a child who knows the counting numbers being introduced to fractions) is not an act of logic nor is it algorithmic or rule-bound in any sense. It is a discrete process, that is, there is a specific and wonderful moment when the child can say "I get it" or else use that expression so beloved of mathematicians, "it's obvious." You understand, for example, that 11/3 is not just a relationship between the numbers 11 and 3, nor is it only a problem in long division. It is a legitimate number in its own right. In this regard, check out Bill Thurston's childhood story of his personal eureka moment with regard to fractions.[6] The key step frequently involves the process of reification[7] whereby in a moment of insight, a process becomes an object (11/3, a problem in division, is a number, and −3 is not only the process of subtraction but the negative number), and as a result, you now have two different ways of looking at the same situation as process and object.[8]

Insight is not a logical process. That is not to say that logic is not involved but logic does not generate the kind of insights I am talking about here. What is involved is a kind of creativity. The essence of creativity, the way so many significant mathematical problems are solved, involves a kind of reframing, that is, inventing a new way to think about the problem. So what is involved in going from one conceptual system to another, from the rationals to the reals, for example, is finding a new way to think about the nature of number.

The process involves a kind of deconstruction (there is no rational number "x" such that "$x^2 = 2$," but we know that there is a geometric measuring number with those properties, so the rationals are inadequate). This breakdown is followed (this may take centuries) by the construction of a new and larger number system. The old mental model that you carry around has to break down so that the new one has room to emerge. Remember that a conceptual system does not describe a pre-existing reality so much as it creates or defines that reality. It determines what is real and therefore what is objective. No one gives up his or her sense of what is real without a struggle. It is hard and hard in a sense that is similar to the way I was using the word when I said that the question of the nature of mathematics or the nature of number is hard! Hard questions always generate resistance and cognitive dissonance for what is being called for is a reorganization of mental space—breaking down pre-existing brain circuits and building up new ones, if you will.

Ambiguity is always lurking around situations of multiple CSs because each CS gives you a different way to think of "number," for example. One way of approaching the notion of "depth" in mathematics (as in the statement, "That result is deep") is to relate depth to the number of different contexts within which you can think about the idea. Thus, you could say (similar to what Leonard Bernstein said about music[9]) that the more an idea can contain both ambiguity and coherence, the deeper it is.

The Escape from Subjectivity

Mathematics and science contain this irreducible subjective element that I have been discussing. But this element is in direct conflict with the reason that we give ourselves as individuals and cultures for doing math and science in the first place, which is to banish subjectivity and therefore contingency from the world, to get at the facts you could say. We come to believe in a truth that is absolutely objective as a matter of faith and are comforted by the thought that even if we are mortal and must pass from the world, our cultural creations access a realm of immortality and will be there forever.

Naïve Platonism, as I said earlier, posits a kind of mathematical heaven where truths live forever. It is a way of escaping from subjectivity and that is good and bad. It's good to reject weak subjectivity but bad and self-defeating to reject mind and intelligence. Formalism also is an escape, this time from nonlogical factors like ambiguity that invariably are called into play in creative work. It can lead to the alarming and self-defeating conclusion that intelligence is something that can be captured by logic and algorithms. Anyone who has done mathematics knows that the sequence is that you first get an idea and then try to write it up. The idea comes first both in time and in importance. Identifying creativity with logic or understanding with verification underestimates the fundamental role that mathematical creativity plays in human thought and culture.

Philosophies of Mathematics as Conceptual Systems

Now let us move this discussion up a level and discuss the nature of mathematics as a whole by thinking of a philosophy of mathematics as a CS for mathematics. Different philosophies of math highlight different aspects of mathematics. They are different windows on mathematics and, like all conceptual systems, determine what is objectively true and false. By analogy with our observations about CSs in mathematics, each philosophy of mathematics, by implicitly defining the nature of mathematics, tends to privilege a certain kind of mathematical activity. Thinking about mathematics as something that is or can be computerized, for example, tends to privilege the discrete in mathematics over the continuous.

If a philosophy of mathematics is a CS for math, then asking whether a philosophy of math is right or wrong or if it captures mathematics definitively is not a good question. Different philosophies produce different kinds of mathematics. It also follows from my discussion of CSs in math that it is impossible to ever create the perfect all-encompassing philosophy of mathematics. *Every* CS is necessarily incomplete, and so every philosophy will miss something that exists today or might exist in the future. Mathematics in general, like number, continuity, randomness, and so on, is best understood as real but informal. Formalization, defining it more or less precisely, reduces it, on one hand, but allows you to bring out its potential properties, on the other. Mathematics generates philosophies of mathematics like number generates different kinds of number systems.

We should expect that embracing the idea that there can be multiple useful philosophies of mathematics will inevitably force us to accept the existence (and even the usefulness) of ambiguity. We gain from being able to look at mathematics in multiple ways, and so we should always be open to new philosophies of mathematics and try not to get trapped within a rigid and supposedly definitive philosophy. This point of view has affinities with Michele Friend's[10] idea of pluralism in the philosophy of math.

Reuben Hersh says that philosophies of mathematics should be thought of as models for mathematics in the same way as applied mathematics provides multiple models for empirical phenomena.[11] This is fine as far as it goes but does not sufficiently account for the incompatibilities between various philosophies. These are not accidental but inevitable and potentially valuable. Think again of a philosophy as a CS for mathematics. As we saw in the case of different number systems, different conceptual systems (viewed as CSs and not as formal structures) are incommensurate with one another. It may be possible to reconcile these CSs in one mega-theory (the power of the real numbers is that it successfully integrates so many subsystems), or it may not. There may be no mega-theory that reconciles everything. Do we even want to create a world where all possible philosophies can live together in harmony? This would be nothing more than the dream of an ultimate theory akin to those people who believe in "the end of physics." It overestimates the power of a single paradigm to cover the entire field of mathematics. If you think that the beauty of mathematics is the way it keeps changing and growing; if you want to generate a new creative insight into the nature of mathematics, then perhaps the thing to do is to highlight the incompatibilities and not hide them for that's where the growth is ultimately going to come from.

Mathematics Education

A brief word about the implications of the conceptual system viewpoint for mathematics education. Education comes in two varieties, namely, (1) becoming familiar (internalizing) a given CS and (2) moving from one CS to another. These are very different situations and require different skills. The mathematics educator, David Tall, has a nice example of a child who was very proficient in addition. Therefore, when she was introduced to multiplication, she translated every problem back into addition. She had been an excellent student but now found that she was falling behind. The problem was not that she was stupid, quite the contrary. It was that she was excessively attached to a technique that had brought her success in the past. She needed to make an intellectual leap into a new mathematical situation. How often have we seen our students use mental models that are inappropriate to some new situation like the real numbers?

The objective of mathematics education is not merely to learn technical skills and algorithms. It is to learn concepts and conceptual systems and that is difficult! But the highest form of learning is even more difficult for it involves giving up on an inadequate mental model and replacing it by something that is more sophisticated. You could call this creative learning since it involves something that is similar if not identical to creativity in research. How do we teach creativity and paradigm change? Where do we find acknowledgment that such creative change is a crucial aspect of education? And yet, almost every child, at some point in his or her life, gives up on "number is positive integer" and learns that "number is fraction." How does that happen? If we could answer this question, not only could we revolutionize the teaching of mathematics but also show simple ways in which human intelligence differs from machine intelligence, why mind differs from simulations of mind, and why the glory of human creativity is in no danger of being superseded by any super or quantum computer.

Implications for Artificial Intelligence and the Prospect of Computer-Generated Mathematics

Human beings and human societies have physical, social, and psychological needs for security, stability, and continuity. Strong objectivity satisfies these needs, and in our culture, science and mathematics give us a sense of stability and objective truth. The great contribution of the Greek mathematicians was their development of logic, proof, and deductive systems. They developed a language and methodology for producing "objective truths." Ever since then, Western culture has had a fascination with this way of using the mind. This method has pervaded many disciplines beyond mathematics, such as science, philosophy, engineering, and commerce.

Recent years have seen a new approach to strong objectivity. Instead of a mind-based system of thought like Euclidean geometry, we have computer-based systems which appear to be objective precisely because they are embedded in machines. This leads to claims for artificial intelligence such as the one made by Paul Cohen that so infuriated Reuben. The human desire for strong objectivity has been reified into computing machines and algorithmic thought. Many people have the dream that a kind of intelligent machine will be created any day now. Take, for example, the futurologist Ray Kurzweil and his omega point, the leap into the realm of autonomous, thinking machines.[12] This is nothing but an updating of a very old dream of taking human beings and therefore contingency out of the world. It is a dream (or is it a nightmare) of a world where machines do mathematics and science and where computing devices are capable of creativity. In such a world, human beings are essentially redundant.

The computerization of thought and culture, regarding the human mind as a kind of computing machine, is perhaps the dominant tendency in contemporary culture whose most recent development is the Internet of Things. It is the most recent manifestation of a myth, a foundation myth of our culture, which is the idea that the scientific and mathematical theories that we create have an autonomous existence, that is, that they have a reality that is strongly objective. I read Reuben Hersh's work as bringing that mythology into question, which is why I agree with much of what he says. It is clear that I consider this myth—that the human being is a machine and mind is algorithmic—to be mistaken in ways that are obvious yet invisible to most of our culture. This is the way it is with conceptual systems. But it is dangerous! If you live within the machine/algorithm CS, then you produce a society where people *are* machines (and treated as such) and creativity degenerates into writing algorithms. It's not that there is something wrong with writing algorithms or that writing them is not creative activity. It's that creativity lies in the minds of the computer scientists and not in the algorithms that they develop. Algorithms cannot capture creative activity; at their best, they simulate creative activity. The human race will always throw up geniuses like Einstein, Shakespeare, and Beethoven who manage to turn the world upside down by looking at things in an entirely new way.

Some Answers to Hersh's Questions

Question 1.1: Can practicing mathematicians, as such, contribute anything to philosophy of math?

Practicing mathematicians in general are more or less embedded within the Platonist/formalist paradigm and, as a result, cannot contribute much to the philosophy of mathematics because they do not tend to question their assumptions about math. Of course it needs to be said that the computer is eroding these assumptions and this in ways which contribute to a changing picture of what constitutes mathematical activity.

Every so often, it becomes clear that the prevailing paradigm is inadequate to describe what is actually going on. This may precipitate some sort of crisis where the nature of the subject can be fruitfully questioned. After some time, a new equilibrium will be achieved, and things will settle down again. This is the very process that I described in any situation where one CS breaks down and is replaced by another. It is very likely that the existence of increasingly powerful computing machines will precipitate a new paradigm for mathematics, and in fact, this has been happening for some time now. Formalism is really not adequate to describe what is going on in mathematics today.

Change, of course, is necessary and healthy, but there is a danger that the change that is coming will be regressive and not progressive. What I mean is that what constitutes mathematical thought (and thought in general) may come to be *defined* by the computer and algorithmic processes. We are beginning to look at the brain and mind as hardware/software, and as a result, we will only attempt to access the process of creativity and paradigm change through computer simulations. We will then tend look to the solution of problems exclusively through the means of big data and analytics. We will forget that the human mind is capable of much more.

Practicing mathematicians have something to contribute to the philosophy of math, and it is in this context that I would rank the importance of Hersh's work. He keeps saying, "I am a mathematician and this is my experience of what is actually going on in the practice of doing mathematics." He keeps coming back to what is going on as opposed to what the prevailing paradigm or philosophy says is going on. His work is important and will be increasingly influential in the future precisely because the nature of mathematical activity is changing.

I spoke about the statement by Paul Cohen above. Its importance lies in what it tells us about the kind of AI mythology that is growing up around mathematics today. In my view, it is obvious that computers will never do mathematics in the sense that Cohen is talking about. As the statement by Thurston implies, mathematics is (by definition) what (human) mathematicians do, and I have tried to explain why this will always be true. So there may be a kind of battle in the future that will pit an AI/machine philosophy of math against a humanistic philosophy. I believe that the stakes are high—extremely high—and we need mathematicians who can think outside of the philosophical box that most are in today.

1.2. Can or should philosophers of math, as such, say anything to practicing mathematicians?

This is a hard question because most practicing mathematicians have no time for anything that is philosophical. They are too busy living within their paradigm, that is, proving theorems. But if you think that this is a time of potential paradigm change, then there is the possibility that philosophers can contribute a great deal to a discussion of what constitutes mathematical activity. Again the philosophers of math were divided into two groups with the majority stuck not only in a traditional view of mathematics but also in a view of philosophy of math as a kind of study of foundations.

My view of the proper approach for philosophers of mathematics is to try to describe what is going on while understanding that the real subject is hiding in an informal area that stands behind all philosophies, all research papers, and texts. Thus, philosophy of mathematics is an activity, just like doing mathematics, that is ongoing and will continue indefinitely.

Nevertheless it is encouraging to note that there is a group of philosophers of math (some of whom are represented in this collection) who have a view that is consistent with the views that I described above. If we are approaching a bifurcation point in our culture in general that I described as mind vs. machine, then a crisis is inevitable, and a certain kind of philosophical perspective may be very timely and influential.

1.3. 20 or 50 years from now, what will be similar, and what will, or COULD, or SHOULD be altogether different, About philosophy of math? About math education? About math research institutions? About data processing and scientific computing?

I think that I have provided a perspective on these questions. The AI question and the relationship of mind to computer will be the dominant problem of the short- and medium-term future. Everything will change: what we consider to be mathematical activity, the philosophy of mathematics, mathematics education, and even research mathematics. It is possible that mathematics will become a variety of computer science, data collection, and analytics. It is even possible that the worst happens and what makes us truly human, namely, creativity, disappears from conscious view and is replaced by algorithmic simulations of creativity and intelligence.

Notes

1. William Thurston (1994). *Proof and Progress in Mathematics*, Bulletin of the A.M.S. 30, No.7I 161-177
2. Ambiguity in mathematics is discussed in William Byers (2007). *How Mathematicians Think: Using Ambiguity, Contradiction, and Paradox to Create Mathematics,* Princeton University Press and by Emily Grossholz (2007). *Representation and Productive Ambiguity in Mathematics and the Sciences*, Oxford University Press.
3. Which is why I used the term proto-concept in William Byers (2011). *The Blind Spot: Science and the Crisis of Uncertainty,* Princeton University Press.

4. Susan Carey, (2009). *The Origins of Concepts,* Oxford University Press
5. Thomas Kuhn (1962). *The Structure of Scientific Revolutions,* University of Chicago Press
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8. c.f. Eddie Gray and David Tall (1994). *Duality, Ambiguity and Flexibility: A 'Proceptual' View of Simple Arithmetic*, Journal for Research in Mathematics Education 25, no. 2:116-140
9. Leonard Bernstein (1976). *The Unanswered Question: Six Talks at Harvard.* Harvard University Press, Cambridge, MA
10. Michele Friend (2016). *Pluralism in Mathematics: A New Position in Philosophy of Mathematics,* Springer
11. Reuben Hersh, *Pluralism as Modeling and as Confusion,* This Collection
12. c.f. Ray Kurzweil (2005). *The Singularity is Near: When Humans Transcend Biology,* The Viking Press

The Exact Sciences and Non-Euclidean Logic

David A. Edwards

What "really" exists pervades the sciences and human thought in general. The belief that the infinite does not really exist goes back at least to Aristotle. Parmenides even questioned the reality of plurality and change. (Einstein's vision has much in common with Parmenides.) Toward the end of the nineteenth century, an acrimonious exchange took place between Kronecker and Cantor regarding the reality of the actual (as opposed to potential) infinite. Kronecker claimed that only the finite integers really exist and all else is merely the work of man. Cantor countered that the essence of mathematics was its freedom and that he had attained a larger vision than Kronecker had who could not see the infinite. Most mathematicians have followed Cantor and found his paradise a more beautiful and alluring universe. Hilbert accepted Kronecker's viewpoint for his metalanguage but tried to recapture Cantor's paradise in a formal language. Hilbert was a formal pluralist in feeling that each mathematical discipline was entitled to its own formalization. Russell was a logical monist and felt that all of mathematics should be constructed within a single formal system. He put a great deal of labor into his program and looked askance at Hilbert. He felt that Hilbert's approach had all the advantages of theft over honest toil. What he did not realize was that in intellectual affairs, as in economic affairs, great fortunes are rarely ever accumulated through honest toil. What is needed is the intellectual leap. Russel's program led to much interesting mathematics, but even if in principle it could be carried out, in practice the result would be computationally intractable. One would be translating simple, clear ideas into the fog of Principia Mathematica. Russell's program has as much relevance to

Dedicated to Reuben Hersh for his ninetieth birthday.

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complex analysis as von Neumann's game theory has to chess. The understanding and appreciation of mathematics has very little to do with formal logic. For example, the following footnote occurs at the beginning of Wall's (1970) book *Surgery On Compact Manifolds*.

Recent results of Kirby, Siebenmann, and Lees have now (1966) provided such a technique. All our methods now extend to the topological case, with only trivial alteration. See (K8), (K9), and (L10).

All the experts could see the truth of this footnote. But this seeing is not explained by modus ponens. In his beautiful book *Proofs and Refutations*, Lakatos (1976) has shown that the mathematical process itself is dialectical and not Euclidean. At all times our ideas are formally inconsistent. But inconsistency, while still recognized as a pathology, is no longer seen to be a fatal disease. If we come across a contradiction, we localize it, isolate it, and try to cure it. But we have to get over our neurotic phobias concerning this disease and recognize it as inseparable from life itself. Hilbert's program collapsed with the startling work of Godel. Mathematical logic and the study of formal systems have become a branch of mathematics instead of its foundation. Moreover, Cantor's paradise has been raised into the metalanguage in order to prove deep theorems concerning formal systems as well as to provide a semantics for such systems. A. Robinson even defined nonstandard formal systems which contain infinite formulas. One thus has a large plurality of different approaches to mathematics. Most mathematicians live in Cantor's paradise in spite of Russell's paradox; they simply learn to avoid making certain moves which have been shown to lead to contradictions.

The situation is similar in physics also. As soon as the phenomena under discussion become sufficiently complex, one must depart from Euclidean strategies and adopt a non-Euclidean approach. This is very clearly stated by Blandford and Thorne (1979, p. 454–460) in the context of black hole astrophysics:

> *The fundamental theory of black holes, as laid out in chapters 6 and 7, is well posed, elegant, clean, and self-contained. It follows inexorably and clearly from the fundamental laws of physics. The theory of black holes in an astrophysical environment is completely the opposite. Because it deals with the physics of matter in bulk-matter orbiting and accreting onto a hole- - it is subject to all the dirty, complex uncertainties of the modern theory of the behavior of bulk matter. If thunderstorms and tornados on Earth have eluded accurate theoretical modeling, how can one expect to predict even qualitatively their analogues in the turbulent, magnetized plasmas that accrete onto a black hole in a close binary system? One cannot. The best that can be hoped for is to develop the crudest of models as to the gross behavior of matter in the vicinities of holes. Fortunately, the resulting models have some modest hope of resembling reality. This is because the relative importance of physical processes near a hole can be characterized by dimensionless ratios that usually turn out to be very large, and consequently, the gross behavior of matter near a hole is dominated by a small number of processes. The task of the model builder is to identify the dominant processes in his given situation, and to construct approximate equations, describing their macroscopic effects. Historically, to identify the dominant processes has not been easy. This is because a vast number of possible processes must be considered and the model builder often, out of ignorance, overlooks an important one. Thus it is that research on black hole astrophysics involves large bodies of physical theory. Within each body of theory one must have at one's fingertips approximate formulae that characterize a long list of possibly relevant processes. The necessary bodies of theory include general relativity, the*

physics of equilibrium and non-equilibrium plasmas, the physics of radiative processes, and the physics of stellar dynamical systems. Research in black hole astrophysics also requires, a good knowledge of the phenomenology of modern astronomy – the observed properties of stars, the main features of their evolution, the structure of the Galaxy, and the observed physical conditions in interstellar space.

The above example illustrates the following features of research in black hole astrophysics:

(i) *It involves an iteration back and forth between the equations of the macroscopic model and the microscopic physics which underlies those equations. One iterates until one obtains self-consistency.*
(ii) *One must search carefully, at each iterative stage, for overlooked processes that might be so important as to invalidate the model (anchoring to a homogeneous interstellar magnetic field in the above example).*
(iii) *One frequently encounters a 'branch point' where the model will take on two very different forms depending on what one assumes for the environment around the hole (homogeneous magnetic field versus tangled field in the above example), and where both branches might well occur in the real universe. This leads to a plethora of possible models, each corresponding to a different black hole environment and/or range of black hole masses.*

Probably the clearest case study of non-Euclidean logic occurs in the paper *Problem Solving About Electrical Circuits* by Stallman and Sussman (1979, p.33–39). We quote from their introduction:

A major problem confronting builders of automatic problem-solving systems is that of the combinatorial explosion of search-spaces. One way to attack this problem is to build systems that effectively use the results of failures to reduce the search space that learn from their exploration of blind alleys. Another way is to represent the problems and their solutions in such a way that combinatorial searches are self-limiting. A second major problem is the difficulty of debugging programs containing large amounts of knowledge. The complexity of the interactions between the "chunks" of knowledge makes it difficult to ascertain what is to blame when a bug manifests itself. One approach to this problem is to build systems which remember and explain their reasoning. Such programs are more convincing when right, and easier to debug when wrong. ARS is an expert problem-solving system in which problem solving rules are represented as demons with multiple patterns of invocation monitoring and an associative data base. ARS performs all deductions in an antecedent manner, threading the deduced facts with justifications which mention the antecedent facts used and the rule of inference applied. These justifications are employed by ARS to determine the currently active data-base context for reasoning in hypothetical situations. Justifications are also used in the analysis of blind alleys to extract information which will limit future search. ARS supplies dependency - directed backtracking, a scheme which limits the search as follows: The system notes a contradiction when it attempts to solve an impossible algebraic relationship F, or when it discovers that a transistor's operating point is not within the possible range of its assumed region. The antecedents of the contradictory facts are scanned to find which nonlinear device state guesses (more generally, the backtrackable choicepoints) are relevant. ARS never tries that combination of guesses again. A short list of relevant choicepoints eliminates from consideration a large number of combinations of answers to all the other (irrelevant) choices. This is how the justifications (or dependency records) are used to extract and retain more information from each contradiction than a chronological backtracking system. A chronological backtracking system would often have to try many more combinations, each time wasting much labor rediscovering the original contradiction.

Note that this is a case where a Euclidean approach does exist but is computationally intractable. We thus have a clear example of a non-Euclidean approach being used for good reasons and not just because of sloppiness. We thus gain a better appreciation of the gulfs that separate the mathematician, the physicist, and the engineer. Their programs are different, their aesthetics are different, and even their "logics" are different. At this point, the question of whether every non-Euclidean logic can be embedded in a Euclidean logic can only be answered dogmatically. Those in artificial intelligence research assume a positive answer, while hermeneuticists assume a negative answer. However, even if you believe in an affirmative answer, a very simple non-Euclidean logic may only be embeddable in an immensely complex Euclidean logic, thus making its embeddability irrelevant from a practical point of view (recall von Neumann's analysis of chess).

Modern physics pervades nearly all aspects of modern biology. The striking difference between the explanations given by Aristotle and by modern biologists is partially explained by the fact that cells, molecules, atoms, electrons, and photons were not part of Aristotle's universe. Modern biology teaches us that we breathe in order that oxygen can be supplied to the cells of the body, so that they can metabolize food to obtain energy needed for the other activities of life. This helps us understand the structure and purpose of the lungs, red blood cells, etc. Much of this understanding is at the level of molecular structure (for instance, the structure of hemoglobin). We explain the workings of green plants by saying that they are green because of the presence of molecules of chlorophyll which are used to capture photons from the sun, thus providing the plant with its basic energy supply. The resemblance of children to their parents is explained by the DNA sequence in their chromosomes. Sight is explained by appeal to photons captured by the rods and cones in the retina. One could go on and on. Considering modern physics' pervasive role in modern biology, it is quite interesting to note how little most biologists know of the foundations of modern physics. Their ideas of photons, atoms, and molecules resemble the incoherent pictures formed by Einstein and Bohr during the period 1900–1925. Even more interesting is the fact (?) that their ignorance of the viewpoints of modern quantum mechanics does not seem to hinder their work at all. While they think of photons and electrons "classically," the success of their work is due to the fact that they only actually use atomic language in the way Ostwald recommended at the end of the nineteenth century. This can be easily seen in Watson's book ***The Molecular Biology of The Gene***. Let us reconsider our previous biological examples using correct quantum mechanical language. First of all, photons are not objects which could come from the sun. Instead, one assumes the existence of certain detectors called photon detectors which give positive readings under certain conditions such as being exposed to the sunlight. Next, one discovers that plants grow well not only in sunlight but also under many other conditions. A basic invariant of all these conditions is that under them our photon detector would yield positive results. Again, molecules aren't objects either. So plants aren't "made up" of molecules such as chlorophyll. Instead, another basic invariant of those conditions under which plants grow successfully and appear green is that a chlorophyll detector would yield a positive result. Children don't resemble their parents because of DNA sequences, but, instead, parents and their children's DNA sequences are highly correlated, and each is somewhat correlated to phenotypes. This is not the type of language usually used by biologists. But biologists like Watson aren't wholly to blame for their misuse of physical language. Watson gets his physics from Linus Pauling. In his book, ***The Nature of the Chemical Bond***, Pauling constantly mixes classical ontology with modern quantum mechanics. Consider, for example, the following quote (Pauling, 1960, p.19):

> *The electron distribution function for molecule-ion is shown in Figure 1-5. It is seen that the electron remains for most of the time in the small region just between the nuclei, only rarely getting on the far side of one of them; and we may feel that the presence of the electron between the two nuclei, where it can draw them together, provides some explanation of the stability of the bond.*

This picture resembles more the hidden variable views of Bohm and Nelson than it does the viewpoints of Bohr and Heisenberg. But the master knew better as is shown by the following quotes (Pauling, 1960, pp. 217–220):

> *It is true that chemists, after long experience in the use of classical structure theory, have come to talk about, and probably to think about, the carbon-carbon double bond and other structural units of the theory as though they were real. Reflection leads us to recognize, however, that they are not real, but a rhetorical constructs in the same way as the individual Kekule structures for benzene. It is not possible to isolate a carbon-carbon bond and to subject it to experimental investigation. There is, indeed, no rigorous definition of the carbon-carbon double bond. We cannot accept, as a rigorous definition, the statement that the carbon-carbon double bond is a bond between two carbon atoms that involves four electrons, because there is no experimental method of determining precisely the number of electrons that are involved in the interaction of two carbon atoms in a molecule, and, of course, this interaction has rigorously to be considered as being dependent on the nature of the entire molecule. I feel that the greatest advantage of the theory of resonance, as compared with other ways (such as the molecular-orbital method) of discussing the structure is that it makes use of structural elements with which the chemist is familiar. The theory should not be assessed as inadequate because of its occasional unskillful application. It becomes more and more powerful, just as does classical structure theory, as the chemist develops a better and better chemical intuition about it. The theory of resonance should not be identified with the valence-bond method of making approximate quantum-mechanical calculations of molecular wave functions and properties. The theory of resonance is essentially a chemical theory (an empirical theory, obtained largely by induction from the results of chemical experiments). Classical structure theory was developed purely from chemical facts, without any help from physics. The theory of resonance was also well on its way toward formulation before quantum mechanics was discovered. The theory of resonance in chemistry is an essentially qualitative theory, which, like the classical structure theory, depends for its successful application largely upon a chemical feeling that it developed through* practice. *We may believe the theoretical physicist who tells us that all the properties of substances should be calculable by known methods– the solution of the Schrodinger equation. In fact, however, we have seen that during the 35 years since the Schrodinger equation was discovered only a few accurate nonempirical quantum mechanical calculations of the properties of substances in which the chemist is interested have been made. The chemist must still rely upon experiment for most of his information about the properties of substances. Experience has shown that he can be immensely helped by the use of the simple chemical structure theory. The theory of resonance is a part of the chemical structure theory, which has an essentially empirical (inductive) basis; it is not just a branch of quantum mechanics.*

Thus, no matter what the future may bring, chemistry is at present an independent science only slightly dependent upon physics and much dependent upon chemical intuition. The situation is similar for biology. Watson goes out of his way to insist that biology requires no "natural laws" not already found by chemistry and physics. He is opposing vitalists such as Bergson and the expectations of physicists such as Bohr and Schrodinger. Bohr hypothesizes that biology would yield a biological complementarity principle where the description of a living organism would be complementary to a description of its molecular structure. Such a position may yet come to pass when biology becomes sufficiently precise to become conscious of its "complementarities."

Conclusions

One of our most powerful myths is the story of the tower of Babel where by an original unity was shattered into diversity. Much of our intellectual history consists of attempts to find common grounds upon which to erect unified sciences. Many bewail the unsatisfactory situation in the social sciences, humanities, and theology where many schools compete for dominance. This situation contrasts with the seeming harmony of the natural sciences. The claim we have made here is that the relative lack of polemics in the hard sciences is not due to a consensus concerning fundamental theoretical structures but is instead a much more complicated sociological fact. For instance, a close study of the various electron models reveals tremendous differences. The differences between the Schrodinger, Dirac, and Feynman theories of an electron appear to us as huge as the differences between the Freudian, Skinnerian, and Piagetian theories of human behavior. But physicists don't mind the diversity. They take an eclectic approach using whichever model seems most appropriate under the circumstances. By contrast, many psychologists feel that the alternative approaches to human behavior are competing theories, and only one of them will eventually prevail. In studying complex phenomena, there are always a variety of possible approaches. Too much of an insistence on consensus results in very impoverished starting points such as those taken by Russell and Carnap. From such starting points, it is very difficult to get anywhere. But the other extreme often results in a tower of Babel where each scientist has not only his own theories but his own scientific methodology and logic. To paraphrase Goethe: one can convince oneself while viewing a great collection of scientific works that nearly each master had a different way of approaching nature. In our opinion, what is required in science is the same thing that is required in art, namely, "taste" and "judgment." To say this, however, is not to opt for an "anything goes" relativism or radical idealism. On the contrary, we have argued that toleration is necessary in order to save realism and thus avoid such radical solutions. To the extent that agreement can be reached about goals, there are clear and demonstrable relative advantages between various positions. The determination of these advantages, though, may not be something that is clearly formalizable. On the other hand, in those cases where there is no agreement about goals, we should not expect consensus. In fact, our point has been that in these cases, the attempt to enforce consensus carries grave risks.

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Xenomath!

Ian Stewart

Mathematics is a collection of practices and concepts of human beings.

Reuben Hersh [12]

What makes you think that the aliens will recognize your mathematics?

Might they not have an entirely different mode of thought?

Jack Cohen, quoted in [17]

Reuben Hersh has argued, persuasively, that mathematics is not a collection of eternal truths existing in some ideal but nebulous world—the Platonist viewpoint—but is instead a shared human mental construct [11]. It seems difficult to maintain that mathematics is *not* a shared human mental construct, since it has been developed by mathematicians communicating their ideas to each other, but Platonism lingers on. The suggestion that mathematics is dependent on human conventions has proved unpopular in some circles, possibly because it appears to smack of relativism, whose more extreme form maintains that the whole of science is merely what scientists choose to believe.

Of course, Reuben's position implies nothing of the kind. The shared mental construct that we call mathematics is by no means arbitrary. Nothing new is incorporated into it unless it passes stringent reality checks—namely, logical consistency with the existing body of mathematics, supported by proof. For similar reasons, science is not merely a belief system; its reality checks involve agreement with experiment and observations and the logical consistency of its theories, modulo the willingness of physicists and others to accept evidence short of rigorous proof

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if it's sufficiently convincing. *Denial* of Reuben's view is thus much closer to relativism than acceptance of it.

However, Platonism is seductive, because that's what it *feels like* when we create new mathematics. We don't get to choose whether Fermat's Last Theorem is true or false: we assume, ignoring Gödel's theorem or hoping it's irrelevant, that it's either one or the other. Then we (I speak loosely: Andrew Wiles [24, 28] did the actual work) struggle to find out which of those possibilities is the case. For Fermat's Last, Gödel did turn out to be irrelevant, and the theorem turned out to be true. During the development of mathematics, other equally plausible conjectures have turned out to be false, and many—notably the Riemann Hypothesis—are still stuck in limbo awaiting solution. Platonism is the *quale* [14, 27] of problem-solving, the vivid impression that the answer is *already out there* and we're just 'discovering' what it is.

In a sense, that's true, if by 'there' we mean the network of all logical inferences that are correct consequences of whichever axiom system for mathematics we happen to prefer. I'll call this the Platonic landscape, because its geography is completely predetermined by the axiom system concerned. It is again a shared human construct, but unlike the existing body of mathematics, the part that we *haven't* sorted out exists only potentially. The known Platonic landscape expands as humans explore it, and the geography of its unexplored regions is a mystery. That's why knowing that it's predetermined by the axioms doesn't help us to answer any specific questions about it.

In this essay, I'll try to get some insight into the contrast between an ideal Platonic world and the actual human construct, by focusing not on the mathematics, or the logic, or even 'shared', 'mental', and 'construct', but on the word 'human'. Even mathematicians who agree with Reuben tend to think that mathematics is universal, in the sense that if we eventually encountered extelligent[1] aliens, their mathematics would inevitably have a lot in common with ours, and the union of the two would necessarily be consistent. That is, we and our alien friends (let's hope they're friends, though I do wonder...) have been exploring different but overlapping regions of the *same* Platonic landscape.

Our symbols and notation would of course differ. Aliens need not work in base ten; they might think that the circumference of a circle has angular measure 1 rather than 2π (a sensible enough suggestion)... but they'll understand such things as the diagram for Pythagoras and the list of primes. Though they might insist that 1 is prime, as we ourselves did until recently. Distinctions like that don't imply that Euler was an alien. In short, many of us assume that

$$\text{alien math} \equiv \text{human math} \pmod{\text{notation}}$$

[1] *Extelligence* [5, 20] is the extension of individual intelligence to an entire culture, as a reservoir of knowledge accessible by all. Writing, books, and the Internet are examples of human extelligence.

This assumption of the universality of mathematics underpins virtually all suggestions for communication with aliens, and it's embodied in the attempts we have so far made to do just that—the Arecibo message, the Pioneer plaque, and so on.

I want to play alien's advocate and question this assumption. If I'm right (though I make no such claim), the alleged universality of mathematics is called into question, and Reuben is spot-on when he states that it is a *human* mental construct. There could, I suggest, be similar alien mental constructs that are so different from ours that even when they're exploring the same territory, they map it in ways that aren't meaningfully equivalent to ours.

The thing about aliens is: they're alien.

Alien Means Alien

'Astrobiology' is standard term for the study of hypothetical alien life, but Cohen and I [5] prefer the broader term 'xenoscience'. It is, of course, one of those dreadful Graeco-Latin hybrids like 'television'. Tough. You can call it 'xenology' if you prefer. 'Xenoscience' is an open-ended description, the science of the strange. 'Astrobiology' is nowhere near as open: it's a fusion of two existing sciences, astronomy and biology, which until recently have developed virtually independently of each other. Astronomers, to be fair, study alien worlds—that is, worlds differing radically from our own, not worlds inhabited by aliens. But biologists study life as it happens to exist on this planet.

In any other context, this would be sensible and reasonable. Earthly life is the only life that we know, and also the only life that's accessible. Indeed, a lot of it isn't even that: the word 'accessible' hardly applies to the depths of the oceanic trenches, but there are a lot of interesting species down there. That said, assuming that alien life necessarily resembles Earthly life begs the question [4]. Aliens are alien, and we should ask: *how* alien? Must they be like us? If not, what could they be like instead?

The science of aliens is unusual in that its *raison d'être* has never been observed. The only life forms we currently know about are down here on planet Earth. So xenoscience is the study of things that are not known to exist. It can be argued that this disqualifies it from being science, but many areas currently accepted as science suffer from the same problem: superstrings, dark matter, cosmological inflation, etc. We don't have direct experience of the core of the sun, but astrophysics is a perfectly reasonable science because we can infer what the sun's interior is like from things of which we do have experience. Xenoscience is more hypothetical, but we can infer a lot about the *possibilities* for alien life from solid science carried out on this planet. And ultimately science is about inference, not about direct experience.

Biology teaches us that certain features (carbon, water, organic molecules, proteins, DNA, etc.) are sufficient for life to evolve, given time and a habitat that suits organisms made of that kind of stuff, but it doesn't, and can't, teach us that these conditions are necessary. Only if all life must be much like life on Earth can we argue that alien life must evolve in a habitat similar to ours. Experiments show that pretty much every feature of our DNA/RNA/protein biochemistry is fungible

[1, 3, 19]. Getting rid of carbon is trickier, but silicon can form the backbone of complex molecules with the assistance of the odd metal atom. With less empirical support, we can imagine 'life' made of silicon microcircuit flakes, or as balloonists floating in the atmosphere of a gas giant. What price magnetic field life forms in the photosphere of a star, or quantum entities in the intergalactic vacuum? Agreed, being able to imagine something doesn't prove it exists; but then, lacking the imagination to consider it doesn't prove it *doesn't* exist.

In any case, 'life on Earth' is considerably more varied than it appears from our own comfortable habitat. Creatures happily exist in conditions that would kill humans. Underwater, where we cannot breathe. At the bottom of ocean trenches, where even if we could breathe, the pressure would crush us. In the boiling water of volcanic springs. In cracks in the rocks several kilometres down. Biologists discovered many of these organisms fairly recently, overturning many cherished assumptions, and named them 'extremophiles'. The name may seem to fit, because they live in extreme conditions. However, it reveals a common default assumption, which is best avoided when contemplating aliens. Those conditions are extreme *to us*. They are not extreme to the organisms that live there. Those organisms would die if they were brought into our living rooms. To them, we are the extremophiles [4, 21]. In fact, it's not terribly sensible to use the same word for creatures that survive in very high temperatures and others that survive in very low ones. They exploit different evolutionary tricks, so lumping them together as being comparably weird betrays a casual parochialism. It leads us to assume that there's something unusual about creatures that differ from us, as though they're desperately clinging on to life in totally unsuitable habitats. No, they're perfectly comfortable; their lifestyle works fine for them. By ignoring this, many people tacitly assume that aliens must be just like *them*, because it's never occurred to them that any other form of intelligent life might be possible.

Cultural Influences

Even if mathematics truly is universal, different cultures may have very different views about what's *important*. They can look at the same landscape, see the same things, yet have different opinions about which features matter. We see this every day in discussions with colleagues. For much of the 20th century, the Pure Mathematics and Applied Mathematics Departments considered each other to be a hotbed of misguided idiots who didn't have a clue. The overlap of topics and methods was vast; the common ground (Pythagoras, primes, calculus, etc.) was immense, but the way of thinking was — well, alien. These and similar attitudes still persist to some extent, but there's been a broad *rapprochement* and a realisation that two heads are better than one. That said, there are algebraic geometers who are absolutely certain that p-adic cohomology is the *only* worthwhile topic in the whole of mathematics, and anyone who is interested in, say, chaotic dynamics, is wasting their time on pointless frivolities.

Less contentiously, the research priorities in, say, a mainly agricultural nation, are likely to be different from those in a country dominated by the financial sector and different again from those in one that relies on tourism. As it happens, chaos theory is currently more likely to interest those nations than p-adic cohomology, but I'm not pressing that point because next week someone will discover an amazingly effective way to analyse plant genetics, or to forecast stock market fluctuations, or to improve the efficiency of hotel restaurants, using p-adic cohomology. If you think I'm joking, look up the uses of persistent homology in security systems [6, 7, 8].

Diverse though these priorities may be, they all take place within the same overall body of mathematics. If the same goes for aliens, then although the Quoich caretakers of Snickelbah IV might be obsessed with the application of inverse gravitonics to quergedulid-skyfishing, they'd still be working within the same mathematical framework as us. Modulo notation, terminology, and language. We could work together, with enough effort. After all, the Pure and Applied species have managed it.

Newton's Ear

Other differences might run deeper.

It's difficult for today's mathematicians, brought up on deep abstractions such as the ZFC axioms for set theory, to recognise just how firmly their subject rests on human perceptions and conventions. It all seems so natural, so inevitable, so logically pure, so untainted by everyday life.

Cohen and I once thought of writing a book exploring the contrary view, called *Newton's Ear*. The underlying conceit (in the Victorian sense) was that when Isaac Newton wrote down his law of motion $F = ma$, it became interpreted as a mathematical explanation of why and how forces cause objects to move. Our point (inasmuch as we had one) was that Newton's personal experience of forces came from his inner ear, where hairs connected to nerve cells were deformed by *acceleration*, and his brain interpreted this as feeling a force. In short, Newton derived his law of motion from what was going on inside his own ears—which was his law of motion.

Similarly, our visual senses present the world to us as a two-dimensional projection (with 3D add-ons from stereoscopic vision), so it was natural for Euclid to do geometry on a plane. His basic concepts of lines and points correspond roughly to the first few stages of image detection by the visual cortex. Congruent triangles are a formal approach to intuitive assumptions related to the movement of objects in the natural world, reverse-engineered in modern times as transformation geometry. The Euclidean group is built into how we perceive the world around us.

Numbers reflect the tendency of the human environment to contain collections of discrete objects, ranging from a pile of stones to a herd of sheep, and, of course, the family members with whom ancient hominins shared their caves. Counting is abstracted from the human process of pointing at things with a finger and chanting 'ug, ug-ug, ug-ug-ug ...' or whatever. Our mathematical terminology contains a great deal of evidence for this process; we speak of numerical *digits* (fingers, on which we counted), of *geometry* (Earth measurement), of *squares* and *cubes* in algebra, of *knots* in topology, of *maps* and *transformations*, and of *groups* and *rings* and *fields*. A *vector* (Latin for carrier) lives in a *space*. A *radius* is a spoke in a chariot wheel. *Matrix* originally meant 'womb', hence a cavity in which things form, hence a framework in which things can be embedded and arranged.

Our favourite coordinate systems reflect our body plan and our egocentric view of the world. We stand upright (up/down), our arms extend sideways (left/right), and our eyes look in one direction and leave us to worry perpetually about what might be sneaking up on us from the opposite one (front/back). That's 3D Cartesian coordinate axes—with us at the origin, the centre of all things, where of course we rightly belong. This bodily coordinate frame arises in the early stages of embryonic development as cells divide and morphogens morph. Polar coordinates are basically us at the centre, spinning on the spot (θ) as we try to make sure a leopard isn't sneaking up on us, and if we see one, estimating how far away it is (r).

Our hardwired binary logic, now seen as the basis of and prerequisite for mathematics, hints at lengthy evolutionary pressures requiring rapid yes/no decisions. Fight or flight? Attack or retreat? Friend or foe? The brain is now seen by many psychologists as a Bayesian decision machine [10], and a decision ultimately is a binary choice, though one arrived at by structures that are more flexible. Logicians and others have invented numerous alternatives to binary yes/no 0/1 logic—three-valued logics, intuitionist logic, fuzzy logic, yet mathematicians gravitate, entirely predictably, back to 0/1 logic. We *like* dualities. Perhaps that's because we're a bisexual species; perhaps that explanation is facile.

I'm not suggesting there's anything wrong with any of this. The history of mathematics is a tangled tale of concepts being imported from the outside world, reworked by a human mind, and exported back. We get many of our best ideas that way. We get more by internally mulling over potential logical connections and letting one thought strike sparks off another. Again, our metaphors (*strike*, *spark*) reveal how it all originates in our humanity. Even the Platonic image of ideal truths 'out there' is that of a wary ape peering out at a vast external universe through two holes in its skull—yet also having the imagination to wonder what *else* might be out there, as yet undetected.

Contact

The question is whether any of this, even if it's true, matters. It might just be a description of what it's like for limited humans to open up a window on the vast Platonic landscape. If there's only one Platonic landscape, any entity that opens a window will see much the same things, and an alien window would reveal similar vistas to a terrestrial one—at least, if they're looking in the same direction. So what's important would be what's *there*, not how you find out about it.

If that's right, then mathematics is universal. $2 + 2$ isn't just 4 here, it's also 4 on exoplanet Snickelbah IV. (What else could it be? Wrong question. See below.) Therefore alien math would be close enough to our own for both sides to understand how they relate. Not perfectly, not in full detail, but in broad terms. In particular, it would then make sense to use math as a kind of universal language when attempting to contact intelligent aliens. Rather than, say, sending them the collected works of Shakespeare.

Carl Friedrich Gauss had an interest in alien life and is often credited (probably incorrectly) with the proposal to initiate contact by inscribing the diagram for Pythagoras in the Siberian tundra [27]. A passing alien would spot the diagram, mutter something along the lines of 'By the exoskeletons of my ancestral hivemates, the creatures on this world have discovered Smuffznoff's Theorem! They must be extelligent!' and then transmit the first 7^7 primes in acknowledgement. Humans would quickly spot base 7 notation, decipher the pattern, and contact would be established. Soon we'd be exchanging family videos.

It's a reasonable point of view, especially if the aliens are much like us. But what if they aren't? Or if they *are* like us, but come at things from a totally different angle? The issue isn't (probably) whether intelligent aliens would think that $2 + 2$ is 5. It's whether they would necessarily understand 2, 4, or $+$. Perhaps aliens whose environment and social life differ from ours might come up with different mathematics—or even no mathematics at all. They could, perhaps, have advanced technology with a purely empirical basis, or have a far better intuition for the workings of the universe than we do. Consider how deep neural networks currently solve difficult problems by methods totally alien to human intelligence [18]. But let's settle for different mathematics. I quote from [20]:

> Imagine creatures that float in the atmosphere of a gas giant, like Jupiter, studying the geometry of triangles by floating small objects in the air. 'Consider a triangle ABC—oops, where's A gone?' Aliens like that would probably have a fantastic intuition for the fluid dynamics of turbulent vortices, which we find almost impossible to understand. But our rigid geometry might seem very alien indeed to them. Counting things doesn't come naturally when everything including the things themselves is fluid. For Jovians, whole numbers might be very sophisticated concepts, characterising the topology of vortices. But sometimes two vortices merge to form a single one, so the Jovians might decide that $1+1$ sometimes equals 1, not 2.
>
> Alien mathematics would probably be logically consistent with ours. But it might carve up the universe in such a different way that its relation to our maths would be incomprehensible. Even on Earth, concepts often don't translate successfully from one language to another. As an American General once said: 'You can't trust those Russians. They have no word for *détente*.'

But surely, as long as aliens do have some form of math, then even if they approach it from a different background, with different emphasis, experiences, even concepts, they can still be led to understand what we mean by $2 + 2 = 4$, or π, or p-adic cohomology, and could then incorporate it into *their* math? And we might take inspiration from quergedulid-skyfishing and finally resolve the status of the Riemann Hypothesis. Or at least publish a small comment improving the exponent in the error term in an asymptotic formula for some obscure number-theoretic function from 3/7 to 42/123.

Pluralism in Languages

We like to imagine this must be so. But our framework for mathematics has evolved in a specific manner, over thousands of years, with every step governed by human cultural prejudices and default assumptions. Even if we're staring at the *same* Platonic landscape as the Quoich caretakers, we may carve it up very differently.

Reuben makes essentially the same point when discussing pluralism in the philosophy of mathematics [13], in particular comparing Platonism and anti-Platonism. Instead of arguing that one stance must be right and the other wrong, why not adopt a pluralistic approach? Both have advantages and disadvantages; as Reuben said, 'The typical mathematician is a Platonist during the week and a formalist on Sundays.' In [13] he says that initially he thought this was a bad idea, but 'now I see a way to make it OK. Just call it pluralism!'

Pluralism offers distinct advantages, even in conventional mathematical thinking. The classic example is coordinate geometry. On the one hand, Euclid; on the other, equations. The link is in principle exact: every geometric object has its algebraic expression; every formula defines something in the geometry. In principle, every theorem that can be proved using one viewpoint—one language—can be proved in the other. It's even possible to translate between them in a systematic manner.

However, this link failed to render either Euclid or algebra obsolete, even though we can prove that either is superfluous, given the other. Why? Because what's natural in geometry and what's natural in algebra are very different beasts. Each language has its strengths and its weaknesses. Some problems are easy in one language, baffling in the other. Short statements in one sometimes translate into long, cumbersome, or impenetrable statements in the other. The two views are compatible in principle but often alien to each other in practice. And that's in our *own* mathematics. Another example is Newton's use of geometry, not calculus, in the *Principia*. It's easy to come up with more.

The *existence* of a translation algorithm, then, isn't the only vital point. The tractability of the algorithm—the computational cost, in number of operations, of implementing it—is paramount. For an explicit example, let's think about two languages describing the natural numbers. The first, L_1, is ordinary decimal representation. In this language, standard arithmetical operations such as addition and multiplication can be carried out by polynomial-time algorithms and hence are computationally tractable. An alternative language L_2 is the factorisation of a natural number into primes:

$$n = p_1^{a_1} \dots p_r^{a_r}$$

Here n is represented by the sequence $(a_1,...,a_r)$. For instance,

$$2001 = 3.23.29 = (0, 1, 0, 0, 0, 0, 0, 0, 1, 1)\,.$$

In L_2, multiplication is a tractable operation but addition is not. At least, the obvious way of adding two numbers—convert to L_1, add, translate back into L_2—is intractable if the standard conjecture that there's no fast algorithm for prime factorisation is correct.

The situation here is asymmetric: translation $L_2 \to L_1$ is tractable, but (conjecturally) $L_1 \to L_2$ isn't. Already we see that the argument 'L_1 is logically equivalent to L_2, therefore nothing new can be stated in L_2' is wrong. There must exist short statements in L_2 that become intractably long when restated in L_1. The translation map is a 'trapdoor code', efficiently computable in one direction but uncomputable in the reverse direction [9]. However, worse is possible: both directions may be uncomputable. For instance, we could replace L_1 by a new language L_3 closely related to L_2. Suppose that the primes, in numerical order, are $p_1, p_2, p_3, \ldots$. Define $\sigma: \mathbf{N} \to \mathbf{N}$ by

$$\sigma\,(2j) = 2j-1 \qquad \sigma\,(2j-1) = 2j$$

and observe that $\sigma^2 = \text{id}$. Define $\psi: \mathbf{N}\backslash\{0\} \to \mathbf{N}\backslash\{0\}$ by

$$\psi\left(\prod p_i^{a_i}\right) = \prod p_{\sigma(i)}^{a_i}$$

so that $\psi^2 = \text{id}$. That is, swap primes in pairs. Language L_3 defines a natural number n by the sequence (a_i) for which

$$n = \prod p_{\sigma(i)}^{a_i}$$

Now $\psi : L_2 \to L_3$ and the same map $\psi : L_3 \to L_2$. The obvious algorithm for computing $\psi(n)$ requires some tractable computations and two conjecturally intractable steps:

- Factorise n into primes.
- For each p_j in this factorisation, determine whether j is even or odd.

How do pluralism and artificial languages for arithmetic relate to xenomath? Well, we work in L_1, but suppose the Quoich worked in L_3.... We'd agree on everything, but be unable to express any common ground.

It Gets Worse

Perhaps we and the Quoich *aren't* looking at the same Platonic landscape. Maybe they're photonic or quantum, and we're not on the same wavelength. Or maybe we just set everything up differently. Then it gets *really* pluralistic.

I mentioned ZFC—Zermelo-Fraenkel axiomatic set theory augmented by the axiom of choice. As far as most mathematicians are concerned, this is today's gold standard as an axiomatic basis for mathematics. Taylor and Wagon [23] expound and reprove two theorems by Mycielski and Sierpiński that are relevant to alien math; for brevity I'll focus on that of Mycielski [15, 16].

The axioms of ZF are technical but mostly plausible. The axiom of choice C is more contentious: recall that it asserts that given a collection of sets, it's possible to choose a single element from each set [29]. Axiom C becomes contentious only when the collection is infinite. It's very useful—for example, it proves that a Cartesian product of compact topological spaces is compact [25]—but it also leads to some strange conclusions. One of the weirdest is the Banach-Tarski Paradox [2]: a solid sphere can be cut into finitely many pieces, which can be reassembled, via rigid motions, to form two solid spheres the same size as the original.

This would be contradictory if the pieces were measurable, but they're not. Most mathematicians are happy with this resolution and understand how the result arises from reasonable combinatorial properties of words in the group $\mathbf{SO}(3)$. However, if you dislike Banach-Tarski, you can get rid of it. You have to get rid of the axiom of choice, but having done so, you could, for example, add the seductively tidy axiom LM: *every set of reals is Lebesgue measurable.* Clearly the Banach-Tarski Paradox then becomes untenable. But, as Mycielski proved, this doesn't put an end to your difficulties, because ZF + LM implies an arguably worse paradox, the Division Paradox. Consider the additive group of reals $\mathbf{R}$ and its subgroup of rationals $\mathbf{Q}$. Then in ZF + LM, the cardinality of $\mathbf{R}$ is *less* than that of the quotient group $\mathbf{R}/\mathbf{Q}$.

If that doesn't bother you, it says that the set $\mathbf{R}$ can be partitioned into disjoint non-empty subsets, in such a way that the number of subsets is greater than the number of points.

Nonsense! Just choose a point in each subset... oops, that uses the axiom of choice. Oh.

Why am I mentioning this?

Well, if we use ZFC but the Quoich use ZF + LM, we'll be in fundamental disagreement. We think the Banach-Tarski Paradox is true; they think it's false. We think the Division Paradox is false; they think it's true. Now imagine a kind of fractal structure to alien logic compared to ours, with this kind of difference of opinion at every level. There would be absolutely no common ground, yet both would 'work' for their practitioners.

So, if the two civilisations came into contact, there might be misunderstandings...

The Unfortunate Incident at Shool-11

The following documents have been extracted from *Report on the Unfortunate Incident at Shool-11*[2] *and Binding Advice on Avoidance of Any Repetition* [henceforth *Unfortunate Incident*] by magnetotorus herder Concurrence-of-Opinion. The

[2]Unless otherwise indicated, all numbers in this document are expressed in Galactic Standard heptadic notation.

originals are of many different mutually incompatible formats, including (but not limited to) Quoich gesture concertos, pressed vegetation, sequential binary, Stormcatcher vortex flows, Gigantomorph incised osmium monoliths, nudged quanta, and plasmoid flare modulates. They have been transmogrified into discrete-symbolic format, but the individual or hive-mind reader should be warned that fine nuances may have been lost in transmogrification. Quantum beings should place themselves in single-world mode and accept the massive diminution in channel capacity.

1. *Executive Projection* concatenated by Implement-of-Consensus [opening summand]

Recipients hardly need reminding of the *Unfortunate Incident*, which at one point threatened to escalate and cause the devastation of two entire Galactic sectors and parts of three others. Executive action was therefore taken by the Council for the Enforcement of Arbitrary Protocols to eliminate the threat posed by Shool-11, in accordance with Consensual Statute 4116-22B Addendum 4.

Post-execution analysis by qualified effectuators revealed the whole thing to have been the tragic consequence of a misunderstanding [see **Mathematics**].

2. *Extract from Galakipedia* [edited]

   Shool-11 [*aka* Poisonblue, Sol-3[3], Terra, Earth, Monde, *and 11,4366 other local designations]*

Before its recent dissolution,

this world was placed on the Indicted List

due to suspected mesophile infestation

For many heptoheptades, Shool-11 was reasonably considered uninhabitable. Its surface temperature was intermediate, lying neither in the cryozone nor the plasmatic region. The presence of large quantities of oxygen (mainly ^{22}O, with traces of ^{23}O and ^{24}O), a volatile substance conducive to spontaneous combustion, was traced to the ubiquity of oxygen dihydride, familiar in the cryozone as ‘rock’, dissociated into nuclei in all plasmatic habitats but rare in its liquid or vaporous phases—except on Shool-11.

In 3,4201 GTL, a few maverick scholars cursed with overwrought imaginations, who referred to their brainmeld as Open Minds Must Prevail, foolishly proposed that Shool-11 might be the habitat of *mesophiles*: hypothetical beings able to exist in the

[3]By the strange native counting system, which worked from the star *outwards*.

middle range of temperatures. This wild conjecture was based on the flimsiest of evidence [see **Shool Enigma**], and was dismissed as wishful thinking by all rational commentators.

Post-purification analysis by qualified effectuators of what turned out to be Shool-11 artefacts has now shown that this hypothesis was correct, a development that could not have been anticipated at the time.

3. *Extract from Galakipedia* [edited][4]
   Mathematics

Religious [**?**] belief system of some Shool-11 mesophiles comprising a loose assemblage of assertions and deductions derived from various arbitrary assumptions [belated summand axiom] by means of a binary deductive system peculiar to Shool-11 [belated summand logic]. Among the more absurd beliefs is the unexamined presumption that this system is universal and would automatically be shared by all extelligent cultural entity/systems.

As events unfolded, this presumption was responsible for the *Unfortunate Incident*.

4. [Included under the Indeterminate Access Agreement of 2,3301 GTL]
   Open Minds Must Prevail, Unorthodoxy Institute, Dome B4, CryoCentral. Tentative evidence for the presence of unengineered mesophiles, some of rudimentary extelligence, on an obscure world in the uninhabitable zone, *Psience* **2,4431** (3,4201 GTL); GOI: 44553-2264100300055.

   Abstract
   A *mesophile* is a hitherto hypothetical entity of phenotype so unorthodox that it can survive in the intermediate temperature shadow between the Cryonic Zone and the Plasmatic Region. Tentative evidence is provided that Shool-11 may be infested with mesophiles. This possibility was discovered during a routine survey of the Shool System. The cryoid survey vessel *Discouragement of Wasteful Overtones* made planetfall on Shool-1, a promising world with a calm, balmy climate, possibly suitable for colonisation. Strange markings observed in the solid nitrogen of Shool-1's surface led the sondage team to dispatch probes to Shool-11, the presumed source of these markings.
   At the cost of several ruined probes, it was confirmed that this bizarre planet possesses tetrahydrogen carbide, though in a useless gaseous form and in trace quantities. Its oceans are molten rock: specifically, oxygen dihydride in its dangerous liquid form. Despite strong evidence of past oxidation events on a vast scale, there remains *free oxygen.* Why has such a corrosive substance not combined with other elements long ago? We propose a tentative explanation: the planet has evolved mesophiles, able to exist under such conditions, which regenerate the gas by dissociating rock using stellar energy. We suggest that these organisms might largely *consist* of molten rock.
   An enigmatic burst of electromagnetic noise, emanating from Shool-11, is advanced as further confirmation of this hypothesis.

[4]After the disassembly, nanobots inactivated, fragmentary records supporting this entry were found in the wreckage of what is now recognised as Shool-11's proto-civilisation.

5. Extract from Galakipedia [edited]
   Shool Enigma
   [see Arecibo Message]
   This article is a stub. You can help Galakipedia by expanding it.

6. Extract from Galakipedia [edited]
   Arecibo Message

Poisonbluvian[5] term for the Shool Enigma**:** random electromagnetic noise alleged by convicted and executed criminal(s) Open Minds Must Prevail to be a signal from an unknown extraconsensual species inhabiting the world Poisonblue [*aka* Shool-11]. Why extelligent beings would fail to use an ansible was not explained. The sole evidence for this emission being a signal was that according to the ancient gematrophily of T'qac, the noise decomposes into 4616 shorter bursts.[6]

For many hectades, the significance of this quantity—if such it is—was hotly debated. The Orbital Tautology Engine at Wormhole 23 asserted that any waveform necessarily decomposes into one or more continuous segments and offered the unsolicited opinion that any finite signal must necessarily contain *some* number of discrete bursts, so the number concerned probably has no special significance.

A suggestion that 4616 represents a grid of 32 rows and 133 columns, or perhaps 133 rows and 32 columns, was widely discounted because the quantity of information that can be conveyed by such a grid is too trivial even to include the obligatory Litany of Respectful Obeisance.

Then came the breakthrough. In the Descenter religion of the Quoich of Snickelbah IV, a sequence of 4620 notes, performed on the snout-horn, is deeply sacred. The Descenters immediately presumed that one note had been omitted, and raised an Accusation of Disrespect with the Council for the Enforcement of Arbitrary Protocols, directed at all hypothetical Poisonbluvian inhabitants, on the grounds that this omission constituted a Vile Act of Disrespect and Heresy.[7] The Descenters demanded the traditional penalty, and the Council initiated proceedings. In the absence of any Poisonbluvian at the trial, despite being invited repeatedly by ansible to teleport a legal representative, it was presumed no extelligent inhabitants existed. Since this presumption was unverified, however, the decision to take the usual precautions was unanimous.

[5] **Poisonbluvian** refers to Shool-11's hypothetical mesophiles, who we now know called themselves *humans* in one of their languages. The term **Arecibo message** was discovered among the remnant artefacts of this ill-fated pre-sentient species and clearly describes the Shool Enigma.

[6] In Galactic Standard heptadic notation for discrete assemblages satisfying the Law of Conservation of Things. Flux-entities should (and in any case will) consider such an invariance property to be meaningless. Quantum beings can, with negligible loss of extelligibility, identify it with multisoliton conservation modulo phase.

[7] Technical point of Xenoecclesiastical Law: Shool-11 is clearly visible from the east pole of Snickelbah IV; thus this omission infringes the Obscure Commandments of the Prophet Brunk-Ploth.

10. *The Purification of Shool-11* by Hauptswarmführer Doomghast, Slayer-of-Animals

This record is protected by a synclastic inhibition field. Entities entitled to access it will know the requisite protocols. Unauthorised entities should retreat to a distance of at least one heptoheptadth of a mean galactic radius.

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Cognitive Networks: Brains, Internet, and Civilizations

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Introduction

In several recent research papers and surveys by neuroscientists (cf. [1, 29], and references therein), it was suggested that cognitive functions of the brain are performed using not only, and perhaps even not mainly, complex networks of interacting neurons (*connectionist view*) but also on the level of individual, highly specialized neurons and their *intracellular* mechanisms. This argumentation went hand in hand with the critique of popular analogies between brains and computers, where neurons were supposed to work as, say, electronic logic gates.

In order to retain the heuristic power of computer science in cognitive neurobiology and simultaneously to keep the door open to such paradigm extension, we consider in this paper possible analogies between the brain and Internet, in which certain neurons and some specific neural networks are being compared with entire computers, in particular, with servers, that in fact do have a very rich internal hardware and software reflected in their functions in the net.

As so many basic ideas and technologies of the information age, the future role of the World Wide Web was presciently understood by Alan Turing. Although the Internet of course did not yet exist then, according to a convincing interpretation by

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B. Jack Copeland ([4, 5], p. 30), Turing's definition and study of oracle machines in his PhD thesis (1938) introduced the notion that computability may involve getting "oracular" data from outside computers.

In this essay, we do not discuss any philosophical problems related to such comparisons (for possibly related discussions, see, e.g., [3, 14], and references therein).

We simply try to suggest plausible and verifiable conjectures about functions, interconnections, and dynamics of various neural structures in the brain using the brain/Internet metaphor. Comparison with computer was already exploited in the enthusiastic book by Jeff Hawkins [11] (cf. further developments in [10, 12, 13].)

Another subject matter concerning us here is the cognitive activity of civilizations. Looking for cognitive network patterns at this level is not a standard preoccupation of the historians of culture and sciences, but one of us first engaged in this line of thinking when researching available data on the development of writing, cf. [24].

Our departure point is a simple remark: although WWW is very complex, the knowledge about its structure and functions is available on all levels since it is constructed and developed by means of engineering, cf. the first section below. Contrariwise, brains are products of evolution, we can observe their structure and functioning at various spatial and temporal scales, but we can only venture some guesses about their modes of data processing. AI might be and in fact was a great inspiration for such guesses.

Important role in this circle of ideas is played by the notion of *information transmission*. Generally, we imagine a source of data, which can be encoded, transferred as a message through a certain channel, received at the other, and then decoded to reconstruct the initial data.

Using a Saussurean terminology, we can say that protocols at the transmitter/receiver ends constitute a language *(la langue)*, whereas each case of transmission is an act of speaking *(la parole)*.

There are many mathematical models of information transmission materialized in the IT domain. What we want to stress in this note is the fact that actual transmission must often be relayed: data at the receiving end become, after re-encoding, data at the sender along another channel, and so on. This involves the rules and protocols of *translation* in linguistics. Usually, basic parts of such protocols are accessible as bilingual dictionaries, but even in human societies, there are exotic exceptions such as *drum languages* of various ethnic groups of Africa, New Guinea, etc.

Thus, there must exist many *neural languages*, each used in respective neural networks and connected by numerous translating neurons/networks.

Finally, the speed with which the brain can solve cognitive problems related to speech generation and recognition (or such a marginal activity as playing chess) unambiguously testifies to the abundance of highly parallel processing in neural networks. Neural organization of such parallel processing must be a very essential logistical task. This was long ago recognized and described by neuroscientists dealing with mechanisms of visual perception.

Here we must stress that mathematical theory of *time complexity of parallel processing* practically does not exist and in any case did not reach maturity comparable with those of *Kolmogorov complexity* and *polynomial time computations.*

Therefore, a better understanding of high parallelism in the brain might serve as a useful heuristic tool for theoretical computer science as well (cf. [17]) and many other studies of visual cortex.

This essay is organized as follows. After a brief survey of the global structure of WWW, we discuss the following subjects:

- Architecture of WWW and the role of search engines.
- Chips, computers, and servers vs. neurons and neural networks.
- Kolmogorov-style compression vs. Charles Darwin-style compression.
- Miscellany.

Computer Networks: Architecture

When describing the information pathways in computer networks, specifically, the Internet, one has to keep in mind that communication between network nodes (individual devices, be that special-purpose servers, personal computers or network-enabled electronic devices) can be considered on several different levels (cf. the OSI model, [31] and [OSI1] where the respective levels are called *layers*). From the lowest level concerned with transferring of raw bits between two neighboring devices to the highest level that operates in terms of such operations as remote file access or search engine queries, each layer has its own semantics and serves as a medium for the next higher layer. On the lowest levels of the hierarchy, each device can communicate only with its immediate neighbors. On the intermediate levels, the complexities of networking are hidden, and nodes can directly address their requests to specific other nodes (identified by IP addresses). Finally, on the highest levels, the notion of a network node is also hidden, and they operate in terms of services, such as a named file share or a particular search engine.

What makes this transparency possible is the existence of "routing protocols" encapsulated in a special class of Internet servers called *routers*. Without getting into details, routers keep exchanging information they learn about network nodes existing in their neighborhood. Since the network configuration keeps changing (as nodes come up on and drop off the network, as new subnets are added and old ones reconfigured), the routing information is never complete, always to some extent outdated, and often contradictory. The robustness of communication between nodes is only achieved by the routers' ability to retry delivery of lost messages using different routes. What is most interesting for us here is that even considered on this level, Internet possesses a (varying with time) map of itself, in particular of its own topology. This map is approximate, somewhat fuzzy, partially delayed, and decentralized. Perhaps it can be likened to the living organisms' proprioception.

From the point of view of information processing, we should look at the application level of the OSI model. As in the brain, information enters the network through peripheral nodes, i.e., mostly consumer devices where people type in texts and upload images or videos. Some of it stays local, of course, but some of it travels on the network to be stored, transferred to other peripheral nodes (e.g., email) or, most interestingly, processed, digested, summarized, and transformed. We can discern several types of memory-like subsystems in the network.

1. *Storage systems.* These are places (server farms) built to provide the archival and backup functions to the users, such as DropBox or Google Drive. They are probably the least interesting type of network "memory," returning exactly what was put in there on a specific request to retrieve it.
2. *Internet archives, such as the Wayback Machine [OSI2].* It crawls the web and stores the current copies of the sites it visits, without overwriting the older versions. Thus, it allows one to reconstruct the history of web's dynamics, though in an unavoidably patchy form.
3. *Internet search engines.* Search engines started as simple keyword retrieval databases of the important information gleaned from the web but have evolved into powerful associative memory-type services. What's most interesting about search engines is that they increasingly perform deep analysis of both the content they index and the search patterns of the users, attempting to serve ever more complex and fuzzy user queries. There is an understanding that to effectively respond to difficult informational queries, a search engine has to possess at least a rudimentary type of world knowledge, such as Google's Knowledge Graph [OSI3] and similar systems developed by other search companies.

Note that a significant portion of world knowledge (perhaps, the vast majority) in such systems is harvested from the web, rather than being manually entered. Search engines perform many different analyses of the content they index, like news aggregation (do these two news articles talk about the same event? if so, who are the event actors?), sentiment analysis (is this a positive or negative news story?) or image recognition (what objects are in the photo?).

Search engines represent the kind of information storage that is inherently capable of self-reflection. As a rudimentary, but highly visible example, consider several incidents where a search engine's algorithms would make a funny or offensive mistake in response to a query, which would become a news item, and then very soon the results it would return to the same search query would prominently feature news about its own mistake in what could be perceived as a form of self-deprecating humor.

Neural Systems: Experiments, Measurements, and Self-Perception

The only direct information channel to one's neural system for each human being is self-perception, including memory, emotions, and conscious sensory perceptions ("I see" means "I know that I see").

Objective information about neural systems of other people, but also of animals belonging to different species, is obtained in laboratories and clinics, but this is an outsider's information.

Bridging together insider's and outsider's views has always been and remains a great challenge. In particular, clinical and scientific interpretation of the data of psychology and psychiatry can be hopelessly caught in the trap of *suggestion*: cf. a very convincing study of the history of psychoanalysis in [2].

Attempts of such bridging based upon computer metaphor were numerous. Below we will briefly survey some of the conjectures summarized in Yu. M.'s paper of 1987 On Early Development of Speech and Consciousness (Phylogeny); see [18], pp. 169–189.

Basically, it was conjectured that the brain contains inside a map of itself and that some neural information channels in the central neural system:

(a) *carry information about the mind itself, i.e., are reflexive;*
(b) *are capable of modelling states of the mind different from the current one, i.e., possess a modelling function;*
(c) *can influence the state of the whole mind and through that the behavior, i.e., possess controlling function* ([18], p. 179)

It was remarked also that that this reflection of the brain inside itself must be unavoidably coarse grained.

This is made much more precise in the already invoked above OSI (Open Systems Interconnection) models of the Internet, where both the notion of the network node and protocols of their communication are subdivided into "horizontal" layers (seven in [31] [OSI1]). The lowest layer represents the topology of physical medium transmitting "raw bit streams," whereas the highest layer represents the most coarse-grained vision of the whole network. Each layer has its own communication language; each individual transaction (information transmission) on a particular layer can involve multiple transactions on the next lower layer and, in turn, serve as a part of a transaction on the next higher layer. Thus, information transmission of the highest layer data is mediated by multiple translations down to the lowest layer at the source, a corresponding translation up at the destination and potentially multiple partial up and down translations at the intermediate points.

We stress again that streams of bits on the wire directly represent only the lowest-level communication. In order to decode higher-level transactions, one would inevitably have to ascend the hierarchy of languages, aggregating multiple lower-layer conversations into a single higher-layer conversation: there is no way to directly jump from the lowest to the highest layer. The same is true about the electrochemical messaging in the brain: individual trains of neuronal spikes do not directly represent thought or perception patterns. This is of course well understood by experimental neuroscientists who use expression *"signature of ... "* in articles summarizing their findings (cf. [16, 30]).

As WWW, the mind can contain several dynamical reflections of itself, differently positioned with respect to the functions of mutual reflection and control. The respective functional modes of the mind manifest themselves in a wide variety of dissociative phenomena: multiple personalities, automatisms, fugues, hypnotic phenomena, etc.

Concrete implementations of fragments of multilayered structure in the brain are evident, for example, in the studies of processing of sensory information of different modalities. The way from a sensory input to the appropriate neural network in the respective projection area should be imagined as "vertical" information transfer from lower layers up. On the other hand, integration of different modalities, storage of the compressed form of this information, etc. should involve a considerable role of horizontal conversation.

In the human brain, anatomy of neocortex involves several (six) layers, and Jeff Hawkins made a series of conjectures about storing and processing information inside and between these layers (see [11], pp. 42, 237–245). In Yu. Arshavski's opinion (private communication), at least part of these conjectures can be or have been experimentally verified, but the general association of these anatomical layers with processing layers is hardly justified.

We believe that understanding of such phenomena as cognitive maps of spatial environment [8], mirror neurons [9], or concept cells [27, 28] can benefit from a purposeful search of WWW-like layers and decoding their languages (cf. [1, 29]).

Information about these layers might also enrich the current rigid juxtaposition of "purely connectionist" paradigm and the "intracellular" paradigm, according to which cognitive processes are primarily served by chemistry and genetics of specialized cells rather than by firing of individual neurons connected into networks. It seems clear that memory must involve chemistry and genetic structures and cannot be based solely on network dynamics.

Information, Compression, and Computation

Civilizational layer of cognition. In [20], one of us argued that cognitive processes in the human brain might and ought to be theoretically considered also at one level above the individual brain, namely, on the *civilizational layer.*

Nodes of this layer are individual brains, but also, starting with early modernity, it is enriched with libraries, laboratories, research institutes, etc.

Comparison of this layer with (more formalized conceptually) layers involving primarily computers was based upon the following suggestion. Let us focus on physics, science that dominates today our understanding of the universe along the vast spectrum of spatiotemporal scales.

It is a common knowledge that physics discovers "laws of nature" that are expressed by compact mathematical formulas. These laws of nature can be then used for prediction/explanation of results of observations (say, in astronomy) and of experiments and also for engineering projects.

It was suggested in [20] that each physical law might be considered as an *analogue of a computer program.* Such a program computes the *output* after accepting *results of observations as an input.* These outputs are *scientific predictions.* The classical example consists in predicting observable positions of planets using models by Ptolemy, Galileo, Newton, Einstein, etc.

This process might also involve other laws/programs, multiple relaying, encoding, and decoding that converge at an additional civilization layer node.

As a contemporary example, consider the recent news that the international team of scientists using LIGO (Laser Interferometer Gravitational-Wave Observer) was able to detect gravitational waves and identify their source: two colliding black holes.

Roughly speaking, gravitational waves resonate with light waves, because high-frequency oscillations of space-time curvature (caused by gravity) cause the entire system of light-like geodesics (which in the first approximation determine the light propagation) to oscillate at the same frequency.

The basic "physical law" involved in this event consists of Einstein general relativity equations and its solutions of a special type (black holes).

At the node of observations, a large sample of other "physical laws" is invoked that determine engineering decisions needed to construct the big observational device called LIGO which detects very small frequency changes of laser beams using the interference techniques.

Finally, at all stages, actual computers are used, whose inputs and outputs represent "vertical" communication between an upper and a lower level involved in this observational activity.

Mathematical models in computer science: computability, complexity, and polynomial time. It is well known that the mathematical theory of computability was created in the 1930s and 1940s in several different versions: Turing machines *(engineering metaphor)*, Church's lambda calculus *(linguistic metaphor)*, Markov's algorithms *(conveyor belt metaphor)*, Kolmogorov-Uspensky's algorithms *(information flow chart metaphor)*, partial recursive functions *(operadic metaphor)*, etc.

All these versions differ in many respects. First of all, their respective domains of inputs and outputs viewed as Bourbaki-style structures are different: finite sequences of bits (zeros and ones) for a Turing machine, finite words in an arbitrary fixed alphabet for a Markov's algorithm, and words of a language which is the basic object of lambda calculus. Second, programs for particular computations are formalized differently as well: a finite list of inner states of pairs *(head, head input)* for a Turing machine, a finite word expressing the sequence of basic operations on recursive functions together with their inputs, etc.

Nevertheless, it was proved that all these constructions produce "one and the same" notion of computability, in a well-defined mathematical sense. One of the most remarkable events in the nascent computer science occurred when one of the founding fathers stated his famous "Church's thesis": *the computability notion is absolute and does not depend on the chosen model of computation* (if the latter is broad enough).

This thesis is *not* a mathematical theorem: it can be called an "experimental fact in the Platonic world of ideas."

The next great discovery in this domain was that of "Kolmogorov complexity."

If a model of computability and the suitable programming language are chosen, then one can prove the existence of the best compressing program U with the following properties.

(a) Let Q be an arbitrary object in the domain of this computability model or else, a description of a partial recursive function, U a semi-computable function. Define the *complexity of Q with respect to U* as the bit length of the shortest object P such that $U(P) = Q$ (or, respectively, Q is a program of computation of the same function). In other words, P taken as input of U produces Q as its output. Such a P always exists.
(b) There exists a class of optimal choices of U such that a different choice of the universal programming language and/or of another U leads only to a possible change of complexity (as function of Q) by a bounded additive constant.

Intuitively, this shortest object Q is best imagined as a *maximally compressed form of P*. Thus, we may say that Newton's classical laws of celestial mechanics

$$F = G\frac{m_1 m_2}{r^2}, \quad a = \frac{F}{m}$$

are maximally Kolmogorov-compressed representations of programs that can calculate and predict future positions of celestial bodies, where observations of their current positions are taken as inputs.

Arguably, this Kolmogorov compression metaphor gives a widely applicable picture of scientific knowledge, when it is restricted to *one of many* timescales of natural phenomena: cf. [15] and the LIGO story.

In the papers [19, 23] and [21], it was argued that brains actually also use neural codes allowing good compression of relevant information.

One set of arguments suggested that such a compression of, say, dictionary of the mother language in human brain can explain the well-known empirical observation, Zipf's law.

This "law" (in fact, a keen and very general observation) states that if one ranges lexemes in the order of their decreasing frequency of usage in a representative corpus of texts, then the product of lexeme frequency by the lexeme rank is approximately constant. In [23], it was argued that a good mathematical model of such behavior is furnished by the L. Levin's probability distribution, if one postulates that Zipf's ranking coincides with (an approximation to) Kolmogorov complexity ranking.

The fact that Kolmogorov complexity in strict mathematical sense itself is not computable cannot refute this conjecture. In fact, successive approximations to Kolmogorov complexity ranking can be obtained by a version of the well-known ranking algorithm.

Consider, for example, encoding and storing in the brain of the vocabulary of a mother language. We suggest that when a new lexeme is being encoded in a brain memory network, the length of this encoding (Zipf's "effort") is compared with lengths of previously encoded lexemes, and the lexeme acquires its temporary Zipf's rank.

Another set of arguments combined the discussion of neural encoding of stimulus spaces in [6] with suggestions of [30] that dynamics in neural networks shows signatures of criticality. "Criticality" here means that, within a certain statistical model of the relevant network, this dynamics happens near a phase transition regime. But it was discovered in [25] that search for good error-correcting ("noise-resistant") codes generally involves activity near a phase transition curve, even though the relevant statistical model does not coincide with the one in [30]: in fact, it again involves Kolmogorov complexity.

Stretching the metaphor further, we can also consider human communication occurring in natural language in the same light. A natural language message is usually treated as carrying information. But it also can be treated as a program that runs in the brain of the receiver and whose purpose is to create a certain mind state in it. This interpretation is particularly interesting for literary texts, especially poetry, because their purpose is not conveying information but rather imparting an emotional state to the reader. It is customary to state that successful poetry *compresses* its language, and, consequently, if one wants to fully explicate the "meaning" of a good poem, an extensive prose text has to be written. So perhaps the right way to conceptualize a great poem is to say that it represents a maximally Kolmogorov-compressed representation of the target mind state.

In the theoretical computer science, besides complexity as the length of a shortest program, an important role is played by various embodiments of the notion "length/time of computation." From this viewpoint, we are interested in minimizing time necessary for producing the output from an input. The most accomplished theory here led to the so-called "P/NP problem." Roughly speaking, if *there exists* a computation of a function which requires time polynomially bounded by the length of input, can one also *find* this computation using polynomially bounded time?

More precisely, in a model of the *universal* NP problem, we consider all Boolean polynomials F with arbitrary number of variables and ask the question whether a given polynomial takes value 1 for some values of its arguments.

If the answer for F is positive, this fact can be proved in polynomially bounded time (wrt the length of F) by starting with an appropriate Boolean vector x and then calculating the value $F(x) = 1$. But can we *find this* x or else find *another proof* that F takes value 1 in polynomially bounded time? This is the P/NP problem the answer to which answer is not known.

What is relevant for our discussion here is the fact that if we allow parallel computations in our models, such as parallel computation of *all* values of any given Boolean polynomial by starting with all inputs of given length simultaneously, then the P/NP problem will obviously have the answer $P = NP$. Thus, economy in computation time can be achieved by allowing multiple parallelism.

This, in addition to program compression, might be another crucial mathematical idea that materializes in large networks, both in brains and in civilizations.

Returning to the intuitive idea of compression, we want now to argue that there is another type of compression which we will call here "Darwinian compression."

Darwinian compression. Charles Darwin's Beagle voyage was one of the defining events in the development of human civilization because it has radically changed our collective self-perception.

Narrowing our focus to see better his method from the viewpoint of its cognitive characteristics, we can say that Darwin started with collecting a vast database of living creatures. The contemporary ideology of data mining could suggest us that his next step would be the search for correlations in this database and discovery of various degrees of their possible interrelationships. However, this kind of research was essentially done before Darwin: Carl Linné introduced the binary classification system (genus/species) and created the principles of taxonomy that are still widely used.

Darwin's great breakthrough consisted in guessing how this diversity could have occurred and what factors could determine the origin, development, and change of genera and species. The possibility to compress his intuition in just two words, "natural selection," motivated our metaphor "Darwinian compression."

But in reality, one cannot rigorously derive, say, the evolution theory from genomics: all our attempts and arguments are of vague qualitative nature, at best convincing us that the two sets of laws are compatible. A succinct and very expressive description of this baffling situation was given by Svante Pääbo in his book [26]: *'The dirty little secret of genomics is that we still know next to nothing about how a genome translates into the particularities of a living and breathing individual.* Hence, we cannot say which genomes would define "the fittest" individuals that, according to the Darwinian metaphor, have better chances for survival and reproduction.

Attempts to fill this gap led to the development of "epigenetics," which is studying factors and developmental processes that modify the activation of various genes without changing the genetic code sequence of DNA: cf. [32]. Such epigenetic processes in a chromosome can lead to the appearance of stably heritable phenotype traits, which then can play their own specific roles in Darwinian evolution.

Another example of a scientific discovery of a similar cognitive type is the Periodic Table of chemical elements which embodied a compression of a huge database of alchemical and later chemical observations, experiments, and guesses.

Both discoveries, evolution and periodic table, can be considered as a way of connecting various floors of scientific knowledge referring to various *space-time/complexity* scales. Each floor is governed by its "laws" in the sense described above, which in principle should be used to generate the laws of the next floor.

But, as in the case of Darwinian evolution, one cannot rigorously derive the periodic table from the quantum theory of elementary particles and fields, and one cannot rigorously derive, say, observable properties of water, ice, and steam from the position of H and O in the periodic table.

More precisely, quantitative theory of atoms of the lightest elements consisting of a minimal number of elementary particles might be accessible (with the help of modern computation resources), but the whole structure of the table (including isotopes) and the very notion of molecules and their "chemical" properties, with its continuing extensions and ramifications all the way up to DNA encoding, remain the "upper-floor" science, not really reducible to the science "one floor below."

This is why we find so naive (and potentially dangerous) the claim by Chris Anderson, Editor in Chief of the Wired magazine, expressed in the title of the cover story "The End of Theory: The Data Deluge Makes the Scientific Method Obsolete" (summer 2008):

The new availability of huge amounts of data, along with statistical tools to crunch these numbers, offers a whole new way of understanding the world. Correlation supersedes causation, and science can advance even without coherent models, unified theories, or really any mechanical explanation at all. There's no reason to cling to our old ways.

For more detailed arguments, cf. [22].

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Reuben Hersh on the Growth of Mathematical Knowledge: Kant, Geometry, and Number Theory

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In his reflective writings about mathematics, Reuben Hersh has consistently championed a philosophy of mathematical practice. He argues that if we pay close attention to what mathematicians really do in their research, as they extend mathematical knowledge at the frontier between the known and the conjectured, we see that their work does not only involve deductive reasoning. It also includes plausible reasoning, "analytic" reasoning upward that seeks the conditions of the solvability of problems and the conditions of the intelligibility of mathematical things. We use, he argues, "our mental models of mathematical entities, which are culturally controlled to be mutually congruent within the research community. These socially controlled mental models provide the much-desired "semantics" of mathematical reasoning" (Hersh 2014b, p. 127). Every active mathematician is familiar with a large swathe of established mathematics, "an intricately interconnected web of mutually supporting concepts, which are connected both by plausible and by deductive reasoning," that include "concepts, algorithms, theories, axiom systems, examples, conjectures and open problems," and models and applications. Thus, "the body of established mathematics is not a fixed or static set of statements. The new and recent part is in transition" (Ibid, pp. 131–2).

This view of mathematics, elaborated in his recent essays and in his book *Experiencing Mathematics: What Do We Do, When We Do Mathematics?* (Hersh 2014a), puts him at odds with the usual schools of thought in the 20th-century Anglophone philosophy of mathematics: intuitionism, inspired by Kant; formalism, championed by Hilbert; and logicism, beloved by logicians. Philosophy of mathematics in the twentieth century was pursued under the star of reduction; whether the reducing field

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was arithmetic, logic, or set theory, geometry was inevitably slighted; so geometry will play a key role in my development of Hersh's insights, though I return to numbers at the end.

Those 20th-century philosophers who have wished to do better justice to the claims of geometry have typically appealed to Kant's doctrine of Transcendental Aesthetic with its "pure intuition of space," and the emphasis on synthetic construction (inter alia, the construction of figures) in his transcendental analytic, as the mind unites the "parts" of the infinite wholes that condition all it knows. Kant calls space and time "manifolds" which must be synthesized to be known, but observes that, unlike the heterogeneous manifold presented by sensation, the pure manifolds of space and time are homogeneous. Early in the twentieth century, Poincaré invoked Kant in order to escape Leibniz's (alleged) claim that all mathematical knowledge is analytic, and Brouwer founded his school of intuitionism on a version of Kant's notion of synthesis. Later exponents of intuitionism, like Carl Posy, have defended Kant's account of the continuum and geometric objects related to it. Jaakko Hintikka has espoused it because it offers a link between knowledge of particulars and processes of acquiring knowledge more general than perception. And Charles Parsons tried to develop it as a form of "immediate" mathematical knowledge, knowledge not mediated by concepts.

Generally speaking, these philosophers regarded Kant as a refuge from the tendency of twentieth-century philosophy of mathematics to logicize and arithmetize mathematics, that is, to regard the things of mathematics as reducible to numbers or sets or to regard mathematics as reducible to a collection of formal theories. In particular, Kant's doctrine of space and of the synthesis of mathematical objects is viewed as a way to salvage the status of geometry within mathematics as an independent and irreducible source of knowledge. As in the writings of Poincaré, Kant's intuitionism is opposed to Leibniz's (alleged) logicism and conceptualism, and epitomized in the claim that space is not a concept, and that the investigation of a triangle (like that of a sum of integers) cannot be carried out as a deduction from definitions.

However, I regard Leibniz as a more likely champion of geometry, and a philosophy of mathematical practice, than Kant, and so too as a possible ally for Hersh, and other allied philosophers. First, Kant's doctrine shares with twentieth-century logical positivism, a dogma that seems to me incompatible with the defense of geometry, i.e., the supposition that the unity of the objects of our knowledge (including our mathematical knowledge) can be referred to the mind. That is, for Kant not only the unity of the judgment, the unity represented by the copula that links subject and predicate, but the very unity of the object is a function of thought. This same dogma underlies the project of set theory to reduce the things of geometry to sets of points and the things of arithmetic to sets built up from the empty set, because points and the empty set are "placeholders," the semblance of mathematical objects with no unity of their own to investigate. Then all unity in set theory stems from the constitution of sets as collections of elements. Thus after its logical reduction, all that remains of mathematics are formal theories, whose unity is the discursive unity of the axiomatic system.

Second, we also find in Kant an account of space as the pure form of intuition, which is strictly speaking unknowable, and of figure as the result of the synthesis of parts which cannot do justice to the integrity and irreducibility of geometrical shape and the role it plays in the development of geometry. Nor can we find in Kant a satisfactory answer to the difficult question of how we are to understand the relation of a figure to its ambient space. The answer to this question opens up the study of non-Euclidean geometry, differential geometry, and topology in the 19th century. An analogous question, how numbers stand in relation to their surrounding field or ring, is key to the development of number theory in the 19th century.

Leibniz's account of the intelligible object (and his account of the judgment which follows from it), of analysis as the search for the conditions of intelligibility of such objects, and of synthesis as posterior to such analysis seems to me a far better way to explain the growth of mathematical knowledge than Kant's Transcendental Aesthetic. I defend my comments in favor of Leibniz by the elaboration of the two important developments just cited.

Kant's Account of Space as a Pure Form of Intuition

Kant defines intuition and conception as very different kinds of representations. Intuition is immediate and receptive; an intuition relates immediately to the object and is singular. Conception is mediate and active or spontaneous; a concept is a general representation that refers to an object "mediately by means of a feature which several things may have in common" (Kant 1929/1965, A320/B376). All intelligible unity is referred to concepts: even the indeterminate unity of "an object, I know not what" is referred to the work of a pure concept, the category of substance. Concepts, Kant claims, "rest on functions. By 'function' I mean the unity of the act of bringing various representations under one common representation" (Kant 1929/1965, A68/B93). Intuition provides a "manifold," but the manifold cannot be organized into unified, intelligible things without the unifying function of concepts. The same function which gives unity to the various representations in a judgment also gives unity to the mere synthesis of various representations in an intuition" (Kant 1929/1965, A799/B105). And as Kant adds right after his presentation of the *Table of Categories*, "by [the categories] alone can [the pure understanding] understand anything in the manifold of intuition, that is, think an object of intuition" (Kant 1929/1965 A81/B107). The relation of intuition and concept is external, and all unity is located in the concept. (Likewise, in the set theoretic reduction of geometry, the relation of set to elements is external, and all unity is located in the function of set-formation; in the logicist reduction, geometry becomes arithmetic and arithmetic becomes logic, so that all unity is located in the formula; and in the formalist reduction, geometry becomes whatever satisfies a certain set of axioms, so that all unity is located in the formal system.)

One might object that the notion of formal intuition which Kant invokes in the B version of the *Transcendental Deduction* is an exception to this external relation.

For there, Kant writes, "But space and time are represented a priori not merely as forms of sensible intuition, but as themselves intuitions which contain a manifold, and therefore are represented with the determination of the unity of this manifold" (Kant 1929/1965 B161). And he adds in a footnote, "Space, represented as object (as we are required to do in geometry), contains more than mere form of intuition; it also contains combination of the manifold, given according to the form of sensibility, in an intuitive representation, so that the form of intuition gives only a manifold, the formal intuition gives unity of representation" (Kant 1929/1965, B161). It may be that Kant wishes, for good reasons, that "formal intuition" should constitute an exception to his rule that intuition and conception are externally related and that all unity is referred to the concept. But at the end of the footnote, Kant refers us back to Section 24, where he writes, Apperception and its synthetic unity is, indeed, very far from being identical with inner sense. The former, as the source of all combination, applies to the manifold of intuitions in general, and in the guise of the categories, prior to all sensible intuition, to objects in general. Inner sense, on the other hand, contains the mere form of intuition, but without combination of the manifold in it, and therefore so far contains no determinate intuition, which is possible only through the consciousness of the determination of the manifold by the transcendental act of imagination (synthetic influence of the understanding upon inner sense), which I have entitled figurative synthesis. (Kant 1929/1965, B154–155).

In other words, Kant cannot explain further what he means by "formal intuition" except by appealing to the combination of the manifold produced by apperception ("in the guise of the categories"). To illustrate what he means, Kant gives the examples of drawing a line and describing a circle, acts which make pure intuition determinate. He concludes, "The understanding does not, therefore, find in inner sense such a combination of the manifold, but produces it, in that it affects that sense" (Kant 1929/1965, B155). Thus it seems that all Kant can mean by determinate "formal intuition" (as opposed to the form of intuition, which is so far indeterminate) is the pure manifold organized (connected, collected, combined) by pure concepts, the categories. Once again, unity is referred to concepts, and concepts prove external to intuition.

We will return to the issue of the construction of figures in the next section; but first we must revisit Kant's characterization of space as a pure form of intuition. The *Transcendental Aesthetic* includes two expositions of the representation of space. The metaphysical exposition sets out what is included a priori in the representation of space and avoids all reference to geometry. By contrast, the transcendental exposition treats the representation of space as a transcendental principle, the necessary condition for the possibility of the science of geometry. In the metaphysical exposition, Kant claims that space is not an empirical concept but rather a necessary a priori representation, which underlies all outer intuitions; it is not a discursive or general concept of relations of things in general but a pure intuition; and it is represented as an infinite given magnitude. His remarks about the unity of space are especially important: "We can represent to ourselves only one space; and if we speak of diverse spaces, we mean only parts of one and the same unique space... These parts cannot precede the one all-embracing space, as being, as it were, constituents out of which it can be composed; on the contrary, they can be thought only as in it. Space is essentially one; the manifold in it, and therefore the general concept of spaces, depends solely on limitations." (Kant 1929/1965, A25/B39).

Space as the form of intuition is an irreducible whole (it is not composed of constituent parts available prior to it), and it is infinite, nondiscursive, and inescapable. Thus as a form of intuition, as an immediate representation, space is unknowable, since without concepts intuitions are blind. Thus, I would argue, this characterization of space accords with certain important features of the plane in Euclid's *Elements*. For in that work, two-dimensional space has no parts or distinguishable places at all and thus no discursive structure: no boundaries, no holes, no separate pieces, no bumps or curves, no density or thickness or texture, no handedness, and no direction: no up or down, left or right, back or forth. Thus described, it appears as a surd, not as a possible object of knowledge. Yet what I have just written describes it accurately and amounts to knowledge of it as a space, distinct from other kinds of spaces. How do I know these features of Euclidean space, if there is nothing to it? The key to Euclidean geometry is not space, but figure: figures have parts in virtue of their shape, and the rational relations among parts are the key to understanding space itself. But this is an insight that comes to mathematical fruition only in the 19th century.

The transcendental exposition of space posits the science of geometry and then asks what must be the case for a science to exist, which offers synthetic a priori knowledge that is yet independent of empirical sense perception. Kant's answer is that space must be an outer intuition which precedes objects and determines them a priori; as the form of outer sense, it has its seat in the subject: it is the subjective form of sensibility. Thus, "space does not represent any property of things in themselves, nor does it represent them in their relation to one another" (Kant 1929/1965, A26, B42). Thus for Kant, there is no avenue to knowledge of space in terms of space as pure intuition; and there is no avenue in terms of the empirical objects which result from the way that the categories organize sense perception. Space qua form of intuition is an infinite, singular something, without determination. Space is also not made determinate by the spatial properties of physical objects; it is somehow determinate prior to that empirical determination. What kind of a determination could that be? (Reuben Hersh offers a related critique of the originally Kantian notion of intuition in (Hersh 2011).)

Kant's Account of Figure

As Kant instructs us in the *Transcendental Aesthetic*, we arrive at space and time by subtracting "everything which the understanding thinks through its concepts" from experience and then subtracting "everything which belongs to sensation": what is left are pure intuition and the mere form of appearances (Kant 1929/1965

A22, B37). The pure manifold that remains is unknowable for two reasons. The first is that just cited: space as a whole without parts is utterly indeterminate. The second is that space is nothing more than *partes extra partes*, all and only parts, a manifold of an utterly unordered multiplicity: without the power of combination of the mind, each "representation is completely foreign to every other, standing apart in isolation" (Kant 1929/1965 A98). The unity of space must be constructed, and what is constructed is figure. The parts of a figure are always prior to the figure; since we construct the figure out of its parts, the figure is knowable by us: we know what we ourselves have put into it (Kant 1929/1965, A163). This claim stands in contrast to Kant's claim about space as a pure form of intuition: space is always prior to the parts of space and in a sense has no parts (Kant 1929/1965 A25).

So to answer the question of how, for Kant, space is rendered determinate and therefore knowable, we must turn first to the *Transcendental Deduction*, and next to the *Axioms of Intuition*. In the *Transcendental Deduction* (A version), we recall, there are aspects or moments of synthesis which Kant distinguishes from the conceptual synthesis brought about by the categories: the synthesis of apprehension in intuition and the synthesis of reproduction in imagination. Such preliminary syntheses are required in order for the "synthesis of recognition in a concept" to be possible. Kant illustrates both preliminary syntheses by reference to space.

Of the synthesis of apprehension in intuition, he writes, "in order that unity of intuition may arise out of this manifold (as is required in the representation of space) it must first be run through, and held together. This act I name the synthesis of apprehension, because it is directed immediately upon intuition, which does indeed offer a manifold, but a manifold which can never be represented as a manifold, and as contained in a single representation, save in virtue of such a synthesis." (Kant 1929/1965 A100) Of the synthesis of reproduction in imagination, he writes, "Experience as such necessarily presupposes the reproducibility of appearances. When I seek to draw a line in thought... obviously the various manifold representations that are involved must be apprehended by me in thought one after the other. But if I were to drop out of thought the preceding representations (the first parts of the line...) and did not reproduce them while advancing to those that follow, a complete representation would never be obtained: none of the above-mentioned thoughts, not even the purest and most elementary representations of space and time, could arise." (Kant 1929/1965 A102) Kant's claim is that these two preliminary syntheses are preconceptual.

Thus, these two preliminary syntheses of the manifold seem to yield "formal intuition," "a synthesis which does not belong to the senses but through which all concepts of space and time first become possible," and whose unity "belongs to space and time, and not to the concept of the understanding," as Kant puts it in the B Deduction (Kant 1929/1965 B161 note). He notes there, the "form of intuition" contains no determinate intuition; but "formal intuition" is "consciousness of the determination of the manifold by the transcendental act of imagination (synthetic influence of the understanding upon inner sense), which I have entitled figurative synthesis" (Kant 1929/1965, B154).

This brings us to the *Axioms of Intuition*. There he asserts, “appearances are all without exception magnitudes, indeed, extensive magnitudes. As intuitions of space or time, they must be represented through the same synthesis whereby space and time in general are determined” (Kant 1929/1965 B203). The representation of an extensive magnitude, Kant claims further, requires the representation of its parts and the synthesis of those parts, just how the mind acts on the manifold of pure intuition: “only through successive synthesis of part to part in its apprehension can it come to be known. All appearances are consequently intuited as aggregates, as complexes of previously given parts” (Kant 1929/1965 B204). Once again, he illustrates his claim in terms of geometric figure: “The mathematics of space (geometry) is based upon this successive synthesis of the productive imagination in the generation of figures. This is the basis of the axioms which formulate the conditions of sensible a priori intuition under which alone the schema of a pure concept of outer appearance can arise – for instance, that between two points only one straight line is possible, or that two straight lines cannot enclose a space, etc.” (Kant 1929/1965, B204/A164).

Various questions must be raised here. If the pure manifold of intuition is somehow organized prior to, and independent of, the categories, what faculty is responsible for this preliminary organization? In the passages just cited, Kant invokes intuition, imagination (reproductive and productive), and the understanding. Why can he not decide? Why is the assignment of agency here so unstable? Does intuition deserve the status of a faculty if it is essentially receptive: aren’t faculties supposed to be active? Can understanding be the pertinent faculty, if its main function is the application of the categories? And what is the mysterious faculty of the imagination, if not a kind of placeholder for a middle term that Kant requires, even though his metaphysics leaves no place for it? Finally, why should the unity of geometric figure be referred to the unifying action of mind, even a “preconceptual” action? Why not say that the mind encounters intelligible but problematic objects and then must investigate them as it can? Why are we constrained to suppose that the intelligible shape of the line, or circle, or triangle, or sine wave is conditioned at all by the human mind that knows it?

Kant is quite right that he needs an account of formal unity in geometry that is neither the indeterminate unity of space nor the unity conferred by concepts, for the unity of shape is something else entirely. But I find his account of “formal intuition” unconvincing: first because I think it is finally inconsistent with his other metaphysical presuppositions and second because I think no account of the determinate unity of figure can or ought to begin from the synthesis of indeterminate parts. The determinate but problematic unity of figure, I submit, is prior to its parts; and its parts are “always already” determinate, though also, of course, problematic. Moreover, the stuff of geometry is heterogeneous, not homogeneous.

The Relation Between Figure and Space

How then shall we understand the relation between figure and space? The relation is not one that can be treated by logic, since it is neither a relation between a generic concept and a specific concept (the relation of abstraction) nor that between a universal concept and an individual object (the relation of instantiation). The relation cannot be treated by physics: it is neither the relation between a physical whole and its parts nor even the measurement of the spatial relations among the parts of a physical whole. Triangles (or lines, or circles, or sine waves) do not instantiate space, nor do they specify it, nor do they compose it. They "articulate" space so that we can come to understand the properties of space; they make space knowable. Moreover, certain figures are canonical, and they reveal the features of canonical spaces; these in turn give us access to (serve as conditions of intelligibility for) other spaces and figures. But what does this "articulation" amount to?

Kant believed, with Aristotle, that the line is "viscous," not reducible to points: he rejects the idea of a linear continuum built up of independently given elements: points. He writes, "The property of magnitudes by which no part of them is the smallest possible, that is, by which no part is simple, is called their continuity. Space and time are called *quanta continua*, because no part of them can be given save... in such fashion that this part is itself again a space or a time... Points and instants are only limits, that is, mere position which limit space and time" (Kant 1929/1965, A 169–179/B 211–212). And yet, because Kant shares with reductionist philosophy of mathematics the assumption that the unity of the object must be referred to the action of the mind, he also shares the assumption that the unity and determination of the mathematical object must be externally or extrinsically imposed upon an indeterminate placeholder. His claim that the things of geometry may be decomposed into combinations of pure and therefore homogeneous representations is then not so different from the set theorist's claim that geometrical objects may be decomposed into sets of points. Thus, the following argument, though first deployed against the set theorist, ultimately turns against Kant as well.

The problem, how we get from the point to the line segment, is puzzling. The point in itself, like the unit, cannot be alienated from itself; like the unit, it also cannot be set beside itself unless the possibility of side-by-side-ness has already been made available. But what provides side-by-side-ness is a geometrical object with spread or extent, in the simplest case the line. If the point is going to generate the line somehow, the line must already be presupposed. Points and lines are coeval. Once spatial extent has been established, points can be set in spatial arrays like lattices to produce interesting geometrical results. Or points can be imagined in infinitary density to mirror the line, as in the analogies of the early infinitesimal calculus and Dedekind's work (though what is meant by side-by-side-ness remains always problematic). Or points can be treated as limits to lines, and then they can participate in geometry. One might say that points become geometrical when they bound lines, for by so doing they serve as the conditions of the intelligibility of lines. By bounding them, points allow lines to stand in analogy to numbers (and so to be measured) and to serve as the boundaries for figures. Just as space would lack intelligible structure without the articulation of a geometric figure, so the line (which is after all a one-dimensional space) would lack it without the articulation of points.

The definition of point precedes that of the line not because lines are composed of points but because points by marking boundaries are conditions of the intelligibility of lines; and the canonical line is a straight line. The canonicity of mathematical things only becomes apparent in mathematical practice. Likewise, lines serve not as components but as conditions of intelligibility for surfaces by bounding them; and the canonical surface is a flat surface. Lines also serve as conditions of intelligibility for angles by bounding them. An angle is a special kind of relation (an inclination) between two lines, given the prior availability of a surface to accommodate their non-collinearity. (A surface is not merely a “special kind of relation among boundary lines,” since the very existence of a surface is required in order that lines may be so related.) What is peculiar about an angle is that its essence is determined by what happens at its cusp; whether its lines are cut off, so that it occurs as the internal angle of, say, a triangle, or whether its lines are left unbounded, it is still the same angle. As an object of thought, then, it has a relativity and indeterminacy that closed plane figures like the circle and triangle do not; the latter are “greater than the sum of their parts” in a way in which an angle is not or not to the same extent.

The angle (right, obtuse, or acute; rectilinear or not) makes a figure thinkable. A circle requires and displays all the possibilities of inclination, all the possible angles formed by the straight lines radiating from its center, in its very constitution. And though a triangle is initially defined in terms of its sides, as a species of rectilinear figure, it can just as well be thought of as a three-angled, three-sided figure. Lines and angles (as a certain kind of paired lines) make surfaces determinate in ways that are much more complex and problematic than the way points make lines determinate. As limits to plane figures, lines participate in the important problems of geometry; lines become problematic when they bound figures. Even the simplest of the plane rectilinear figures, the triangle, and the simplest of the plane curvilinear figures, the circle, are not trivial; they are on the contrary deep and endless sources of problems and discoveries, not least when they are considered in combination. As Hilbert says of the circle, on the first page of *Geometry and the Imagination*, “Even so simple a figure as this [the circle] has given rise to so many and such profound investigations that they could constitute a course all by themselves” (Hilbert 1999).

The main theme of Book I of Euclid’s *Elements* is the triangle; his meditation on the triangle is organized around a master problem, the enunciation and proof of the Pythagorean theorem, which is solved in Proposition 47 (Euclid 1956). The main theme of Books III and IV of the *Elements* is the circle; there his meditation is also—rather more loosely—organized around a master problem, the squaring of the circle (the precise determination of the area of the circle), though the problem is not solved there. Both problems have to do with the way the whole of the figure constrains its parts, imposing an ordered relationship upon them: how a right triangle constrains its sides and how a circle constrains its diameter and circumference, as well as the angles inscribed within it.

The nature of this constraint is significant. Its investigation (the kind of analysis that takes place in Book I and Books III and IV) seeks to discover what makes a shape the shape it is: what are the requisites for a right triangle to be a right triangle or a circle to be a circle? Though these analyses look into the parts and their interrelations, what they uncover is the way in which the whole (of a right triangle or a circle) is greater than—heterogeneous with respect to and prior to—the parts, which in turn is the key to the irreducibility of shape as bounded extension. The analysis of shape, the search for its conditions of intelligibility, return us to the integrity of shape. First, figure is not the mere concatenation of parts somehow homogeneous to it: a triangle is not a set of points. Second, when a part of a triangle or circle is altered, other parts must undergo a compensatory alternation, an adjustment so that the triangle may remain a triangle (or the kind of triangle it is) or so that the circle may remain a circle. No part has a relation to another part that is not mediated by the whole to which they belong. We are used to calling "parts" the boundaries of a triangle—primarily the lines which are its sides, but sometimes also the points which are its vertices (and the boundaries of its sides) as well as its internal angles. So they are, but they are parts in a sense different from the way in which units are the parts of whole numbers, or atoms are parts of molecules, or bricks are the parts of a house. Analytic methods and mathematical practice in different domains must proceed differently. Euclid's definitions remind us (as is so often the case for modern readers) to keep distinct, while still rationally related, things we are used to thinking of as homogeneous. Kant with his doctrine of knowledge as intuition and conception, and set theory in connection with modern logic, has persuaded us to accept as an equation what is only an analogy and to suppose that the things of geometry may be decomposed into (respectively) combinations of pure and therefore homogeneous representations, or sets of points.

If, by contrast, we keep in mind that the boundaries of a triangle are conditions of its intelligibility—what we arrive at through rational analysis of a triangle—then some important aspects of the kind of unity a triangle have become clearer. A brick house is homogeneous with the bricks that compose it; a cardboard box is homogeneous with a set of pasted-together cardboard leaves (bracketing some questions about the role of glue and mortar.) There is no house or box until the process of concatenation, of setting side by side, is finished. In the case of the triangle, however, the triangle must first be given along with the vertices, angles, and sides, which are moreover inhomogeneous with it. The vertices serve to bound something different from themselves, which by themselves they could not generate (lines); pairs of lines bound something different from themselves (interior angles); and all three sides together bound something different from themselves (the shaped surface that is a triangle). Moreover, within a given triangle, the sides and interior angles impose constraints on each other. (This is one of the reasons we do want to say that they are "parts" of the triangle.) Many propositions in Book I show that sides and angles in a triangle provide both global and local constraints on each other. The right triangle in Book I, Proposition 47, the Pythagorean theorem, is then presented as caught in a web of constraints pictured by the auxiliary constructions that surround it and which include not only the squares built upon its sides but a series of parallelograms and triangles within them. These constraints show that no matter how we perturb a right triangle, it will always be the case that "the square of the side subtending the right angle is equal to the (sum of the) squares on the sides containing the right angle."

If we keep these insights in mind, it is easier to see how the study of figures opens up the possibility of the study of space itself, that is, how two-dimensional space and three-dimensional space can become objects of study for geometers. Around 1800, geometers come to see that if a figure is understood as what remains invariant under transformation, that (stubborn) invariance reveals the features of space: the plane is flat, unbounded, and without holes. However, this very formulation opens the possibility of investigating spaces that are curved, bounded, and perforated. This possibility in turn raises the question, what would the canonical objects of Euclidean geometry look like, if they lived on a different kind of space? Or rather, what would the analogue be, of a line, a triangle, or a circle, on a different kind of space, and what would their properties be? What becomes of the claim that the interior angles of a triangle must always add up to 180° if the triangle lives on a surface of constant positive curvature? What becomes of the parallel postulate, when straight lines are geodesics? What happens if the surface has constant negative curvature? What happens globally if the surface can only be mapped locally to Euclidean space? Oddly, in every case, we find that novel figures in these novel spaces exhibit their own stubborn features and can be used to explore those spaces further. (For the best detailed account of this historical development, see Gray 1980/1989 and Gray 2015.)

Meanwhile, Euclidean space continues to exhibit its canonicity with respect to the new upstarts, with its flatness, its right triangles, its parallel lines, and its circles. When Hilbert, in his role as formalist, claimed that all relevant geometrical information is embedded in sets of axioms, so that geometry is only what is common to all interpretations of a theory, up to isomorphism, he was at odds with himself as a geometer where his work always presupposed and depended upon the canonicity of certain objects. If "point," "line," and "plane" can be given alternative interpretations that yet produce a model isomorphic (with respect to a set of axioms) to Euclid's, then there is no reason to demur; and there is no reason to prefer a Euclidean to a non-Euclidean theory for geometry. Geometry then becomes a kind of smorgasbord of models; philosophers of the late nineteenth century were dismayed because they felt they had lost all grounds for choosing which geometry was "true," since the appeal to (usually Kantian) intuition had been discredited as subjective, in either a psychologistic or transcendental sense.

Yet Hilbert, writing in his role as geometer, acknowledged the canonicity of certain objects as a matter of course, since without appeal to that canonicity, certain domains like differential geometry could not even be broached. Introducing the chapter on differential geometry in *Geometry and the Imagination*, he writes, "we will, to start with, investigate curves and surfaces only in the immediate vicinity of any one of their points. For that purpose, we compare the vicinity, or 'neighborhood,' of such a point with a figure which is as simple as possible, such as a straight line, a plane, a circle, or a sphere, and which approximates the curve as closely as possible in the neighborhood under consideration...." The related, more modern notion of differentiable manifold central to topology makes the same appeal to canonical (flat) space. A (Hausdorff) topological space is called "locally Euclidean" when, for every point of the space, there is an open set containing the point and a homeomorphism that maps that open set onto an open set of $\mathbf{R}^n$, that is, when the space looks flat in sufficiently small neighborhoods (Singer and Thorpe, 1967, pp. 109–110). This notion underlies the more general notion of differentiable manifold and makes possible the definition of functions, and the differentiation and integration of functions, on them.

Canonical items do not present themselves as canonical to Kantian intuition but prove themselves to be canonical in the practice of the mathematician, a process of discovery and reflection. They drive the practice of analysis by exhibiting conditions of intelligibility of more general and complex mathematical things, which precipitates methods of problem-solving. So, for example, in order to define and integrate a function on a strange new topological space, a mathematician must find a way to lead the situation back to more well-known and tractable situations; understanding the new space as locally like other canonical spaces is one highly successful method for solving the problem.

In sum, I believe that the Leibnizian notion of analyticity, the search for the conditions of intelligibility of things and solvability of problems, is more likely to help the cause of geometry and to further a philosophy of mathematical practice than the Kantian doctrine of space and the mathematical object as a construction or synthesis of pure intuition. The Kantian doctrine leaves us with the unsolved problem of how to move from the infinite and indeterminate unity of space as the form of intuition, to the formal intuition which results from the "preconceptual" combination of space qua multiplicity of indeterminate spaces, to geometry as the study of space made determinate by concepts. The first is unknowable; the move from the first to the second is unstable in his doctrine, assigned variously to different faculties; and the move from the second to the third is vexed both because geometric figure is a unity prior to its parts and because shape is not a concept. What the object requires from us is not synthesis but analysis. Leibniz assures us that there are always reasons (perhaps many different kinds of reasons) why a triangle or circle has the properties that it has and that the search for them will always reward philosophical reflection (Grosholz 2007, Ch. 2; Grosholz 2016, Ch. 1).

The Relation Between Number and Field

We have just seen how the growth of mathematical knowledge may be driven by using figure to explore space. Likewise, number theory advances when numbers are used to explore number fields or rings. (See also the discussion of elementary number theory in (Hersh 2016).) A. Fröhlich and M. J. Taylor begin their textbook *Algebraic Number Theory* (1991) with a renowned example of how a problem is carried up into a broader context and then re-situated. The problem was first pronounced in 1640 by Fermat, who once again did not have enough room in the margins for his proof: it is to show that an odd prime p can be written as the sum of two squares (that is, $x^2 + y^2$, where x and y are integers) if and only if $p \equiv 1 \pmod 4$. It was first proved by Euler (in two letters to Goldbach in 1747 and 1749) using the method of infinite descent, a kind of *reductio* proof that relies on the fact that the natural numbers are well-ordered and that there are only a finite number of them smaller than a specified n. Lagrange proved it again in 1770 using his general theory of integral quadratic forms (quadratic forms over the ring of integers). But Fröhlich and Taylor present the second of Dedekind's two distinct proofs, both of which use Gaussian integers, which was published in 1894. The key explanatory insight offered in this proof (which I discuss in some detail in Grosholz 2016, Ch. 5) is that a prime number p congruent to 1 (mod 4) loses its irreducibility as a prime number in the ring $\mathbf{Z}[i]$ of Gaussian integers in the field $\mathbf{Q}[i]$, while a prime number p congruent to 3(mod 4) remains prime. That is, the fact that a number is a prime is not simply an inherent feature of the number but also depends upon the number system in which it is located! This discovery is just as astonishing, as the insight that the interior angles of a triangle add up to 180° if and only if its ambient space is Euclidean.

Fröhlich and Taylor offer a general pattern for solving Diophantine equations (indefinite polynomials where the variables range only over the integers): one embeds a problem about integers in subrings of some algebraic number field K, that is, a finite extension field K of $\mathbf{Q}$, the rational numbers. The most important such subring is the set of all algebraic integers in K (designated $\mathbf{O}_K$), and one canonical example is, not surprisingly, $\mathbf{Q}[i]$ whose ring of algebraic integers is $\mathbf{Z}[i]$, the Gaussian integers. Gauss introduced them in his monograph on biquadratic or quartic reciprocity (1832), which investigates the solvability of the congruence $x^4 \equiv q \pmod p$ with respect to that of the congruence $x^4 \equiv p \pmod q$ (where p and q are distinct odd primes greater than two). This work followed his investigations into quadratic and cubic reciprocity, analogously stated for x^2 and x^3. He found that solving these problems was easier if he first posed them in terms of the Gaussian integers rather than ordinary integers and then re-embedded them in the integers afterward.

In the introduction to their textbook, after the example just discussed, Fröhlich and Taylor go on to exhibit other examples. They examine the claim that $p = x^2 - 2y^2$, where x and y are integers and p is an odd prime, if and only if $p \equiv \pm 1 \pmod 8$. The pertinent ring of algebraic integers is $\mathbf{Z}[\sqrt{2}]$, nested inside the algebraic number field $\mathbf{Q}[\sqrt{2}]$. The historical antecedent for this problem is a problem studied at the time of Pythagoras in both Greece and India, which can anachronistically be stated in algebraic notation: given the Diophantine equation $x^2 - 2y^2 = 1$, find integer solutions for x and y. Infinitely many solutions exist, and the ratios x/y can be used to find good approximations for $\sqrt{2}$, for example, $x = 17$

and $y = 12$, or $x = 577$ and $y = 488$. This problem can in turn be generalized to that of finding solutions to the Diophantine equation $x^2 - ny^2 = 1$, where n is a non-square integer.

The delicate issue here is that the norm taking $\mathbf{Z}[\sqrt{2}]$ back down to $\mathbf{Z}$ is not the same as it was in the first example: $\mathbf{N}_2(x + y\sqrt{2}) = \mathrm{x}^2 - 2y^2$. This means that while $\mathbf{Z}[\sqrt{2}]$ is again a principal domain, the group of units of $\mathbf{Q}[\sqrt{2}]$ is not finite, since an algebraic integer $u + v\sqrt{2}$ ($u,v \in \mathbf{Z}$) is a unit of $\mathbf{Z}[\sqrt{2}]$ if and only if $\mathbf{N}_2(u + v\sqrt{2}) = u^2 - 2v^2 = +-1$. However, the difficulty can be managed because the infinite group of units has a tractable structure: it is the product of $<1, -1>$ with a cyclic group generated by $(1 + \sqrt{2})$, for which one can then find a recursive description. A "fundamental unit'" can thus be identified, which generates the whole (cyclic) group of units. In this case, it is relatively easy to find the fundamental unit; in other cases it is not so easy. For example, in the pure cubic field where one adjoins the cube root of 23 ($\sqrt[3]{23}$) to $\mathbf{Q}$, the fundamental unit of $\mathbf{Q}[\sqrt[3]{23}]$ is $2166673601 + 761875860\sqrt[3]{23} + 267901370\sqrt[3]{529}$. And when one adjoins the square root of 46 to $\mathbf{Q}$, it turns out that the normalized fundamental unit of $\mathbf{Q}[\sqrt{46}]$ is $24335 + 3588\sqrt{46}$. Dirichlet's unit theorem shows that the ring of integers for a number field K has a fundamental unit only in case K is a real quadratic field (as it is in the problem just mentioned), a complex cubic field, or a totally imaginary quartic field, that is, when the unit group has rank 1. The "rank" measures the density of the group of units within the ring of algebraic integers of K; the rank is equal to 1 when the group of units modded out by its torsion subgroup is infinite cyclic. A torsion subgroup of an infinite group is the subgroup of all the elements that have finite order. More generally, computing the ring of integers for an arbitrary algebraic number field may be very difficult.

Fröhlich and Taylor also look at the question of when a prime number p can be written in the form $p = x^2 + 6y^2$ or $\mathrm{p} = x^2 - (-6)y^2$; we recognize the same form as in the problem just discussed. In this case, the problem-solver works in $\mathbf{Z}[\sqrt{-6}]$, the ring of algebraic integers in $\mathbf{Q}[\sqrt{-6}]$, which turns out however not to be a principal ideal domain. The authors conclude, "Whilst rings of algebraic integers are not in general principal ideal domains, and so do not possess unique factorization of elements, they do still possess unique factorization of non-zero ideals: that is to say, given a number field K, every non-zero $\mathbf{O}_\mathrm{K}$-ideal [an ideal in some ring of algebraic integers] can be written uniquely (up to order) as a product of prime ideals of $\mathbf{O}_\mathrm{K}$ [that ring of algebraic integers]." (p. 4) The extent to which unique factorization fails in the ring of integers of an algebraic number field (or more generally the extent to which it fails in any Dedekind domain, an integral domain in which every nonzero proper ideal factors as a product into prime ideals) can be described by a certain group known as an ideal class group (or class group). If this group is finite (as it is in the case of the ring of integers of an algebraic number field), then the order of the group is called the class number, which registers the extent to which prime factorization fails. The class group of $\mathbf{O}_\mathrm{K}$ (the ring of all algebraic integers in K) is defined to be the group of fractional $\mathbf{O}_\mathrm{K}$-ideals modulo the subgroup of principal fractional $\mathbf{O}_\mathrm{K}$-ideals, where K is an algebraic number field. When $\mathbf{O}_\mathrm{K}$ is a principal ideal, or when we can define a Euclidean norm on $\mathbf{O}_\mathrm{K}$, this class group is trivial, that is, it is just the (multiplicative) group consisting of the element 1. More generally, the multiplicative theory of a Dedekind domain (whose class group may be infinite) is intimately tied to the structure of its class group. For example, the class group of a Dedekind domain is trivial if and only if the ring of integers is a unique factorization domain.

Here we see a general strategy. We generalize from **Z** to **Z**[i] and find that **Z**[i] has the multiplicative structure that is so valuable in **Z**, unique prime decomposition. Further generalization, to different rings of integers within different algebraic number fields, results in new concepts that help us to make precise the obstructions to unique prime decomposition. On the one hand, even where unique prime decomposition of elements fails, there is unique decomposition of every ideal in the ring into prime ideals; this gives rise to the notion of a Dedekind domain. On the other hand, there is the new concept of the class group associated with the various rings of integers, whose order measures its deviation from unique prime decomposition. The density of the pertinent units within each ring of integers, the nature of the "torsion subgroup" (a concept central to the theory of Abelian groups), and the kind of norm which it is possible to define also intervene.

Generalization precipitates new kinds of things and so do the processes that register how far each generalization departs from the original case. This is a phenomenon we find throughout mathematics. So, for example, complex analysis developed by investigating the obstructions that keep functions from being holomorphic, that is, what stands in the way of a complex function's analytic continuation on the whole plane. Topology developed by investigating obstructions that keep loops on a manifold from being continuously deformed to a point or more generally what keeps certain generalized kinds of manifolds from being continuously deformed into other canonical manifolds (like the sphere). Both the generalization (made in the hope of keeping nice properties or finding analogues to nice properties) and the diagnosis of the obstructions (which requires a relating back to the original case as the touchstone) generate new things, procedures, and methods. There is nothing about class groups or torsion subgroups in the Peano axioms or even in the axioms of set theory; these novel additions to number theory which help to solve problems about the integers **Z** within the rationals **Q** do not stand to those axioms in anything like a relation of deductive derivation: you can't deduce apples from oranges.

Exploring the Fine Structure Above Q

In sum, the task of looking for structure in rings of algebraic integers within algebraic extension fields that will be analogous to structure in the ordinary integers turns out to be very difficult. Not only does one not know exactly what to expect when heading upstairs, the redescent must also be carefully negotiated. The integers **Z** are nested inside the rationals **Q**, which in turn are nested inside the algebraic numbers **A** (sometimes written $\mathbf{Q}^{-}$ the algebraic closure of **Q**), as well as inside the completions of **Q**, the reals **R**, the complex numbers **C**, and the p-adic numbers $\mathbf{Q}_p$.

As a reminder, **Z** is a ring, while **Q**, **A**, **R**, **C,** and $\mathbf{Q}_p$ are fields, although considered as topological spaces the latter all have different properties. Thus there are very different kinds of embedding: a closure is quite different from a completion, and the different completions possible for **Q** are also all distinct. So too, depending on the field we choose from which to pick up an element to adjoin to **Z** or **Q**, and which element or elements we choose, the properties of the resultant structure are very different. Although in the cases just given, we encountered norms defined for *ring* extensions $\mathbf{Z}[i]$ and $\mathbf{Z}[\sqrt{2}]$, a norm is typically defined in terms of a field K where the quotient K[u]/K can be treated as a vector space. The structure that intervenes "sideways" then is that of a vector space. (Just as important, the automorphisms of K[u] that leave K invariant pointwise form a group called the Galois group of K[u]/K; so group theory, in particular the theory of Galois groups designed to examine permutations of roots of polynomials, also comes in sideways.)

Given a norm, defined so that one can treat the quotient of a field extension over the field as a vector space (e.g., $\mathbf{Q}[i]/\mathbf{Q}$), the norm assigns a positive length to any element of the vector space, except that the zero vector is assigned length zero; so it plays a central role in "downstairs" procedures. The canonical norm is still the Euclidean norm. Thus, Irving Kaplansky in his textbook *Fields and Rings* (Kaplansky 1972) introduces field extensions in the first section of the first part of the book, Theorem 2, in these terms: "Let K be a field, u an element of a larger field, and suppose that u is algebraic over K. Let f be a monic polynomial with coefficients in K of least degree such that $f(u) = 0$, and let this minimal degree be n. Then: (a) f is unique, (b) f is irreducible over K, (c) 1, u, u^2, ..., u^{n-1} form a vector space basis of K(u) over K, (d) [K(u):K] $= n$, and (e) a polynomial g with coefficients in K satisfies $g(u) = 0$ if and only if g is a multiple of f." In the field extension of **Q** where we adjoin i, the pertinent monic polynomial with coefficients in **Q** of least degree such that $f(i) = 0$ turns out to be $f(x) = x^2 + 1$, where f is irreducible over **Q** (there is no rational number whose square equals -1); a basis for the vector space $\mathbf{Q}[i]$ over **Q** is given by 1 and i; the degree of $\mathbf{Q}[i]/\mathbf{Q}$ is 2; and if $g(i) = 0$, then g is a multiple of f. Similarly, when we adjoin $\sqrt{2}$ or $\sqrt{-6}$ to **Q**, the resultant vector space will be of finite dimension, though in general it may not be easy to locate the "monic polynomial with coefficients in **Q** of least degree" in order to determine its degree and therefore the dimension of the vector space.

We can view the construction and exploration of algebraic extensions of **Z** and **Q** as a passage to the wider domains of **A**, **R**, **C**, and $\mathbf{Q}_p$. But we can also view this mathematical activity as an occasion to construct novel objects like $\mathbf{Q}[i]$, Gal ($\mathbf{Q}[\sqrt{2}]/\mathbf{Q}$), or the class group of $\mathbf{O}_{\mathbf{Q}[\sqrt{-6}]}$ in order to explore their properties, a search that takes us far beyond **Q** itself, even when it is motivated by problems originally arising in **Q**. And while $\mathbf{Q}[i]$ is a field that presumably lives somewhere between the fields **Q** and **C**, we can also think of it as a vector space; the Gaussian integers form a ring, and Gal ($\mathbf{Q}[\sqrt{2}]/\mathbf{Q}$) is a group: they are located sideways, as it were, from the tower of nested fields. We can also view this activity as an exploration of the fine structure of the larger fields: for example, all the possible $\mathbf{Z}[u]$ and $\mathbf{Q}[u]$ (u algebraic) might be regarded as the fine structure of **A**, which presumably "contains" all such extensions.

But this notion of containment is different both from the way the algebraic numbers themselves are contained in **A** and the way one field is contained in another. Can we think of **A** as **Q** with every root of every monic polynomial with rational coefficients adjoined, $\mathbf{Q}[u_1, u_2, \ldots u_n, \ldots]$, where the u_n are all the (countably many) algebraic numbers? What can be gained from thinking of **A** in terms of the quotient $\mathbf{Q}[u_1, u_2, \ldots u_n, \ldots]/\mathbf{Q}$, an infinite dimensional vector space, or in terms of all the automorphisms of $\mathbf{Q}[u_1, u_2, \ldots u_n, \ldots]$ that leave **Q** invariant, a Galois group of infinite order? And what is the relation of $\mathbf{Q}[u_1, u_2, \ldots u_n, \ldots]$ to the various smaller entities $\mathbf{Q}[u_1]$, $\mathbf{Q}[u_2]$, $\mathbf{Q}[u_1, u_2]$, and so forth? What are the relations of those smaller entities to each other? Here are two theorems pertinent to these questions, but they are only the tip of the iceberg: If two algebraic elements u and v over a field F generate the same extension field $\mathrm{F}(u) = \mathrm{F}(v)$, then u and v have the same degree over F. If n elements u_i form a basis for a finite extension K of a field F, while m elements w_j constitute a basis for an extension L of K, then the mn products $u_i w_j$ constitute a basis for L over F. My point is, once again, that going upstairs from **Z** or **Q** to algebraic number fields in order to solve certain problems that arise in number theory is not merely going upstairs to **A**, **R**, **C**, and $\mathbf{Q}_p$. The problem-solving is ampliative, going up, going sideways, and coming down.

After an introductory chapter that introduces the vocabulary of abstract algebra (fields, algebras, and rings), Fröhlich and Taylor discuss Dedekind domains in Chapter II, along with "downstairs" maps (back down to **Z**) like valuations and absolute values; "upstairs" embeddings—completions—generalizing from the embedding of **Q** in **R**, so that sequences that satisfy Cauchy's convergence criterion have a limit; and module theory, a generalization of the notion of a vector space, with rings instead of fields. Chapter III is devoted to field extensions, with a detailed analysis of how prime ideals factorize in various cases (explained in terms of Galois groups). Chapter IV is devoted mostly to units and class groups, though with an oddly geometrical section on lattices in Euclidean space. This is noteworthy because Fröhlich and Taylor try very hard to keep geometry and real and complex analysis out of this book: this is a book about *algebra*. They then conduct the reader into a more detailed study of fields of low degree (Chapter V deals with quadratic, biquadratic, cubic, and sextic fields) and then of cyclotomic fields (Chapter VI), where, significantly, quadratic fields are revisited and the quadratic reciprocity law is reproved. The quadratic reciprocity law asserts a relationship between the congruences $x^2 \equiv p \pmod{q}$ and $x^2 \equiv q \pmod{p}$: they are both solvable or both unsolvable unless $p \equiv q \equiv 3 \pmod{4}$, in which case one is solvable and the other isn't. It was Gauss's favorite theorem: in the *Disquisitiones Arithmeticae* (1801), he called it the fundamental or golden theorem, and by the time he died, he had proved it in eight different ways. In this essay, in three different ways, I have tried to show why a strong commitment to following the detail of mathematical practice, as Reuben Hersh urges us to do, is the best way for philosophers to explain how mathematical knowledge grows, from the double root of numbers and figures.

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Do Mathematicians Have Responsibilities?

Michael Harris

I have been an admirer of Reuben Hersh ever since I received a copy of *The Mathematical Experience*, then brand new, as a birthday present. At that stage, of course, I was admiring the tandem Reuben formed then, and on other occasions, with his coauthor Philip J. Davis. It was only almost 20 years later, after I started reading *What is Mathematics, Really?*, that I could focus my admiration on Reuben—and not only on the mathematician, the author, and the thinker about mathematics but on the person Reuben Hersh—the unmistakable and unforgettable voice that accompanies the reader from the beginning to the end of the book. So unforgettable was the voice, in fact, that when Reuben wrote to me out of the blue three years ago to ask me what I thought about a certain French philosopher, I so clearly heard the voice of the narrator of *What is Mathematics, Really?* (and no doubt of many of the passages of his books with Davis) that I could honestly write back that I felt that I had known him for decades, though we have never met and until that time we had never exchanged a single word.

The voice in question is the voice of an author who is struggling to put words on an intense and intensely felt experience and who has intimate knowledge of how it feels to be a mathematician and also a knowledge no less intimate of the inadequacy of the language of our philosophical tradition to do justice to that experience, so that all attempts to do so inevitably end in failure; but this knowledge is compensated by the conviction that the stakes are so important that we can't choose not to try. What makes Reuben's authorial voice compelling is that it sounds just as we

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expect the voice of a person in the middle of that struggle must sound.[1] It's the strength of this conviction that comes across in Reuben's writing, so that reading his books and essays is remembered (by me, at least) as a conversation, a very lively conversation, filled with the passionate sense that we are talking about something that *matters*. A conversation also filled with disagreements—because I don't always agree with everything I read in Reuben's books and essays. Beyond questions of detail, the difference might come down to my sense that Reuben is trying to *get to the bottom* of the mathematical experience, whereas I apprehend the experience as bottomless; or I might say that it's the effort to get to its bottom that is at the bottom of the experience. But the differences are of little moment; what stays with me after reading a few pages of Reuben's writing is the wholeness of the human being reflected in his words, a human being who cares so deeply about his mathematical calling that he is ready to add his own heroic failure to the long list of admirable failures to account for mathematics by the most eminent philosophers of the Western tradition; and without these inevitable failures, we would not begin to understand why it does matter to us.

It's not a coincidence that Reuben's philosophy of mathematics is human centered, that it takes as an essential and not an incidental feature of mathematics that it is an activity of human beings, and that his writing is an expression of an entire human being. Inspired by his example, I was looking forward in this essay to devising a philosophical failure of my own, which might nevertheless hint at something about mathematics that deserves further scrutiny. That was what I had in mind on October 24, 2016, when I agreed enthusiastically to contribute to this *Festschrift* and specifically to address what might be called the founding dogma of humanistic mathematics, namely, that anyone who claims that human mathematicians would be replaced by computers (as Paul Cohen reportedly claimed to Reuben 40 years ago) has failed to grasp what mathematics (really!) is and probably has a shaky understanding of human beings as well. But shortly after October 24, an Event took place that has been disturbing the sleep of everyone I know, as well as a great many people I don't know—every public lecture or round table I have attended in the intervening period has allusions to the Event, with the partial exception of mathematical lectures—and I can't help feeling that any expression of confidence in the future of human mathematics would be more convincing if I could find more solid reasons for continued confidence in the future of humans.

So I have (in the spirit of the times) made a deal with myself, and I hope Reuben will not object. I will set down my thoughts on the social responsibilities of mathematicians, which I feel compelled to discuss in view of the circumstances. In so doing, I will attempt to justify my belief—straining logic and credulity and readers' patience if necessary—that the way we approach questions of objectivity in

[1] As I wrote that sentence I remembered that I have still not met Reuben, nor have I ever spoken to him; but I checked one of the videos online in which he appears, and, sure enough, his literal voice is very much as I expected.

mathematics in the dominant strand of English-language philosophy, a perspective Reuben has questioned so vigorously, is an obstacle to taking our responsibilities seriously, as well as to understanding the point of the mathematical experience.

I understand why so many of my colleagues argue that mathematics and politics should not mix and why the mathematical profession should avoid taking political stands. The language mathematicians share ignores political differences, and our professional ethics compel us to recognize the contributions of colleagues with whom we may disagree profoundly on everything that is not specifically mathematical. This characteristic ethos of the profession, with which I think most of my colleagues agree, is often used to promote the position that the profession should maintain strict neutrality with regard to political questions and specifically that it is as individual citizens, and not as members of a professional community, that we should address the applications and implications of our work, as researchers, as teachers, and as participants in the institutions that make our work possible.

I think that position has never been tenable, either philosophically or morally, and developments during the past ten years have made this spectacularly clear. But before I remind the reader of some of these events, I want to discuss an older story, one in which the mathematical sciences play at most a supporting role, but that I think illustrates well how philosophical confusion about the nature of mathematics can interfere with informed judgment. Here is a sentence that, syntactically at least, looks like a legitimate question to which scientific investigation can be applied.

Does Mathematical Talent Have a Genetic Basis?

On the one hand, the answer is obviously yes: bonobos and dolphins are undoubtedly clever, but they are unable to use the binomial theorem. The question becomes problematic only when the attempt is made to *measure* genetic differences in mathematical talent. Then one is forced to recognize that it is not just one question innocently chosen from among all the questions that might be examined by available scientific means. It has to be seen against the background of persistent prejudices regarding the place of women and racially defined groups in mathematics. I sympathize as much as anyone with the hope that the study of the cognitive and neurological basis of mathematical activities can shed light on the meaning of mathematics—and in particular can reinforce our understanding of mathematics as a *human* practice—but given how little we know about the relation between mathematics and the brain, why is it *urgent* to establish differences between the mathematical behavior of male and female brains? The gap is so vast between whatever such studies measure and anything resembling an appreciation of the difficulties of coming to grips with the conceptual content of mathematics that what really needs to be explained is why any attention, whatsoever, is paid to these studies. Ingrained prejudice is the explanation that Occam's razor would select. But I've heard it argued often enough, by people whose public behavior gives no reason to suspect them of prejudice, that it would be unscientific to refuse to examine the possibility that the highlighted question has an answer that might be politically awkward. It's the numerical form of the data, I contend, and the statistical expertise brought to bear on its analysis that provide the objectivity effect, the illusion that one's experiment is actually measuring something objective (and that also conveniently forestalls what ought to be one's first reaction: why has Science devoted such extensive resources to just this kind of question?). The superficially mathematical format of the output of the experiment is a poor substitute for thought. Maybe something is being measured, but we have only the faintest idea of what it might be.

This example, which is only mildly hypothetical, has the advantage of highlighting how an illusion of objectivity, produced by dressing up a question that is not necessarily meaningful as a quantitative measurement, is linked to the failure to reform the philosophy of mathematics to account for what mathematical talent is (really). And that, I submit, is why Reuben Hersh's project becomes profoundly ethical.

Here is another example that demonstrates why the development of humanistic mathematics is urgent in a way that studying hypothetical gender differences in mathematical behavior of human brains is most definitely not. The prediction by Paul Cohen that so irritated Reuben is constantly echoed in a journalistic narrative that is transparently driven by corporate priorities. An article by Elizabeth Kolbert, entitled *Our Automated Future*, in the Books section of a recent issue of *New Yorker* posed a rhetorical question:

> "What business will want to hire a messy, complex carbon-based life form when a software tweak can get the job done just as well?"[2]

The books Kolbert reviews highlight the looming threat of mass unemployment on a catastrophic scale—"nearly half the occupations in the U.S. are 'potentially automatable,'" she writes, and "this could play out within "a decade or two." This already ought to inspire mathematical scientists to start thinking about our social responsibilities, and I'll have more to say about that in a moment. Mathematical research itself, meanwhile, could be a collateral damage of the "automated future's" version of the bottom line, if the vision of Paul Cohen (and of increasing numbers of our colleagues) is realized. Provided, that is, that Cohen's implicit vision of what it means in mathematics to "get the job done" also comes to dominate and that mathematical research is no longer understood as synonymous with *human* mathematical research. Or as Kolbert wrote, in a somewhat different context, "if it's unrealistic to suppose that smart machines can be stopped, it's probably just as unrealistic to imagine that smart policies will follow. "

The implications of the arrival of "smart machines" were brought home to me a few months ago at the New York Psychoanalytic Institute, of all places, during a round table discussion entitled "Embodied AI." While the panel was billed as a report on AI's promise to "augment individual human senses and abilities, giving that technology platform the ability to see a patient's complete medical

[2]Elizabeth Kolbert, *New Yorker*, December 19 and 26, 2016 Issue.

condition, feel the flow of a supply chain, or drive a factory like a maestro before an orchestra," the discussion rapidly veered to ethical matters. We were naturally reminded that HAL 9000, in *2001: A Space Odyssey*, thought he was being a pretty "smart machine" when he computed that the best way to "save the mission" was to wipe out the crew. In connection with this kind of risk, among others no less alarming, it was announced that Facebook, IBM, Amazon, Google, and Microsoft had just formed the "Partnership on AI" for the purpose of "conducting research and promoting best practices." Mentioned in passing was the likelihood that rapid progress in "embodied" artificial intelligence would lead to the replacement of a large proportion of human workers by robots, as Kolbert predicted. Someone in the room was not convinced that the definition of "best practices" should necessarily be left to the tech giants that had come together in the Partnership on AI[3] and asked a question: had it occurred to none of the speakers that a process they saw as inevitable ought to be subject to democratic oversight? Why are decisions with such grave long-term implications being left to a handful of corporations with massive resources at their disposal? In response,[4] a historian recited the familiar story of Gandhi and his promotion of handloom weaving during the Indian independence movement; he called Gandhi's intentions "noble" and used the word "resistance" but only to conclude that it was futile.[5]

I draw three lessons from this brief exchange. The first is that the panelists had internalized a purely *instrumental* view of human activity, the presumption that humans work in order to "get the job done"; on instrumental grounds they are therefore expendable. The implications of this worldview are so repellant that it is to be hoped that more attention will be given to how it converges with the dominant ethos in Silicon Valley, where the sum total of human experience is treated as data to be mined for content. Kant's dictum from the *Groundwork of the Metaphysics of Morals* has been rescinded, and it's now OK to treat human beings as a means rather than an end. Applying this form of instrumental reason to mathematical research is the main mistake made by Paul Cohen in his comment to Reuben and by those of our contemporaries who agree with Cohen's prognosis; this is where the panel discussion becomes a challenge to humanistic mathematics.

The second lesson is that those who promote the instrumental view of human activity have little sympathy for democratic decision-making. In the context of

[3]Now joined, predictably, by Apple. In all fairness, I should add that on January 27 the Partnership on AI, which had reported no news during the previous three months, has added six independent members to its Board of Trustees, including a representative of the ACLU. I am cautiously optimistic. The outreach to civil society does not invalidate the impressions I took home from last October's panel.

[4]The word "Luddite" had been pronounced earlier, and hung over the discussion, as if to reinforce the sense that the social transformations the panelists were discussing were foreordained; the question I just quoted—I happen to be the one who asked it—was the only one to challenge this claim of inevitability; and many of the original Luddites were also handloom weavers.

[5]He seemed to have forgotten that Gandhi's movement was primarily a reaction to colonialism, a strange oversight for a historian.

the meeting on "Embodied AI," it would probably be more accurate to say that sympathy is beside the point; the panelists all appeared to be convinced that technological determinism trumps democracy. When resistance is futile and the new technology is on its way whether we like it or not, the best we can do—our only option, really—is to leave management of the impending social dislocations in the hands of those best equipped to steer the transformation consistently with our principles—the latter being identified, of course, by the billion-dollar corporations that stand to benefit the most.

The third lesson, most important for our purposes, is that the primary qualification for membership in the steering committee of our inescapable technological future is a command of relevant quantitative sciences. Progress is a wave and you either ride it or go under; and you learn to ride the wave by mastering complex mathematical theories. Ethical principles not backed up by calculations stand no chance against the people with the clipboards. The experts may appear arrogant when they dismiss your concerns; but the fact is that they know more than you, so why should your opinion count as much as theirs?[6]

Once mathematics, narrowly conceived as the gathering and analysis of quantitative data, is accorded the role of the sole standard of *objectivity*, there indeed seems to be no alternative—in the spirit of Margaret Thatcher's *There Is No Alternative*—to surrendering control not only of democratic decision-making but of *meaning* itself, to mathematically trained experts. This is not a novel observation.[7] Here, for example, are Horkheimer and Adorno[8] on what happens when one reduces "thought to a mathematical apparatus":

> *What is abandoned is the whole claim and approach of knowledge: to comprehend the given as such; not merely to determine the abstract spatio-temporal relations of the facts which allow them just to be grasped, but on the contrary to conceive them . . . as mediated conceptual moments which come to fulfillment only in the development of their social, historical, and human significance . . . .*

More recently, Achille Mbembe[9] pictured the "21st century political landscape" as a Big Data apocalypse:

> *In this new landscape, knowledge will be defined as knowledge for the market. The market itself will be re-imagined as the primary mechanism for the validation of truth.*
>
> *As markets themselves are increasingly turning into algorithmic structures and technologies, the only useful knowledge will be algorithmic.*
>
> *Instead of people with body, history and flesh, statistical inferences will be all that count. . . . The new human being will be constituted through and within digital technologies and computational media.*

[6]This is not to suggest that expertise should give way to the populism of a strongman, but rather that it's incumbent on experts to be less arrogant.

[7]For Jane Austen, it was the *mathematician* who had the "coldest heart and the steadiest brain." *Emma*, Volume III, Chapter 3.

[8]*Dialectic of Enlightenment*, New York: Continuum (1994) pp. 26–7.

[9]"The age of humanism is ending," *The Mail and Guardian*, December 22, 2016.

If I have devoted so much space to the implications of "Embodied AI" it's because I happened to be on hand at a meeting where the three lessons just outlined were displayed with exemplary clarity. And one has to suppose that a volume devoted to mathematical humanism would value the inclusion of a human perspective on post-humanism, even if it is only my own. But the same three lessons can be drawn in any of the increasingly frequent situations in which mathematics-based technology has come into conflict with democratic principles. While pure mathematicians in particular may have wondered whether much of their work would ever be socially useful, it was generally believed that at least it caused no harm.[10] Events of recent years have called that belief into question. The sophisticated and often opaque derivatives developed by financial mathematics magnified the effects of a downturn in sectors of the US housing market into a global financial crisis whose consequences are still with us. Edward Snowden's revelations in 2013 served as a reminder that contemporary cryptographic techniques based on number theory can also be used to facilitate general surveillance by governments. The rapid growth of Big Data has made it possible for commercial as well as public actors to track individual behavior with increasing precision, with grave implications for privacy.

In each of these applications of mathematics, one finds the same three features that were visible in that brief panel discussion on "Embodied AI": an approach to human activity that is purely instrumental (serving the interests of the market or of government surveillance; of course there are also military applications, but they are not especially new), a disdain for democratic decision-making, and the empowerment of experts on the basis of their mathematical training. And in each case, a few mathematical scientists have pointed out that the power of mathematical technology imposes social responsibility on those who develop it, beyond putting trust in experts. Responses have been as varied as the *Code of Ethical Conduct for Virtual Reality Research*, formulated by Michael Madary and Thomas K. Metzinger[11]; the *Hippocratic Oath* for financial modelers, proposed by Emanuel Derman and Paul Wilmott[12]; Cathy O'Neil's suggestion for an analogous *Hippocratic Oath*[13] for data scientists and developers of algorithms; the debate sponsored by the *Notices of the American Mathematical Society* on the role of the NSA, in the wake of the Snowden revelations[14]; and the reactions of a number of

[10]Nearly 30 years ago, in an article entitled "A Hippocratic Oath for Mathematicians?" (in Christine Keitel, chief editor, *Mathematics, Education, and Society*, Science and Technology Education, Document series No. 35, UNESCO (1989)), Chandler Davis was already suggesting that the harmlessness of the work of pure mathematicians deserved closer examination. Davis's article mainly referred to military applications; those considered here mostly concern the civilian sector, though no one can ignore the military implications of "Embodied AI," for instance.

[11]*Frontiers in Robotics and AI*, 19 February 2016, volume 3, article 3, www.frontiersin.org.

[12]in the *Financial Modelers' Manifesto*, see https://en.wikipedia.org/wiki/Financial_Modelers'_Manifesto and the references given there.

[13]*Weapons of Math Destruction*.

[14]See http://www.ams.org/notices/201504/rnoti-p400.pdf and the references indicated there.

French mathematicians to public blaming of mathematics for the 2008 financial crisis. The spirit of these oaths and codes is concisely summarized by Amaury Lambert and Laurent Mazliak.

> *As long as no one calls into question the ultimate goal of a technique, it can persist on its own, and it remains impossible to dissociate its harmful effects from its positive effects. Moreover, in order to guarantee the correct usage of financial techniques, it is not only necessary to define what that means; we must also all be prepared to refuse to cooperate if that usage appears to us to have been hijacked.*

Their specific target is financial mathematics, but their words apply to all the technologies mentioned above and to many others not yet conceived. Lambert and Mazliak add:

> *Rather than taking the time to question the aims of participation in the game of financial mathematics, efforts have been made to throw all our weight behind it, and we shielded ourselves from the consequences behind a supposed neutrality.*[15]

As I wrote, I don't believe this neutrality is tenable.

Reuben Hersh has done as much as any living mathematician to remind us how exciting it is to pay attention to the philosophical challenges inherent in our profession. I want to close by pointing out two substantial challenges that are authentically *philosophical* and that mathematicians will have to overcome in order to formulate a coherent commitment to socially responsible behavior. The first is an uncritical acceptance of conventional standards of "usefulness" as they apply to mathematics. My book *Mathematics without Apologies* devoted much of chapter 10, and nearly all of chapter 4, to analyzing the risks and contradictions of adopting a purely instrumental understanding of what it means for mathematics to be "useful," especially when the goals for which mathematics is meant to serve as an instrument are absurd or socially destructive. The most hostile reviews of my book saw these passages as proof of my irresponsible elitism, or my elitist irresponsibility, warning of dire consequences if government funding agencies were to realize that pure mathematical research is largely not aimed at generating what decision-makers find useful, whether it be new life-saving therapies or new techniques of mass surveillance. No consensus on norms of social responsibility is possible if the word "useful" is deemed to be neutral and off-limits for philosophically analysis.

The second challenge is more difficult still, because it goes to the heart of the philosophical disorientation that surrounds mathematics and that Reuben has explored in so many of his writings. The insistence on political neutrality is sustained in the minds of many mathematicians by the four Myths Reuben identifies in *What is Mathematics, Really?*—and especially by Myths 3 and 4—certainty and objectivity. While it can't be denied that the promises of mathematical certainty and

[15]Amaury Lambert, Laurent Mazliak, E la nave va?, *Gazette des mathématiciens,* **120**, avril 2009, pp. 103–5. My loose translation.

objectivity are a source of comfort—especially in an era of "alternative facts"[16]—Reuben argues that mathematicians are well aware that they are Myths, though we may wish it were not so:

> *Mathematicians want to believe in unity, universality, certainty, and objectivity, as Americans want to believe in the Constitution and free enterprise, or other nations in their Gracious Queen or their Glorious Revolution. But while they believe, they know better.*[17]

Nevertheless, being entrusted with power by virtue of our role in the transmission of mathematical knowledge imposes the responsibility to insist on the limitations of that knowledge. The ideology of mathematical certainty and objectivity is our most potent weapon; we should not allow it to be used to undermine democracy. With regard to mathematical modeling, we should constantly remind anyone who is willing to listen that a model is not objective or scientific just because it is mathematical. As Cathy O'Neil writes in regard to the Big Data algorithms she calls "Weapons of Math Destruction":

> *Though economists may attempt to calculate costs for smog or agricultural runoff, or the extinction of the spotted owl, numbers can never express their value. And the same is often true of fairness and the common good in mathematical models. They're concepts that reside only in the human mind, and they resist quantification.*[18]

I appreciate Thomas Piketty's bluntness in emphasizing how an unquestioning belief in the objectivity of mathematical formalism has damaged critical thinking in economics:

> *To put it bluntly, the discipline of economics has yet to get over its childish passion for mathematics and for purely theoretical and often highly ideological speculation, at the expense of historical research and collaboration with the other social sciences. ... This obsession with mathematics is an easy way of acquiring the appearance of scientificity without having to answer the far more complex questions posed by the world we live in.*[19]

Reuben Hersh has tirelessly challenged us to look at what lies behind the appearances of scientificity in mathematics. For this we should all be grateful to him.

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[16]Nevertheless, see this from Fox News, in 2011: "the talk of the new year is this repealing Obama-care .... The debate should be about the liberals ... trying to repeal the laws of math and physics. "http://www.morrisanderson.com/resource-center/entry/Boehner-Offers-Evidence-Obama-care-is-Job-Killer-Spending-Trillion-on-Plan-/.

[17]Hersh, Reuben. What is Mathematics, Really?. Cary, US: Oxford University Press (US), 2001. ProQuest ebrary. Web. 4 February 2017, p. 39.

[18]*Weapons of Math Destruction*, New York: Crown (2016) p. 207.

[19]*Capital in the Twenty-First Century*, Cambridge, MA: Belknap Press (2014) p. 32. Also, on p. 574: "For far too long economists have sought to define themselves in terms of their supposedly scientific methods. In fact, those methods rely on an immoderate use of mathematical models, which are frequently no more than an excuse for occupying the terrain and masking the vacuity of their content."

School Mathematics and "Real" Mathematics

Bonnie Gold

I was delighted to be invited to contribute to this festschrift for Reuben Hersh. I've known and (usually) admired Reuben's work in the philosophy of mathematics since I read his book written with Philip Davis, *The Mathematical Experience*, and have known him since 2001, when he was on a panel that Joe Auslander and I put together on the philosophy of mathematics at the winter joint mathematics meetings. Since that time, he has frequently shared drafts of writings on the philosophy of mathematics with me, and I have much appreciated the interchanges. His thinking has evolved in interesting ways; I thought his talk for a recent Calcutta conference on "Pluralism in the philosophy of mathematics" (in this volume) was a wonderful contribution.

Why "What Is Mathematics?"

I have been thinking about the question "What is mathematics?" since I finished my Ph.D. thesis (in mathematical logic) in 1976. I was strongly influenced by an undergraduate teacher of mine, Stanley Tennenbaum, a mathematician and platonist. I realized that, although by that time I knew a lot of mathematics, had Socrates come to me and asked me this question, I would not have been able to provide an acceptable answer. At one time, Reuben took a stab at an answer (in [2]): "the study

This paper is a revision of a talk I gave, "Is School Mathematics 'Real' Mathematics?" as the POMSIGMAA invited speaker at the 2016 joint winter mathematics meetings.

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of mental objects with reproducible properties is called mathematics." At another time, he wrote "Mathematical entities or concepts are equivalence classes of mental models.... A student is accepted as competent by the mathematical community if, by passing the tests, she has demonstrated that her mental model of the concept is congruent to the standard one of the community" [4]. More recently, Reuben, in his Pluralism paper [5], discourages even attempting to answer this question: "'Taking a position' on the nature of mathematics looks very much like the vice of essentialism–claiming that some description of a phenomenon captures what that phenomenon 'really is,' and then trying to force observations of that phenomenon to fit into that claimed essence."

While I don't expect to be able to answer the question once and for all (it's rare to answer any philosophical question in the definitive way that we can answer mathematical questions), I do believe that this is potentially a very productive time in the history of mathematics to reconsider it. Philosophers have largely discussed the question from the perspective of what the nature of the objects of mathematics is; I am not proposing yet another take on that approach. However, in the last century or so, a wide range of new areas of mathematics have been developed (topology, algebraic geometry, mathematical logic, combinatorics, for example, each of which had roots in earlier mathematical questions or activities, but were primarily developed in the last century), and several fields that are not classified as mathematics but that clearly are related to mathematics have appeared (e.g., computer science, operations research, mathematical biology). So it seems to me that now is a good time to examine areas that are classified as mathematics, and those that are not, to try to determine what properties those that do join the category of mathematics have that those that are not considered mathematics lack.

How I Plan to Approach the Problem

I have written a paper setting forth my overall plan of approach [3], which I'm in the process of revising and hope to publish soon, but in this contribution, I am going to look at one area related to mathematics and examine to what extent it does belong with mathematics, and to what extent it does not, as part of this overall work on the question of what mathematics is. I'm assuming, as I do this, that the audience is primarily mathematicians, who have a pretty good sense of what mathematics is—we know it when we see it—even if we can't completely translate that sense into a verbal description.

Turning to School Mathematics

The area I am going to consider here is the mathematics that is taught at the school level, that is, from kindergarten through high school. To what extent is that material "real" mathematics?

One might reply, "Of course it's mathematics; after all, that's what we call it." Well, sort of. When I was in school, until we started algebra in ninth grade, the subject was actually called arithmetic. And I think there was a reason for this distinction. What I learned was a combination of ritual and memorization. You memorize the tables for addition and multiplication of single-digit numbers (or, for those a half-generation older than me, the numbers 1 to 12), and then you learn certain algorithms for adding, subtracting, multiplying, and dividing multidigit numbers. (As taught in the schools, it would appear these are the only possible algorithms; but different countries teach different algorithms.) Next you learn additional procedures for performing these operations on fractions and terminating decimals. (Of course, terminating decimals *are* fractions, but the algorithms children are taught for working with them are completely different!)

We all worry about the fact that students (especially if they haven't taken calculus in high school, but often even if they have) come to college thinking they want to major in mathematics, and then when they get there, and start taking our upper level courses, realize that this isn't the subject they thought it was. And yet, they've been studying "mathematics" for twelve or more years by then. So is school mathematics *real* mathematics? Can examining it help us think about what mathematics is, really?

So, let's turn to school mathematics. I'm going to consider four different topics within this.

A. Traditional Elementary School Arithmetic

When I was in school, back in the 1950s, we learned to count in kindergarten, to add in first grade, subtract in second grade, multiply in third grade, divide in fourth grade (I was fortunate enough to skip fourth grade; I learned long division on the first day of fifth grade), fractions in fifth grade, and decimals and percents in sixth grade. (Seventh and eighth grades were review; algebra came in ninth grade.) The emphasis was on the manipulations:

Compute $5364 \div 78$—here is most of it, in several steps:

$$
\begin{array}{r}
6 \\
78\,\overline{)\,5364}
\end{array}
\qquad\qquad
\begin{array}{r}
6 \\
78\,\overline{)\,5364} \\
-\,\underline{468}
\end{array}
$$

$$
\begin{array}{r}
68 \\
78\,\overline{)\,5364} \\
-\,\underline{468} \\
684 \\
\underline{-\,624}
\end{array}
$$

To divide 5364 by 78, you observe that since $78 > 53$, you must try to divide 536 by 78; that is, what is the largest single-digit multiple of 78 that is no larger than 536. You place that digit (which is a 6) above the 6 in 5364, multiply it by 78, place that result below the 536, and subtract it from 536. Then you bring down the 4, and repeat the process, putting the 8 above the 4. If you're at the stage of dealing with decimals, you might then put a decimal point to the right of the 4 in 5364, and some zeroes after it, and put a decimal point after the 8 in the quotient, bring down a 0, and continue for a while; if you're not at that stage, you'd subtract 8*78 and what is left (60) is called the remainder, so that the answer is 68, remainder 60, or $68\frac{60}{78}$. There was no discussion, or very little, of the meaning of all this manipulation. Why can you start by ignoring the 4 and treating it as if you were simply dividing 536 by 78? What's this "bring down the 4"? In the middle of the last century, the only calculators were about twice the mass of a typewriter (for you younger folks, that's a mechanical word processor in which the printer is mechanically attached to the keyboard, with the CPU being the human operating it). Few people had them. So most people needed to do addition and subtraction by hand or mentally on a regular basis, and many needed to be proficient at multiplication and division—hence the year spent practicing each. We also learned methods to check our work, since it was important to consistently get the answers right, but there was very little, if any, explanation of why these algorithms worked or what they meant.

Now, I'm not denigrating this activity: in a time before the widespread availability of electronic calculating machines (much less computers), there was a need for it. And it took humanity several thousand years to develop relatively efficient algorithms for these calculations—imagine trying to do the division problem I just mentioned in roman numerals!

But I would argue that this sort of activity is *not* mathematics, any more than predicting the coming of the messiah by giving numerical values to the various letters of the Hebrew alphabet and doing some computations with the Bible is doing mathematics. It's using mathematical developments for some human purpose, but it isn't doing mathematics. Why do I say this? What makes it seem not mathematics, to me, is that it lacks ideas, concepts, understandings, interrelations among meanings. In fact, slogans were developed to tell folks, "don't worry about any meaning: just do it"—"Yours is not to reason why: just invert and multiply" for division of fractions, for example. I think those of us who are engaged in mathematics, whether professionally or even as amateurs, feel that meaning and understanding are central to doing mathematics.

Indeed, this view is shared by some school teachers involved in teaching the "New Math" in the 1960s, not just university-level mathematicians, as exemplified by this quote from an eighth-grade teacher [1, p. 470]: "Teaching the SMSG materials has made me realize that before using these texts, I was not teaching mathematics at all; I was simply teaching manipulative skills. Now I feel that I have been teaching at least some mathematics."

Not that I'm saying that inherently elementary school arithmetic isn't mathematics—but I think that a certain approach to it isn't. A little while after I had moved to junior high school, the "New Math" took over. This movement began in many universities around the country; its aim was to put meaning into mathematics. The New Math began soon after the end of World War II, but became supported nationally and moved to the forefront after the launch of Sputnik in 1957. Its approach to putting meaning into school mathematics was partly motivated, I think, by the foundational difficulties mathematics itself had run into and by the success, among some mathematicians, of the Bourbaki-style formalist approach, together with axiomatic set theory. So the proponents started with a set theory and functions approach—counting consisted of one-to-one correspondences between sets, for example—as well as working in different number bases to give an understanding of arithmetic manipulations. But many proponents of the New Math also advocated a pedagogy of active learning, which, unlike the formalist set theory approach, I view as a definite improvement. Another eighth-grade teacher, teaching the New Math: "Most of the students of above-average ability reacted well to the material, obviously being glad to have something besides pages of problems on which to use their minds. *Some of the most striking cases of success, however, came from the ranks of students of low ability who made progress because they understood why they were performing certain operations*" ([1], italics added). A much more serious attempt was made with the New Math, than is currently being made with the Common Core State Standards, to bring teachers to the point that they were able to teach this material. (There were many summer institutes for teachers, largely sponsored by the NSF.) But its very alien, formal, from the intellectuals-to-the-masses French approach was totally rejected by parents, leading to a "back-to-basics" movement—"if this is what meaning in mathematics is, let's get rid of it!"

B. Traditional High School Mathematics

Much of high school mathematics has also been taught from a purely manipulative approach, and I would argue that it is then also only minimally mathematics. High school algebra is especially vulnerable to this. First, we introduce many new notations, often with little explanation of what they mean or the rules under which they operate, or even with ambiguous rules that change in what appears, to many students, arbitrary ways. For example, we use letters to represent assorted objects (generally at first, numbers, but later functions and more), and we do so in many different contexts. We say that "$3(x + y) = 3x + 3y$" is an identity, the distributive property, whereas we usually say that "$\sin(x + y) = \sin x + \sin y$" is an error—misusing the distributive property—though it's often not explained why the distributive property isn't applicable here. However, the latter equation also could be asking us to find all x and y with that property. A lot of our notation is similarly ambiguous: the letter x can be a variable quantity in the definition of a function, an unknown quantity to be solved for, an arbitrary number in an expression for an identity, etc.

Second, often very little emphasis is placed on the concepts involved: that there is usually a purpose in manipulating an equation, such as to keep the same solution set while replacing the equation by one where the solution is easier to obtain or that gives insight into what the expressions represent; that if an equation is satisfied by some object, to keep this true, what is done to one side must be done to the other; that if something doesn't work for numbers, it won't work for variables (but some things won't work for variables that do for numbers). Of course, one doesn't have to teach algebra in this mindless way, but teachers who haven't made the connections—between the symbol-pushing and the concepts those symbols are representing—are unlikely to make them for their students.

So traditional high school mathematics is often taught in a way that is not mathematics. This is one reason I have pushed to not have "college algebra" count for general education credit in mathematics at the university level—because even if there are some concepts there, they tend to become overwhelmed by the manipulative activities.

On the other hand, the concepts are closer to the manipulations at the high school level than at the elementary school level, and some subjects, such as geometry, almost inevitably require consideration of concepts. Yet even geometry can be taught in a deadening way that makes it, at best, borderline mathematics. For many, the formality of two-column proofs, memorizing definitions, axioms and postulates, and so on, deadens the potential pleasure of discovering new mathematical results, interconnections, or justifications of observations. But inherently it seems much closer to "real" mathematics.

As I have been discussing school mathematics, I have been identifying some qualities that make something mathematics, or make it "not really mathematics." So maybe we're making some progress. Mathematics seems to be about ideas or concepts of a certain kind—among them, ideas that are preserved under some kinds of transformations. And mathematics seems to involve exploring these ideas, not just repeating or memorizing them. In particular, mathematics is not symbol manipulation devoid of a relationship to what those symbols represent. Symbols are important, for several reasons. We have learned that, once we know how that symbol manipulation affects the concepts, we can, in fact, manipulate the symbols without referring to their meaning much more efficiently than we can manipulate the concepts. Thus, learning how to manipulate symbols has been an important part of mathematical development. Further, in some circumstances, we can learn new relationships simply from manipulating those symbols (e.g., when, in dealing with a differential equation that we don't know how to solve, we say, "well, what if we had a solution in the following form: what would the solution then have to be?"). Nonetheless, the manipulations themselves are not mathematics: it's the concepts they are representing that is where the mathematics lies.

To examine whether it is possible to teach mathematics at the school level in such a way that it participates more in real mathematics (and to use it as a foil in trying to get a better description of what mathematics is), I'd like to look at two more recent approaches.

C. Common Core State Standards

I'd like to turn first to a particular direction in school mathematics that has been receiving a lot of attention in the last half-dozen years, the Common Core State Standards. I'm not writing either as a proponent or as an opponent of these standards, but more as an observer. There is clearly an attempt, via these standards, to get students to think more about the meaning of the mathematics they are studying—we'll look at several examples in a few minutes. On the other hand, the standards have also been introduced in some very problematic ways and with a variety of agendas on the part of those introducing them. Unlike the New Math, not a lot of money has been spent on institutes that train teachers to teach in ways consonant with the standards. They have been introduced at a time when many political leaders are looking for mechanisms to use to weed out ineffective teachers or, alternatively, mechanisms to punish or blame teachers for the problems of society. I'm not interested in getting into a fight over this right now—clearly I do have some opinions about what I've seen, but that's not my point in bringing them up.

What I'd like to do is look at some of the sample assessment items for the CCSS that one of the consortiums developing those tests, PARCC, released, and the extent to which these appear to be introducing real mathematics at the school level. I have chosen examples that I feel have some important mathematical concepts in them.

1. Third grade: "Art Teacher's Rectangular Array." Three classes of children made painted tiles; the classes made 48 tiles altogether. (The question starts with numbers of tiles for each class and asks for the total, a straightforward addition question.) The teacher wants to display the tiles in a rectangular array not more than ten by ten (displayed in the problem as a square grid of small squares), and the student is to shade the array to show how to display all the tiles. Notice that there is an issue of terminology: students need to know that "rectangular array" means that just shading in four rows of ten squares, and then shading eight squares in the fifth row is not what is being requested.

Also, 48 is quite an interesting number to think about in this context, since it has many factors, and thus there are many rectangular grids that one could make: 2 by 24, 3 by 16, and so on. Presumably a teacher getting children ready for test questions such as this will have them experiment with making such arrays and explore how many different ways one can do this. But for an assessment, they want a small finite number of possible student responses so that it will be fairly quick to grade. Since the grid is 10 by 10, the only factors of 48 that will work are 6 by 8 or 8 by 6. Asking students to explore, to find different rectangular arrays and see if there are some lengths that won't work *is* doing real mathematics, exploring the concept of factorization in the integers. It also involves multiple representations of numbers (as numerals, as lengths, as areas) and the relationship between two areas of mathematics, arithmetic and geometry, here as multiplication and area. It potentially leads to many concepts in number theory: explorations of numbers that can only be represented by one rectangle (primes), how many different ways a number can be factored (and unique factorization into primes), etc.

2. Fifth grade: "Mr. Edmunds' Pencil Box." This problem is quite like many mathematics problems from the middle ages that you'll find in history of mathematics books: Mr. Edmunds shared 12 pencils among his four sons as follows: Alan received 1/3 of the pencils; Bill received 1/4 of the pencils; Carl received more than 1 pencil; David received more pencils than Carl.

On the number line, represent the fraction of the total number of pencils that was given to both Alan and Bill combined. (This is an online problem showing a number line, and students use buttons (labeled "More tick marks," "Fewer tick marks") to increase or decrease the number of equal sections on the number line.) What fraction of the total number of pencils did Carl and David **each** receive? Justify your answer.

The first part is entirely computational but can be solved either by simply adding 1/3 + 1/4 OR by first finding how many pencils Alan and Bill each received (by finding 1/3 of 12, 1/4 of 12), adding the number of pencils, and then finding what fraction of 12 total pencils this is. To represent it on a number line, children have to understand the idea of denominators of fractions representing the number of parts a unit is divided into, that they'll need a common denominator, and that therefore they need to make the screen have 12 parts (11 tick marks) between 0 and 1.

But the second part requires considerable reasoning: a student who found, in the first part, correctly, that Alan and Bill together got 7 pencils, knows that in total Carl and David got 5. It requires further reasoning to realize that there is only one solution: since Carl gets at least two pencils, he must in fact get two and David must get 3, since he's supposed to get more. Again, because it's an assessment item, it's restricted to having a small number of correct answers (in this case, just one). However, in class one could extend this problem either by not requiring that Carl gets more than one pencil (thus leaving numerous solutions) or by using a larger number of pencils and exploring what range of possibilities there are: it can be the beginning of a lot of combinatorial activity. Further, students are asked to justify their answer: mathematics is not just a computation but a coherent reason behind the computation.

So we see several aspects of mathematics in these problems: the fact that there can be multiple approaches to solving mathematical problems, that they can be approached from several perspectives (numerical, geometric, via fractions or integers), that potentially open-ended problems can be used even in early grade school, that one can reason without having an algebraic (or other) formula, that one can do experiments with mathematical objects (even putting a different number of tick marks on the number line is a form of experiment), and that justification is part of doing mathematics. Let's turn to some high school problems.

3. "High school functions." Two different functions, f and g, are given, but neither as formulas. The function f is given as a graph; it's said to be quadratic, and its vertex (2,9) and its x- and y-intercepts, $(-1,0)$, (5,0), and (0,5), are shown on the graph. The function g is said to be linear, and four of its values, from (– 4,7) through (5,-11), evenly spaced, are given in a table. Four comparison questions are

asked of the functions: (a) whether the y-coordinate of the y-intercept of f is less than, greater than, or equal to that of g, (b) similarly comparing the values of the functions at $x = 3$ (neither function’s value is given explicitly at this x-value), (c) comparing the maximum values of the functions on the interval $(-5,5)$, and (d) comparing $\frac{f(5)-f(2)}{5-2}$ with $\frac{g(5)-g(2)}{5-2}$ – that is, comparing the slope of the line between two points on the functions.

Again, this question can be approached in several different ways. One *can* find algebraic expressions for the functions, and then compute. But it is faster to *think* about the information given. For example, for (a), since g is a decreasing function and $g(-1)$ is already below f (0), $g(0)$ must be less than f(0); for (c), since the slope of g is -2, the maximum value of g on the interval is at $x = -5$ and is equal to the maximum value of f, which occurs at its vertex. Notice that one has to pay close attention to the language: while part (b) asks to compare the values of the functions at the same input value, part (c) is looking at the maximum value over an interval.

So what do I see that involves what I’d call real mathematics in this problem? First, multiple ways of representing an object (in this case, a function): graphically, via numerical data, and, implicitly, algebraically. Also, that different types of functions have different characteristic properties, the understanding of which lets one compare them in a range of ways. (I often say to my students that we’re getting to know various inhabitants of the mathematical zoo.) These properties are used for thinking about the concepts and determining further properties. So one explores the functions, via the ways they are presented; one “experiments” with them to see what they do (such as finding the slope of g and determining whether that slope is sufficient that g is higher than f somewhere on a given interval, or whether its slope is always larger or smaller than that of f on that interval).

4. “Seeing structure in a quadratic equation.” “Solve the following equation: $(3x - 2)^2 = 6x - 4$.” There is, of course, a rote way of solving this problem: expand the left side of the equation, subtract $6x - 4$ from both sides, and factor or use the quadratic formula. However, the form the problem was given in invites observing that this is a variation (translation and stretching) of the equation $u^2 = 2u$. Doing so enables the student to solve the equation very quickly and with much less symbolic computation or memorization, via a much more conceptual approach. I think this is typical of “real” mathematics: not that we don’t compute—even in this problem one has to do minor computation to see where $3x - 2$ equals 0 or 2. But I think we’ve all learned that it’s generally a good idea to think about a problem for a while before starting to compute.

Problems such as the ones I have mentioned tend to encourage active learning: rather than memorizing something like the quadratic formula, one can ask students to look for structure in $(3x - 2)^2 = 6x - 4$: can they find some similarity in the expressions on each side of the equation? What would happen if we substituted u for $3x - 2$: what does that do to the equation? Can they then solve it? How would that help with the original equation? Similarly in the high school function example, one can ask the class to think of properties of these functions that would allow answering the questions without finding formulas for the given functions. And elementary school students can experiment with ways to make a rectangle with area 48.

Possibly disturbing is that all of these problems can be approached via old-fashioned symbolic manipulation as well as conceptually—but presumably, given the titles the problem developers chose, the aim is that students will see the conceptual connections.

By the way, I found these problems on the PARCC website in 2014; when I looked in early 2017, they seem to have disappeared, replaced by full sample tests for each grade level. These sample tests are disappointing when compared with the sample items I've mentioned—most problems are much more similar to what has been on state tests for years—although there are still a few rich problems on each sample test.

D. Hyman Bass's School-Level Problems

Finally, I'd like to look at something that Hyman Bass has been presenting recently—I heard him give a talk at the joint meetings in 2015, and he did a workshop (somewhat different presentation, but roughly the same topic) about it at the New Jersey MAA section meeting. Bass is a well-known algebraist who has been interested in school-level mathematics—and the mathematical education of teachers—for quite a number of years and has worked with folks (especially Deborah Ball) with substantial experience actually teaching at the elementary school level. I'm first going to mention a problem I heard him speak about only at the MAA-NJ meeting: if you have m cakes to divide among n people (say 5 cakes among 7 people), what is the smallest number of cuts you can make to divide them evenly? This is clearly a problem 3rd grade students can work on; fair sharing is of interest at that age, and they have the sophistication to work on it. If asked of a class, students can work in groups on the problem, compare different groups' solutions, and discuss how one might decide that one solution is the best that can be done. There are many ways to cut the cakes: cut each cake 5/7 of the way; then, with one of the 2/7 pieces, cut it in half. So 5 people get uncut 5/7 pieces, and the remaining two get three pieces: two 2/7 pieces and one 1/7 piece. Or, cut the first cake at 5/7; put the remaining 2/7 together with 3/7 of next cake. Next, take its remaining 4/7 together with 1/7 of next cake. This is the only cake cut twice: at 6/7 also. And so on. It turns out that the question is related to the Euclidean algorithm, to square tilings of a 5 by 7 rectangle, to several questions in graph theory, and more. It's another illustration of the importance in mathematics of approaching a question from the standpoint of a variety of mathematical structures and of mathematical areas.

But the main thing I've seen him do is hand out a list of four arithmetic problems to one group, three rate problems to another, three geometry problems to a third group, three algebra problems to a fourth group, and suggest that they work on them for a while. Here, for example, are the four arithmetic problems:

1. Find all ways to express ½ as the sum of two unit fractions.

2. Find all rectangles with integer side lengths whose area and perimeter are numerically equal.
3. The product of two integers is positive and twice their sum. What could these integers be?
4. For which integers $n > 1$ does $n - 2$ divide $2n$?

For contrast, here is one of the geometry problems: Given a point *P* in the plane, find all integers *n* such that a small circular disk centered at *P* can be covered by non-overlapping congruent tiles shaped like regular n-gons that have *P* as a common vertex.

And one of the rates problem: Nan can paint a house in *n* days, and her mom can paint it in *m* days (*n* and *m* positive integers). Working together, they can paint the house in 2 days. What are the possible values of *n* and *m*?

First, observe that these are all school-level problems—that is, they could be posed to high school classes, or lower. Of course, all mathematicians know that there are very difficult problems that can be posed with high school, or even elementary school, vocabulary. Also notice that they all involve not only some symbol manipulation but also understanding and examining concepts, and what the implications of the concepts are. One fairly quickly sees that the first problem has two solutions: $1/4 + 1/4$ or $1/3 + 1/6$. One doesn't see any more: but how can one show that that's all there are? "Hmm: $1/m + 1/n = (m + n)/mn$. So if $1/m + 1/n = 1/2$, $2(m + n) = mn$ – wow – perimeter equals area; the product is twice the sum...."

In any case, as people work on their collection of problems, they start realizing these are all variations of the same problem. That is, the same question can be seen through over a dozen different lenses.

What Can We Learn About the Nature of Mathematics from These School-Level Problems?

So school mathematics *can* be real mathematics, but only if we choose to teach it in ways that enable students to experience it as mathematicians do. Looking over the school-level problems we considered, we see many aspects of mathematics in them.

Mathematics is about concepts that are multi-faceted and interconnected via a complex web, so that the same problem often can be represented via apparently different areas of mathematics. (There are, of course, also many results that appear to be completely within one area of mathematics.)

Therefore, although there is often just one correct answer to a mathematical problem, that answer can be correctly found in many different ways, sometimes fairly similar, sometimes apparently totally unrelated.

Mathematics is often about ideas that are preserved under some kinds of transformations.

Although mathematical concepts are abstract, often specific examples can be represented by concrete physical objects (including drawings).

Finding an appropriate symbolic representation is often important for understanding and working with a problem or concept.

These representations can allow people to experiment with mathematical objects as well as to communicate about and work cooperatively with the objects.

Working with mathematical objects (however philosophers characterize them) gives one a feeling for the diversity of characteristic properties of the objects.

One is often less interested in the final answer than in the justification of that answer and in what else it might be related to.

Part of the process of doing mathematics involves sorting examples that meet the criterion of a concept from those that do not.

Taking a complex object (such as $3x - 2$) and renaming it so that the question is centered on it—refocusing the problem—is often helpful.

Doing mathematics involves exploration, the ability to start down a path, realize it isn't being productive, and that it's necessary to go back to the problem to look for a new approach.

Mathematical questions can often be extended and generalized from specific examples.

The excitement in mathematics is in problem-solving—an excitement that had largely been taken out of mathematics when I was in school: everything was rote or routine.

One thing I'd like to point out about many of the problems that I've mentioned here that can be approached from several totally different fields of mathematics: it makes any "foundation" for mathematics that I've seen—whether through logic, set theory, model theory, or category theory—inadequate. None of them account for the wide range of different pairings of fields and approaches one finds in mathematics. Logic and model theory are especially susceptible to this criticism—simply changing whether you're describing the natural numbers with just one constant (1) and one function (the successor) versus with several functions (say, addition and multiplication) makes it, from the perspective of logic, completely different structures. One can interpret one within the other—there are ways of partially accounting for this difficulty—but this is only for perspectives that are from the same field of mathematics. Logic really seems inadequate to view mathematical objects from different perspectives. But so does category theory. It does enable one to talk about certain ways in which very different structures are essentially the same—but only when there are clear ways of replacing every item in one perspective with its equivalent in the other. It's one reason that I, and many mathematicians, tend to be, at least in some respects, firm platonists. If it's a world out there that we're reporting on, then mathematicians may well be somewhat like the blind men examining the elephant—we each perceive it from our own perspective and are amazed when someone shows us how to see it from a different perspective.

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What Is Mathematics and What Should It Be?

Doron Zeilberger

Dedicated to Reuben Hersh on his 90th birthday

Preamble

In the classic "**The Mathematical Experience**", Reuben Hersh and Philip Davis gave a fresh look at mathematics and mathematicians, and showed us that notwithstanding Plato and Hilbert, mathematics is a *human* activity and culture, and its pretensions to absolute truth are unfounded. Mathematics *is what mathematicians do*!. This *leitmotif* was further expanded in the wonderful 'sequel' "**Descartes' Dream: The World According to Mathematics**" , also with Davis, and the more philosophical *"What Is Mathematics, Really?"*, written by Reuben all by himself. The fact that mathematics is what (human) mathematicians do is further expounded, beautifully, in "**Loving and Hating Mathematics**", written in collaboration with Vera John-Steiner, that, inter alia, debunks G.H. Hardy's stupid quip that 'Mathematics is a young men's game', by describing many excellent women mathematicians, and many excellent 'old' mathematicians.

What Hersh and Davis preached back in 1980 is still true today, but it has to be tweaked quite a bit, since very soon (in ≤ 50 years), mathematics would be 'what (machine) mathematicians do', and machines have different 'emotional life' than humans. Hence mathematics would finally, *hopefully*, become a true science.

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What Is Mathematics (as Practiced Today)?

- A religion with its doctrines and dogmas
- A game with its (often arbitrary) rules
- An (intellectual) athletic competitive sport, akin to chess and go
- An art form with rigid rules

One thing, it is not is a **science**. Scientists, by definition, are trying to discover the *truth* about the outside world. Mathematicians do not care about discovering the truth about the mathematical world. All they care about is playing their artificial game, called [rigorous] *proving*, and observing their strict dogmas.

Beware of Greeks Carrying Mathematical Gifts

Once upon a time, a long time ago, mathematics was indeed a true science, and its practitioners devised methods for solving practical mathematical problems. The ancient Chinese, Indian, and Babylonian mathematicians were dedicated good scientists. Then came a major setback, the Greeks!

The dirty (open) secret of the *enlightened* Athenian "democracy" was that it was a **slave** society. Since slaves did all the manual labor, the rich folks had plenty of time to contemplate their navels and to ponder about *the meaning of life*. Hence, *Western philosophy*, with its many *pseudo-questions*, was developed in the hands of Plato, Aristotle, and their buddies, and "modern" pure mathematics was inaugurated in the hands of the gang of Euclid et al.

Not all Greek mathematics was like that, and Archimedes, Heron, and many others also did excellent applied mathematics, but even Archimedes was bound by the Euclidean "party line."

Let's digress and summarize the pernicious Greek influence.

A Brief History of Mathematics as a Sequence of (Unsuccessfully!) Trying to Answer Stupid Questions

- **Stupid Question 1**: Prove the parallel postulate, i.e., make it a theorem.

  Many very smart people tried, in vain, to solve this problem, until it turned out, as we all know, to be impossible.

  This was really an *artificial puzzle*. Play the game of "logical deduction," starting with the (other) axioms, and step-by-step construct an artificial structure called "rigorous mathematical proof," whose *bottom line* is the statement that given a (geometrical) line and a point outside it, there is exactly one line through the point that does not meet the line.

- **Stupid Question 2**: Trisect an arbitrary angle only using straight edge and compass.

  Many very smart people tried, in vain, to solve this problem, until it turned out, in the 19th century, to be **impossible** (trisecting an angle involves constructing a cubic-algebraic number, while it is not too hard to show that a **necessary** condition for a ruler-and-compass constructible number is that its minimal equation is a power of 2).
- **Stupid Question 3**: Double the cube.

  Ditto, $2^{1/3}$ is a cubic-algebraic number.
- **Stupid Question 4**: Square the circle.

  Many very smart people tried (and some still do!), in vain, to solve this problem, until it turned out, in the late 19th century, thanks to Lindemann (inspired by Hermite), to be **impossible**. Indeed, π (and hence $\sqrt{\pi}$) is *transcendental*, all the more reason why it is not constructible.

  Note that these three classical Delfian problems were really artificial puzzles, in an artificial game, whose artificial rules "only use straight edge and compass."
- **Stupid Question 5**: Find a general, closed-form formula, as an expression in p, q, r, s, *only using addition, multiplication, division, and root extraction* for a solution of the quintic equation:

$$x^5 + px^3 + qx^2 + rx + s = 0\,.$$

Recall that the analogous question for a quadratic equation was answered, essentially, by the Babylonians. The questions for the cubic and quartic were answered by Tartaglia and Ferrari (and published by Cardano, ca. 1530), but after that, many smart people, for about three hundred years, tried, *in vain*, to "solve" the quintic equation.

Let's digress to examine how the Renaissance mathematicians "solved" the cubic equation:

$$x^3 + px + q = 0\,.$$

(Recall that we can always transform $x^3 + a_2x^2 + a_1x + a_0 = 0$ to the above form by writing $x = y - \frac{a_2}{3}$.)

We first do an *ad hoc trick*, writing $x = u + v$, where u and v are **to be determined**.

Then

$$(u + v)^3 + p(u + v) + q = 0\,.$$

Expanding

$$u^3 + 3u^2v + 3uv^2 + v^3 + p(u + v) + q = 0\,.$$

Rearranging

$$u^3 + v^3 + 3u^2v + 3uv^2 + p(u + v) + q = 0\,.$$

Replacing $3u^2v + 3uv^2$ by $3uv(u+v)$ we get

$$u^3 + v^3 + 3uv(u+v) + p(u+v) + q = 0\,.$$

Factoring out $(u+v)$ from the third and fourth terms

$$u^3 + v^3 + (3uv + p)(u+v) + q = 0\,.$$

We now do *wishful thinking*, **demanding** that

$$3uv + p = 0\,.$$

In other words

$$uv = -\frac{p}{3}\,.$$

Going back to the above equation we have

$$u^3 + v^3 + 0 + q = 0\,.$$

So

$$u^3 + v^3 = -q\,.$$

Cubing the equation $uv = -\frac{p}{3}$, we get

$$u^3v^3 = -\frac{p^3}{27}\,.$$

Hence the sum of u^3 and v^3 is $-q$ and their product is $-\frac{p^3}{27}$; hence **both** are solutions of the **quadratic equation**

$$z^2 + qz - \frac{p^3}{27} = 0\,.$$

By the quadratic formula, the two roots u^3, v^3 are

$$\frac{-q \pm \sqrt{q^2 + \frac{4}{27}p^3}}{2}$$

So one root of the cubic is

$$\left(\frac{-q + \sqrt{q^2 + \frac{4}{27}p^3}}{2}\right)^{\frac{1}{3}} + \left(\frac{-q - \sqrt{q^2 + \frac{4}{27}p^3}}{2}\right)^{\frac{1}{3}}\,.$$

The other two roots are $\omega u + \omega^2 v$ and $\omega^2 u + \omega v$, where ω is a root of $\omega^2 - \omega + 1 = 0$.

So by an ad hoc trick, and **a lot of luck**, we *reduced* solving a cubic to that of solving a quadratic.

Similar ad hoc tricks work for the quartic, but mathematicians were stumped for three hundred years, until, famously, Ruffini, Abel, and most notably Galois proved that it is **impossible**.

In hindsight, "solvability by radicals" is an *artificial game* with an *artificial* set of "legal moves," and Galois et al. proved that it is impossible to get from position A, the quintic equation, to position B, an algebraic formula, only using addition, multiplication, division, and root extraction.

I admit that while the question itself "solve a quintic by radicals," which turned out (at least in hindsight) to be very stupid, it led to something not quite as stupid, *Group theory* and *Galois theory*.

- **Stupid Question 6**: Find a "rigorous" foundation to the non-rigorous differential and integral calculus of Newton and Leibnitz, thereby "resolving" the paradoxes of Zeno and "addressing" Bishop Berkeley's critique.

  This was *allegedly* "solved" by Cauchy and Weierstrass, but their "solution" was unnecessarily complicated and pedantic, creating the so-called real analysis as one of the most unattractive courses in the math major curriculum, where one does scholastic mental gymnastics to "prove" intuitively obvious facts.

  As I pointed out in my essay *'Real' Analysis Is a Degenerate Case of Discrete Analysis*,

  ["New Progress in Difference Equations", edited by Bernd Aulbach, Saber Elaydi, and Gerry Ladas (Proc. ICDEA 2001), Taylor and Francis, London] available online from my website,

  http://www.math.rutgers.edu/~zeilberg/mamarim/mamarimPDF/real.pdf,

  a much better, **conceptually simpler**, and more honest foundation is to **chuck** infinity and *limits* altogether and replace the derivative of a function $f(x)$ that is defined as the limit of $(f(x+h)-f(x))/h$ as h goes to zero, by the difference operator $\Delta_h f(x) := (f(x+h)-f(x))/h$, where h is a very small, but **not** "infinitesimal." Since h is so tiny (and unknowable), it is more convenient to leave it *symbolic* (analogously to the way physicists write h for Planck's constant). It is true that technically things get a bit messier; for example, the product rule is a little more complicated, but this is a small price to pay, and at the end of the day, when you replace the symbolic h by 0, you get the familiar rules.

  We live in a **finite** and **discrete** world, and the infinite and the continuous are mere *optical illusions*.

  Ironically, the way that "continuous" differential equations are (numerically) solved today, by computers, is by *approximating* them with finite-difference equations, and numerical analysts make a living by proving a priori *error estimates*. The true equations are finite-difference equations to begin with, but the mesh size is too small (and unknowable, the above mentioned h) that it would be impractical to solve numerically and one has to replace it by a far coarser grid.

- **Stupid Question 7**: Is there a set whose cardinality is strictly between that of the integers and that of the real numbers?

  This is Hilbert's first problem, the so-called *continuum hypothesis* or CH for short. It is really a *pseudo-question*, since it pertains to two "infinite," and hence fictional, sets. It is really an artificial puzzle. Can you reach, by finitely many legal moves (in the game called "logical deduction") starting with the axioms of ZFC, either the statement that there exists such a set or the statement that there does not exist such a set. Paul Cohen famously proved that neither! The conventional way of saying it is that CH is **independent** of ZFC, but what Cohen *really* (brilliantly!) meta-proved was that the question was *stupid*! Neither CH nor its negation is reachable in the logical deduction game starting from the axioms of ZFC; hence *both* CH and *ZFC* are devoid of content, and what Cohen (and Gödel) meta-proved was that the so-called infinity does not exist [taken at *face value*].
- **Stupid Question 8**: Are the axioms of arithmetic consistent?

  This, Hilbert's second problem, was famously shown to be a stupid question by Kurt Gödel. The conventional, polite interpretation is that there exist **undecidable** statements, but a more honest interpretation is that every statement that involves quantities over "infinite" sets is *a priori meaningless*. Some such statements, e.g., that $n + 1 = 1 + n$ for *every* natural number, can be made *a posteriori meaningful*, by thinking of n as a *symbol*, but what Gödel proved was that many such statements may not be resurrected like that.
- **Stupid Question 9**: "Given a diophantine equation with any number of unknown quantities and with rational integral numerical coefficients, *devise a process according to which it can be determined by a finite number of operations whether the question is solvable in rational integers*."

  This was famously (brilliantly) meta-proved to be a stupid question by Yuri Matiyasevich, standing on the shoulders of Martin Davis, Hillary Putnam, and Julia Robinson. What he really proved was that while for some *specific* diophantine equations, it is possible to prove that there are **no solutions**, (e.g., $x^2 - 2y^2 = 0$ and the equation $x^n + y^n = z^n$, $n > 2$, $xyz \neq 0$), for other ones, it is possible to find many (in fact, *potentially* infinitely many) solutions (e.g., $x^2 - 2y^2 = 1$), and many such (seemingly sensible, but in fact meaningless) questions can't be resolved in the artificial game called proving number theoretic statements. In other words, they are not only a priori meaningless (as all statements involving "infinite" sets are), they are also a posteriori meaningless.

Many Games Have Unreachable Positions

Time and time again, mathematicians realized that in their human-made, artificial game that they naively believed to be the *real thing*, many things are **impossible**. Sometimes, it is easy to prove the *impossibility*, for example, tiling, by domino pieces, an $n \times n$ checkerboard where two opposite corners have been removed, when

n is odd. Since $n^2 - 2$ is odd, any covering by domino pieces must cover an even number of unit squares. Other times, the impossibility is more subtle; this is the case when n is even (e.g., $n = 8$, the usual chessboard). Coloring the unit squares (as in a chessboard) white and black alternatively, any putative domino covering must cover an equal number of black and white unit squares, but if you remove two opposite corners (from an $n \times n$ board with n even), they must be of the same color; hence, the remainder has an excess of two of the other color.

A less obvious example of proven impossibility is in the solitaire game called the *fifteen puzzle* that Sam Loyd was safe in offering a large prize for its solution (transposing the 14 and 15 and leaving the rest the same), since, as proved in one of the first volumes of James Joseph Sylvester's periodical, *American Journal of Mathematics*, it is **impossible**.

Today's Mathematics Is a Religion

Its central dogma is **thou should prove everything rigorously**. Let me describe two examples of the religious fanaticism of two of my good friends, George Andrews and Christian Krattenthaler. I admire their mathematics, and I like them as people, but I was disappointed (but also impressed) at their *fanaticism* (and [misguided] *integrity*).

Drew Sillls and I **disproved** (in the everyday sense of the word), *conclusively*, a long-standing conjecture by Hans Rademacher (one of the greatest number theorists of the 20th century and George Andrews' thesis advisor). Since the disproof was not "rigorous," George Andrews refused to consider it for the *Proceedings of the National Academy of Sciences* (PNAS), and when we submitted it, it was rejected, because he was consulted. Then we went to the less prestigious journal *Mathematics of Computation*, and once again it was rejected, since it was "too computational." Then we went "down" to the Journal "Experimental Mathematics," and once again it was rejected, by its editor, Yuri Tschinkel, since it was "too experimental." Luckily (for Drew Sills, I already was fully promoted and would have left it in arxiv.org), it was finally accepted in another journal.

Another amusing example of the religious dogmatism of current, otherwise reasonable, mathematicians is when my collaborators, Manuel Kauers and Christian Koutschan, and I submitted to the not-exactly-prestigious journal *Séminaire Lotharingien de Combinatoire* an article

A Proof of George Andrews' and Dave Robbins' q-TSPP Conjecture (modulo a finite amount of routine calculations).

It contained a proof plan that, given enough computer resources, would give a fully rigorous proof, and we had plenty of evidence that the plan should work. But Christian Krattenthaler objected to the title! He would have been happy to accept it if we changed the title to:

A Proposal for a Possible Computer Proof of George Andrews' and Dave Robbins' q-TSPP Conjecture, but we refused, so it remained in our websites (and the arxiv). Luckily, a year later, we found a way to prove it with today's computers, and this *fully rigorous* version was gladly accepted by George Andrews for the PNAS. But all the main ideas were already contained in the previous, semi-rigorous version, with clear evidence that given enough computing power, it should work.

Today's Mathematics Is a Competitive Sport

Number theory is full of conjectures that are obviously true, but humans are unable to prove them [in their outdated, narrow-minded sense of the word, meaning "rigorous proof"]. For example, Yitang Zhang got instantly famous by getting ever so close to the twin-prime conjecture. But even the still-open twin-prime conjecture is so far from what is definitely true, by very plausible heuristics. The twin-prime conjecture asserts that there are infinitely primes p such that also $p + 2$ is prime. The true fact is that there are lots and lots of them, not just "infinitely many"! In fact, there are $O(n/\log(n)^2)$ such twin-prime pairs less than n. But mathematicians do not care about *truth*; they only care about playing their (artificial!) game.

Today's Mathematics Is an Artificial Game

As I demonstrated at length above.

Today's Mathematics is an Art Form

Paul Erdős famously talked about *proofs from the book*, and, indeed, *elegance* and *beauty* are great values cherished by human mathematicians.

Humans, starting with Plato, via Dirac, Hardy, and almost everyone else, waxed eloquently about *beauty*. G. H. Hardy went as far as saying:

There is no place for ugly mathematics.

Excuse me, my dear Hardy, this is even stupider than your "young man's game' unfortunate quip [debunked by Hersh; btw, note the word *game*]. Beauty is only skin-deep and is also in the *eyes of the beholder*, and who are you to *exclude* "ugly" mathematics! Ironically, personally, I find most of your hard-analysis mathematics much uglier than the average mathematics, but this is beside the point.

In a cute New York Times [April 16, 2017] Opinion piece, entitled *Beautiful Equations*, math groupie and distinguished Cornell psychiatry professor, Richard A. Friedman, describes a psychological experiment conducted by Semir Zeki (in collaboration with three other coauthors: John Paul Romaya, Dionigi M. T. Benincasa, and, guess who?, Abel Prize Laureate and Fields Medalist Sir Michael Atiyah).

[*The experience of mathematical beauty and its neural correlates*, Front. Hum. Neurosci., 13 February 2014 https://doi.org/10.3389/fnhum.2014.00068].

In that experiment, fifteen mathematicians were asked to rank a list of sixty famous equations according to beauty, and then electrodes were connected to their brains (using fMRI scanners), and there turned out to be a high correlation between the subject's beauty rank of the formula before the scan and the activation of the area of the brain responsible for aesthetic pleasure.

This is a very interesting psychology experiment that tells a lot about humans but does not say anything about what mathematics *should* be.

It is instructive to see what rankded first and what ranked last. The number-one-hit formula was Euler's

$$e^{i\pi} + 1 = 0\,,$$

and the bottom one, the *ugliest* (in the eyes of the mathematicians' guinea pigs), was Ramanujan's formula for $\frac{1}{\pi}$

$$\frac{1}{\pi} = 2\sqrt{2}\sum_{k=0}^{\infty}\frac{(1103 + 26390k)\,(1/4)_k(1/2)_k(3/4)_k}{k!^3}\cdot\frac{1}{99^{4k+2}}\,,$$

where $(a)_k = a(a+1)\ldots(a+k-1)$.

This shows that human mathematicians are superficial. In the "eyes of God," Ramanujan's formula is much prettier than Euler's. It is a deep relationship enabling a very fast computation of π to billions of decimals. In contrast, Euler's *beauty queen* is an utter triviality, only a bit less trivial than

$$1 + 1 = 2\,.$$

In fact, these two equations have something in common; they may be viewed as *definitions*. 2, by definition, is $1+1$, and π, by definition, is the smallest real number larger than 0 for which e^{ix} happens to be equal to -1. In other words, the smallest x for which $\sin x$ equals 0 (and hence $\cos x$ equals -1 and hence $e^{ix} = \cos x + i\sin x = -1 + i\cdot 0 = -1$).

While there is no harm in enjoying mathematics for its (subjective!) beauty, it is wrong to make it a *defining* property and to *exclude* what one (subjectively!) finds *ugly*. Every statement that has a *proof from the book*, is ipso facto, *trivial*, and, at least, a posteriori, since all deep statements have long proofs. In my eyes, the most beautiful theorems are those with succinct statements for which the shortest known (and hopefully any) proof is very long. So in my eyes, both the Appel-Haken's *four color theorem* and Tom Hales' *Kepler's ex-conjecture* should be in the **book**.

Today's Mathematics Is an Elitist and Exclusive Club

It has a very intricate *social structure*, with a fairly rigid pecking order, and hierarchy, and "peer-reviewed journals," with pompous, often *sadistic* editors, who must enjoy rejecting submissions (or else they would refuse to take the job).

Some areas are considered very *respectable*, while other ones are *slums*. This is a dynamic process, and areas come and go out of fashion, but there are always, like in a high school, the "cool kids lunch table," and there are always the outcasts and the *pariahs*. Humans will be humans!

Today's Mathematics Is Not a Science

As we saw above, mathematics today is many things, but one thing that it is **not** is a *science*. It is amazing that, nevertheless, mathematics was so *effective* in science, so convincingly told by Eugene Wigner. One reason it was so effective was that the kind of mathematics that scientists needed was either discovered or rediscovered by themselves (e.g., Heisenberg rediscovered matrix algebra, ab initio), and they develop their own brand of mathematics, **without** the mathematicians' misplaced obsession with *rigor* (e.g., quantum field theory and the renormalization group), and the great success of mathematics in science is **in spite** of the mathematicians' superstitious dogmas. Imagine how more effective it would have been if they threw away their artificial *shackles*.

The Computer Revolution

The reason mathematics today is the way it is is due to the *contingent* fact that it was developed without computers. Most of the questions (and pseudo-questions) that occupied mathematicians through the ages are now *moot* and *irrelevant*. It may be a good idea to start *all over* and develop mathematics ab initio, not peeking at the human-generated mathematics done so far and taking full advantage of computers.

Let's Make Mathematics a Science Again!

Mathematics **should** become a *science*, and its main *raison-d'être* should be the discovery of mathematical **truth (broadly defined!)**. In particular, one should abandon the *dichotomy* between *conjecture* and *theorem*.

If a mathematical question is in doubt (and it looks that it can go either way: true, false (or meaningless (i.e., "undecidable")), then it is a mathematical *question*. If there is overwhelming empirical and/or heuristic evidence, then it is a *theorem*. What was formerly called a theorem should be renamed "rigorously proved statement" [using the artificial game of logical deduction].

Final Words

Don't get me wrong, while you may call me a *self-hating mathematician*, I do *love* elegant proofs and still enjoy traditional standards. So like Reuben Hersh's wonderful book mentioned above, I **love and hate** (traditional) mathematics. But I **do** believe that it is time to make it a **true** science.

Humanism About Abstract Objects

Julian Cole

Introduction

In fall 2015 and spring 2016, I was lucky enough to be on sabbatical. My project was to complete a manuscript generalizing my institutional account of mathematics to cover other paradigmatically abstract objects. Ten years earlier, while finishing my dissertation, I had read Reuben's *What is Mathematics, Really?* At the time, I remember thinking I'm not really sure whether we agree about the nature of mathematical objects. On the one hand, Reuben's book was the first one in which I encountered an explicit comparison between mathematical objects (e.g., numbers, circles,[1] and ordered fields) and institutional objects (e.g., marriages, wars, corporations, and the US Supreme Court), a comparison that is central to my institutional account of mathematics. On the other hand, while Reuben seemed to be committed to there being mathematical and (other) institutional objects, many of the things that he said about them suggested that he viewed them more like fictions than genuine existents. I was intrigued, but didn't really follow up on that intrigue until my sabbatical offered me the opportunity to visit some of the folks who I took to endorse similar accounts of mathematics to my own. Two such individuals were Reuben and Sol Feferman. For me, our meetings revealed something interesting: while we were all three natural allies in taking humans to be responsible for mathematics—we were *humanists*, to use Reuben's terminology—we did not agree on the underlying nature of mathematical objects. At least as I interpreted them, Reuben and Sol held fairly similar views about the underlying nature of such

[1]I am referring to perfect circles rather than the roughly circular objects that we find around us.

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objects: they are something like intersubjective mental objects. Sol—see (Feferman 2009, 2014)—expressed his view in this way, “the basic objects of mathematical thought exist only as mental conceptions,” where, according to Sol, these mental conceptions are highly constrained by social interactions concerning them. Reuben (2014, p. 13), on the other hand, expresses his view in this way: “A mathematical entity is a concept, a shared thought” and “The concept ... is nothing other than the collection of the mutually congruent ... ‘mental models’ ... possessed by those participating in the mathematical culture.” To summarize Reuben’s view as he did in personal communications that followed our November 2015 meetings, “mathematical objects are ‘equivalence classes’ of mutually congruent ‘mental models’ of those objects.”

I could not agree. On neither view, it seems to me, are there mathematical objects. On both views, we have only shared, or at least shareable, propositional and nonpropositional mental representations of mathematical objects, where, to my mind, it doesn’t really matter what we call these representations. We do, of course, possess shared propositional and nonpropositional mental representations of mathematical objects of various kinds. For instance, all of us believe that “$2 + 2 = 4$,” many of us are aware of the conjecture that “every even natural number greater than 2 is equal to the sum of two prime numbers,” anyone who is reading this article will possess nonpropositional mental representations of the natural numbers and circles, and anyone with any mathematical sophistication will possess such representations of ordered fields. Yet these mental representations of mathematical objects are not mathematical objects, for we must keep separate mental representations of Xs, even shared mental representations of Xs, from Xs.[2] Mental representations of chairs aren’t chairs, mental representations of tectonic plates aren’t tectonic plates, mental representations of unicorns aren’t unicorns, and mental representations of numbers, circles, and ordered fields aren’t numbers, circles, or ordered fields. For one thing, mental representations aren’t prime, circular, or ordered in the relevant sense. Sol, I am confident, would have agreed. But Reuben, I believe, does not; when I quoted him above as writing “A mathematical entity is a concept, a shared thought,” I did not quote a slip of the keyboard, so to speak, but a view of mathematical objects to which I believe he is committed. While Rueben recognizes the “logical” problem that I just identified, i.e., that mental representations of mathematical objects possess very different features than do mathematical objects, to date, at least, this problem has not convinced him to change his account of mathematical objects.

Moreover, Reuben’s account of mathematical objects has some important implications. For instance, mental models of mathematical objects of particular kinds are both dated and contingent. In particular, there is a time at which the first person to ever possess such a model comes to possess that model and a time at which any particular person first comes to possess such a model. Similarly, it is a contingent truth that there is a person who possesses a mental model of mathematical objects of a particular kind and that any particular person possesses such a model.

[2]There is, of course, one exception: when the Xs are, themselves, mental representations.

Furthermore, both our individual and collective mental models of mathematical objects of particular kinds are in a state of change; as we both individually and collectively investigate mathematical objects of particular kinds, our mental models of those objects change. Accordingly, if Reuben's account of mathematical objects is correct, three theses that philosophers of mathematics have spent significant effort explaining and justifying are false. Specifically, mathematical objects are neither atemporal existents nor necessary existents, nor unchanging; on the contrary, they are finite, contingent, and changing.

Reuben and Sol are not alone in holding views like those that I ascribed to them above (e.g., it is clear that George Lakoff and Raphael Núñez (2000) take the work of explaining "where mathematics comes from" to be complete once they have explained the origins of our mathematical concepts). Such views arise from the thought that, in order to explain mathematical practice and the role that mathematics plays in other activities (e.g., the development and statement of scientific theories), one really doesn't need to postulate anything other than (propositional and nonpropositional) mental representations of mathematical objects of particular kinds; in particular, one doesn't need to postulate mathematical objects themselves. The reasoning behind this thought goes something like this: given that mathematical objects, if there are any, are abstract, mathematicians, scientists, and others who take themselves to be investigating such objects or employing such objects in understanding reality really can't be investigating or employing these objects themselves. What then are they investigating and employing in their activities? They are investigating and employing shared, or at least sharable, mental representations of mathematical objects. Accordingly, what we really need to postulate to account for mathematical practice and the role that mathematics plays in other activities is not mathematical objects but shared mental representations of mathematical objects. And, if this is all that we need to postulate in order to do all of the explanatory work that is of interest to us, why postulate anything other than this? Why postulate "spooky," "mysterious," "otherworldly" objects that aren't facets of the spatiotemporal reality to which we are all, quite correctly, committed?

The answer to this question that many philosophers of mathematics accept is typically ascribed to Frege (1884). Here is a conceptual truth about the natural numbers:

The number of *Fs* is *n* if and only if there are exactly ***n*** *F*s.

Accordingly, provided that claims of the form "there are exactly ***n*** *F*s" are true (e.g., "there are exactly **four** computers in this room" and "there are exactly **two** objects that are identical to the natural number zero or the natural number one"), then so are claims of the form "the number of *Fs* is *n*" (e.g., "the number of computers in this room is four" and "the number of objects that are identical to the natural number zero or the natural number one is two"). On any reasonable analysis of the logical form and content of claims of the form "the number of *Fs* is *n*," the numerals that appear in these claims are singular terms that purport to refer to natural numbers. Moreover, it is standardly believed that claims of this particular logical form can be true only if the singular terms, i.e., numerals, that appear in them do in fact refer. Yet, if numerals that purport to refer to natural numbers do in fact refer, there are particular natural numbers. Indeed, if one uses the trick of considering “the objects that are non-self-identical,” “the objects that are identical to the natural number zero,” “the objects that are identical to the natural number one or the natural number two,” etc., one quickly sees that all natural numbers exist.

So, there is a kind of explanatory work that requires us to acknowledge that there are natural numbers (and, by parallel reasoning, that there are mathematical objects of many other kinds): we need to provide a plausible and uniform semantics for our everyday claims. The problem with this observation is that it really doesn’t allay the naturalistic/physicalistic concerns of those who, like Reuben, find the idea of acknowledging that there are mathematical objects that are distinct from our mental representations of such objects intellectually abhorrent since to do so, they believe, is to commit themselves to a “spooky,” “mysterious,” “otherworldly” platonism about such objects. That is, it is to commit themselves to there being mathematical objects that exist independently of our representations of them in a reality that is both distinct and causally/metaphysically isolated from the spatiotemporal reality that we occupy. How, they ask, could we ever come to know about such objects? How could we ever come to refer to them? Of what use could such objects be?

It is precisely at this point that the genius of Reuben’s (1997) comparison between mathematical and institutional objects can be appreciated, for it points toward a way out of this dilemma. The US Supreme Court is not identical to our (propositional and nonpropositional) mental representations of it. It has the authority to interpret the law of the land and decide on matters of life and death; our (shared) mental representations of it do not have this authority. This court also isn’t an everyday physical object in the way in which the computers in this room are; I can’t literally pick it up and move it around. This court exists; it really does decide matters of life and death. Moreover, this court wouldn’t exist if there weren’t any human beings; in a straightforward modal-existential sense, it has an existence that depends on the existence of human beings. Furthermore, this court isn’t “mysterious,” “spooky,” or “otherworldly” either; it exists because we represent it to exist and, give or take, it possesses the features that we provide it by representing it as possessing those features. The key point for our current purposes, though, is that the US Supreme Court and similar institutional objects clearly demonstrate that it is possible for humans to be responsible for there being objects that aren’t everyday physical objects, aren’t identical to our mental representations of them, and aren’t “mysterious,” “spooky,” or “otherworldly” either. Accordingly, there shouldn’t be a problem with maintaining that humans are responsible for there being mathematical objects that aren’t everyday physical objects, aren’t identical to our mental representations of them, and aren’t “mysterious,” “spooky,” or “otherworldly” either. In other words, Reuben’s comparison suggests that we can acknowledge the force of Frege’s argument without being committed to a “spooky,” “mysterious,” “otherworldly” platonism about mathematical objects. The devil, of course, is in the details.

The Details

We need to know how, precisely, humans can be responsible for there being objects that aren't everyday physical objects, aren't identical to our mental representations of them, and aren't "mysterious," "spooky," or "otherworldly." I don't have the space to provide all of those details here, but let me at least outline some of them. First, note that, sometimes, we make reality a certain way by representing it to be that way. For instance, when, at the beginning of one of my classes, I utter "let's get started," I make it the case that my class has started by representing it as having started. Similarly, when a judge, in an appropriate setting, utters "I hereby order you to spend thirty days in prison," he or she thereby makes it the case that the person who he or she is addressing must spend thirty days in prison by representing him or her as having to do so. Formally, philosophers call these types of speech acts declarations, as in the (adoption of the) Declaration of Independence, which made it the case that there was a new nation by representing there to be such a nation. Informally, we may think of these actions as making it the case that things are certain ways or that there are certain objects by collective agreement.

Once one starts considering making things the case by collective agreement, one quickly realizes that many things are the case and that there are many objects by collective agreement. For instance, that I am an Associate Professor within the State University of New York (SUNY) system and that I am a British citizen are the case by collective agreement. Similarly, that there is a SUNY system, that there is the United Kingdom of Great Britain and Northern Ireland, and that there is the US Supreme Court are the case by collective agreement. Thus, if we take Reuben's comparison between mathematical and institutional objects seriously, the proposal to which we should be drawn is this: mathematical objects of particular kinds (e.g., natural numbers, circles, and ordered fields) exist by collective agreement, which, a little more formally, amounts to it being the case that mathematical objects exist by declaration, i.e., in virtue of us representing there to be such objects. I like to talk about objects that exist in virtue of us representing there to be such objects as having an existence that is *representationally dependent* on, i.e., *dependent*$_R$ on, collective intentionality.

Now, if one considers the collective agreements responsible for people possessing the statuses "associate professor within SUNY" and "British citizen" and for there being SUNY, the UK, and the US Supreme Court, it is clear that these agreements are relatively formal and legalistic. But there is no need for the collective agreements that are responsible for people or objects possessing particular statuses or for there being objects of particular kinds to be formal and legalistic. For instance, while I was younger, one of my friends would regularly make himself the goalkeeper in our makeshift game of soccer simply by standing in front of the makeshift goal on our makeshift field. Similarly, it is easy for me to make one of the two identical beers that I just bought at the bar David's and the other Juan's simply by handing them, respectively, to David and Juan. Likewise, someone can easily become a member of a particular clique simply in virtue of the current members of that clique allowing him or her to join them in their favored activities. In none of these cases are the relevant collective agreements either formal or legalistic.

Moreover, it isn't merely that people or objects can possess new statuses in virtue of informal and non-legalistic collective agreements; there can be objects in virtue of such agreements as well. For instance, in making a particular beer David's, I might make it the case that there is such an object as the 1,000$^{\text{th}}$ beer owned by David, while, in surreptitiously joining a new clique, David might make in the case that there is such an object as the 10$^{\text{th}}$ member of the said clique. For our current purposes, though, the important point is that taking Reuben's comparison seriously doesn't demand that there be formal or legalistic collective agreements that are responsible for there being mathematical objects of particular kinds; one might be party to the relevant agreement(s) without ever explicitly formulating them or considering them, just as most of us are party to the agreement that one can make a beer some particular person's beer simply by giving it to him or her without ever explicitly formulating or considering this agreement.

Next, observe that we collectively agree that certain people and objects possess certain social statuses and collectively agree that there are objects of certain kinds in order for them to serve *functions*, i.e., in order for those people and objects to occupy roles that promote certain ends, goals, or purposes. For instance, collectively agreeing that there is such an object as SUNY facilitates the provision and certification of the higher education of numerous people, while collectively agreeing that I am an Associate Professor within the SUNY system facilitates me in performing my role in providing and certifying the higher education of students who attend a particular branch of this system. Accordingly, if we are to take Reuben's comparison seriously, we should be able to point to a function or functions that mathematical objects of particular kinds serve.

In service of identifying the function(s) that mathematical (and many other abstract) objects serve, let me make some observations. First, particularly when facets of reality are important to us, there are a variety of activities relating to them in which we frequently engage. These include reasoning about them, inquiring after their features, discovering truths concerning them, analyzing situations that involve them, planning actions that surround our interactions with them, and stating or representing how reality is with respect to them. Henceforth, label these and similar activities *representational* in recognition of the fact that they employ representations of the said facets of reality.

Second, we find it significantly easier to engage in representational activities when their *subject matter*—what we are inquiring after, discovering truths concerning, reasoning about, analyzing, representing, etc.—is a plurality[3] of objects or is treated as such in our representations. Indeed, we find engaging in such activities easiest when their subject matter is, or is treated as, a *small* plurality of objects, where this amounts to the said plurality containing four or fewer objects. That we

[3]For readability, I assume throughout that pluralities can be of any finite cardinality, including **zero**.

find it significantly easier to engage in representational activities when their subject matter is, or is treated as, a (small) plurality of objects fits well with our everyday experience. For instance, anyone who has compared the difficulty of assessing the validity of a modal inference using representations of its premises and conclusion that employ possible worlds—a plurality of objects—and representations that do not will recognize how much easier such an assessment is when undertaken using representations of the former type. Similarly, anyone with experience of doing things both ways will recognize how much easier it is to investigate the relationship between the cardinalities of various finite pluralities when one represents such cardinalities using natural numbers—a plurality of objects—than when one simply takes them to be features of finite pluralities.[4] Ultimately, however, the fact that we find it significantly easier to engage in representational activities when their subject matter is, or is treated as, a (small) plurality of objects is best accounted for by our innate representational capacities. Specifically, this fact is best accounted for by the cognitively basic nature of our capacity to represent reality in terms of (small) pluralities of objects, for this capacity ensures that many of the cognitive abilities and capacities that we possess for engaging in representational activities are configured to function best when we employ representations of reality that represent it in terms of (small) pluralities of objects.[5]

Third, we find it so much easier to engage in representational activities when their subject matter is a (small) plurality of objects that, when the subject matter of some representational activity R_N is not of this type, we often obtain R_N's outcome by using an alternate or *surrogate* representational activity R_S whose subject matter is a (small) plurality of objects. Or, to put this point as I shall throughout, we engage in R_N using a *surrogate subject matter* that consists of a (small) plurality of objects, i.e., we engage in R_N by treating its subject matter as a (small) plurality of objects by replacing its (non-surrogate) subject matter with a surrogate subject matter that is a (small) plurality of objects.[6] For instance, when we want to assess the validity of a modal inference, instead of doing so directly, we frequently restate its premises and conclusion using possible worlds and assess the restated argument rather than the original. Similarly, when we want to investigate the relationship between the cardinalities of certain finite pluralities, we frequently represent the said cardinalities using natural numbers and conduct the investigation in these restated terms rather than the original. Another way of expressing these points is that we employ possible worlds as a surrogate subject matter for the non-surrogate subject matter of various modal inferences and the natural numbers as a surrogate subject matter for the non-surrogate subject matter of various investigations into the relationships between the cardinalities of finite pluralities. Yet another way that I

[4]For a convincing illustration of this second point, see (Field 1980, §2).

[5]Unfortunately, I do not have space in this article to explore and defend the cognitively basic nature of our capacity to represent reality in terms of (small) pluralities of objects.

[6]Here, I presuppose that we may individuate representational activities on the basis of their outcomes.

sometimes express points such as the aforementioned is that, when assessing modal inferences, we frequently employ possible worlds as surrogates for how reality would be if it were to be different in certain respects and for how reality could be for all that we know, while, when investigating relationships between the cardinalities of finite pluralities, we frequently employ natural numbers as surrogates for such cardinalities.

In illustrating my third observation, there is no need to consider only philosophically contentious objects like possible worlds and natural numbers, though. Consider, for instance, our division of continents into nations and towns/cities into plots of land. We undertake these divisions so that particular individuals and pluralities of individuals can claim ownership of the said nations/plots, where such ownership comes with special *deontic powers*—rights, responsibilities, authorizations, etc.—concerning the land in question. As such, it is a consequence of how we divide land for the aforementioned purposes that, at certain locations, there are transitions in deontology between individuals and/or pluralities of individuals. Moreover, given the importance that we place on ownership of land, the exact location of these transitions is frequently of great interest to us. When we engage in representational activities concerning these transitions, we almost invariably do so in terms of boundaries (aka borders) rather than the transitions themselves, i.e., we use a plurality of objects—boundaries—as a surrogate subject matter for these activities.

Next, consider complex organizations, such as large corporations, university systems, and intricate systems of government. Such organizations are associated with numerous people who, and pluralities of people that, contribute to their operation, and it is often difficult to understand the respective roles of these people and pluralities in the operation of their respective organizations. In order to facilitate such understanding for the purposes of engaging in various representational activities, it is common to represent organizations as possessing operational structures, where the positions in these structures serve as surrogates for people who, and pluralities of people that, perform particular roles in the operation of the said organizations. For instance, in explaining the US system of government, it is common to describe it as possessing three branches, which, in turn, contain such positions as those of the President, the Supreme Court, and Congress. Moreover, that we collectively represent the operational structure of the US system of government as containing three branches that in turn contain small numbers of positions is no accident but illustrates our preference for engaging in representational activities using representations of reality that represent it in terms of *small* pluralities of objects rather than merely pluralities of objects of any cardinality. Our representations of the operational structures of complex organizations often take them to include layers that consist of small pluralities of positions since doing so allows us to engage in representational activities concerning these structures whose subject matters are small pluralities of objects.

As a third example, consider the game of chess. In teaching people to become better chess players or proving results about chess such as that it is impossible to force a checkmate against a lone king with a king and two knights, we frequently represent games of chess as mere sequences of moves, i.e., we engage in representational activities concerning the (token-individuated) games of chess in which actual players participate using the objects that philosophers call type-individuated games of chess as a surrogate subject matter for the non-surrogate subject matter of these activities.

It should now be clear that serving as a surrogate subject matter for a particular representational activity or for representational activities of a particular kind is a function that objects of a particular kind can usefully serve. This is the function that, in developing Reuben's comparison between mathematical and institutional objects, I take mathematical objects to serve; for convenience, I talk about mathematical (and numerous other abstract) objects as serving *surrogacy functions* in representational activities.

Given the immense variety of (non-surrogate) subject matters that aren't (small) pluralities of objects with respect to which we might wish to engage in representational activities, it would be nothing short of miraculous if there were pluralities of objects that exist independently$_R$ of intentionality that could serve as surrogate subject matters for all of these activities. More plausibly, we make it the case by declaration/collective agreement that there are pluralities of objects that can serve the relevant surrogacy functions; that is, we make it the case by declaration/collective agreement that there are mathematical (and many other abstract) objects.

For clarity, let me further explain the basic idea behind my proposal that we make it the case by declaration/collective agreement that there are mathematical objects of particular kinds. As I see things, during the course of various undertakings, mathematicians come to have an interest in investigating the consequences of facets of reality standing in certain relations to one another, which can be thought of them coming to have an interest in investigating the consequences of a plurality of facets of reality possessing a particular structure, where this investigation can also be understood as an investigation into the features shared by all possible pluralities of facets of reality that stand in the said relations to one another/possess the relevant structure. This investigation can more easily be carried out using a surrogate subject matter that consists of a plurality of objects that stand in the relevant relations to one another/possess the relevant structure as a surrogate subject matter, where, give or take, these objects possess only features that are consequences of them standing in the said relations to one another/possessing the said structure. In other words, this investigation can be carried out more easily using a particular mathematical structure as a surrogate subject matter, where a *mathematical structure* is a special type of object that is made up or constituted by other objects—its *positions* or *places*—that stand in certain particular relations to one another—in this case, the relations that are of interest—and are metaphysically incomplete in that, give or take, the only features that the places of a mathematical structure possess are those that are consequences of them standing in the particular relations to one another that they do. Given this, mathematicians make it the case by declaration/collective agreement that there is such a mathematical structure in order for it to serve as the surrogate subject matter of this particular investigation, though it should be remembered that this happens in an informal and non-legalistic way rather than by means of some formal, legalistic declaration/collective agreement. Investigating the consequences of a plurality of facets of reality possessing a particular structure by investigating the features of a mathematical structure whose places possess that structure is just how mathematicians go on in most situations, much as making a beer a particular person's by giving it to him or her is just how most of us go on in most situations. In each case, there is a background institution, with suitable constitutive rules/standing declarations, that makes our doing so perfectly appropriate.[7] In the latter case, the relevant institution is property ownership, particularly some of the more informal elements of this institution, while in the former case, it is what I have elsewhere called the *surrogate subject matter institution*. The central constitutive rule/standing declaration of this institution can be expressed in this way: our possessing a concept of a plurality of objects with particular features that, were it to be used as a surrogate subject matter for a given representational activity, would facilitate our engagement in that activity suffices for there being that plurality of objects/subject matter.

Perhaps a more concrete, historical example will aid the reader in understanding my account of mathematical objects/structures. While investigating algebraic solutions to cubic and quartic equations, sixteenth century mathematicians recognized that the domain of the square root function should include negative numbers, which, in turn, led them to recognize that there are possible outputs of this function that cannot be identified with real numbers, which, in turn, allowed them to form a shared concept of the complex number structure, i.e., a mathematical structure whose places stand to one another in the relations that *all* outputs of the square root function do. These mathematicians decided to investigate the consequences of facets of reality standing to one another in these particular relations. The central constitutive rule/standing declaration of the surrogate subject matter institution was, and continues to be, responsible for there being the complex number structure to serve as a surrogate subject matter for this investigation.

In understanding the surrogate subject matter institution and its central constitutive rule/standing declaration, it is helpful to compare it with other institutions that are responsible for there being objects of particular kinds. One such institution is corporate activity in the State of California, which is governed by the constitutive rules outlined in the State of California's Corporations Codes. Section 200a of these Codes reads "One or more natural persons, partnerships, associations or corporations, domestic or foreign, may form a corporation under this division

[7] As I understand an *institution*, it is a plurality of activities governed by constitutive rules, which are *standing declarations*, i.e., declarations that are in place for an extended period of time and, during that time, specify that fulfillment of certain conditions suffices for something being the case. For instance, basketball is an institution, and one of its constitutive rules/standing declarations specifies that throwing the ball through the opposing team's basket from outside the three-point line during play suffices for your team scoring three points. Please note that my use of institution is somewhat different from a colloquial one according to which institutions are what I earlier called organizations (e.g., universities and corporations).

by executing and filing articles of incorporation." This is a standing declaration/collective agreement to the effect that the right kind of individual(s)—one or more natural persons, partnerships, associations, or corporations, domestic or foreign—may make it the case that there is an object of a particular kind, a Californian corporation, by performing the right kind of actions, executing and filing articles of incorporation. Of course, the reality is somewhat more complicated than this in that the State of California has to recognize the execution and filing of the articles in question as legitimate, etc. Yet, give or take, the aforementioned is all that is involved in making it the case that there is a new Californian corporation.

There are, of course, some differences between how the institution of corporate activity in the State of California is responsible for there being Californian corporations and how the surrogate subject matter institution is responsible for there being mathematical (and other abstract) objects. For instance, the constitutive rules/collective agreements governing the former institution are highly formal and legalistic, while those governing the latter are informal and non-legalistic, Californian corporations serve very different functions than surrogate objects of various kinds—roughly, limiting the financial responsibilities of various people who are associated with them—and all that needs to happen for there to be surrogate objects of a particular kind is that someone formulate a shareable concept of those objects, while certain kinds of individuals must execute and file articles of incorporation for there to be a particular Californian corporation. But these are differences of degree, not differences of kind; essentially, the two institutions work in the same way to make it the case that there are objects of particular kinds.

So, drawing on Reuben's comparison between mathematical and institutional objects, I have been able to provide an account of how humans can be responsible for there being mathematical (and other abstract) objects: institutions, which are simply activities governed by clusters of standing declarations/collective agreements, can be responsible for their existence. What of the features of such objects? One claim that I have made repeatedly is that mathematical objects are abstract; how can this be the case without our being committed to "spooky," "mysterious," "otherworldly" objects?

To begin, let us consider what it means for an object to be abstract. I favor a strategy for specifying what abstract objects are that has come to be known as "the way of negation," that is, I take abstract objects to be those that lack the features characteristic of being concrete. Moreover, I take both "abstract" and "concrete" to be governed by a plurality of features rather than a single feature, where some of the features in these pluralities are more important than others to whether an object is, respectively, abstract or concrete. More specifically, an object is *abstract* if and only if it fails to possess *the most central feature* in the plurality associated with concrete, while the more such central features it fails to possess, the more paradigmatically abstract it is.

In order of centrality, the following are, according to my account, the most central features of the relevant pluralities:

	Concrete	Abstract
1.	Spatial/spatiotemporal	Nonspatial/non-spatiotemporal
2.	Causally efficacious	Causally inefficacious/acausal
3.	Exists for a finite period	Eternal/semi-eternal/atemporal
4.	Contingent existent	Necessary existent

As noted above, I take only the first of these features to be essential to being, respectively, concrete or abstract. Moreover, since we developed the concepts "concrete" and "abstract" before we understood that space and time are linked, the features listed under 1 can be understood in purely spatial rather than spatiotemporal terms. Accordingly, what I take to be essential to being abstract is lacking a spatial location; having any of features 2 through 4 in addition simply makes an object more paradigmatically abstract.

Now, what are the implications of this account for our making it the case by declaration/collective agreement that there are abstract objects? Put simply, they are that we can make it the case by declaration/collective agreement that there are abstract objects simply by collectively agreeing that there are objects that fail to possess a spatial location, while we can make it the case by declaration/collective agreement that there are paradigmatically abstract objects simply by collectively agreeing that there are objects that not only fail to possess a spatial location but also fail to causally interact with other objects, fail to be finite existents, and fail to be contingent existents. Admittedly, it can be difficult to understand how we might achieve the last of these things and, perhaps, the penultimate one as well. But it isn't difficult to understand how we might make it the case that objects of a particular kind that exist by collective agreement fail to possess a spatial location or fail to causally interact with other objects: we just adopt a collective agreement that fails to provide the said objects with a spatial location or to specify any causal relations in which they stand. In other words, there is nothing "spooky," "mysterious," or "otherworldly" about our collectively agreeing that there are mathematical and other abstract objects; all that is involved in our doing so is our adopting collective agreements that fail to provide the objects for which they are responsible with spatial locations and a causal profile.

We can also make sense of the idea that declarations/collective agreements are responsible for there being objects that are atemporal and necessary existents, but to do so, we must first consider some background issues. Most importantly, we need to understand what it would be for objects of a particular kind to exist atemporally or exist necessarily. Moreover, in order to understand this, we need to understand something more fundamental: what we are claiming in claiming that there are objects of a particular kind. Frege (1884) offered a simple account: there are objects of kind K if and only if the extension of the concept "K" is nonempty. I want to offer a slightly modified version of this account. In this connection, observe that sortal concepts and nominative terms that are associated with sortal concepts come with two types of conditions: *application conditions* and *coapplication conditions*. The former are conditions that must be met in order for it to be permissible to apply the relevant term or concept in some representational activity, while the latter are conditions that must be met in order for a permissible reapplication of a term or concept in a representational activity to be a coapplication of that term or concept, i.e., an application of it to the same object of the relevant kind. With these definitions in place, it is clear that there are objects of kind K if and only if it is permissible to apply the concept "K" in representing reality (as it actually is now), i.e., if and only if the application conditions for "K" are met by reality (as it actually is now).

Before I extend this understanding of existence to include what it is for objects of kind K to be necessary and/or atemporal existents, let me make an observation. We engage in representational activities not only concerning how reality actually is now but, for instance, concerning how reality would be were it different in certain respects, i.e., under subjunctive suppositions, concerning how reality might be for all that we know, i.e., under indicative suppositions, concerning how reality is according to a particular fiction, i.e., under fictional suppositions, concerning various times that are not now and even concerning no time at all. With this observation in place, the basic ideas behind what it is for objects of kind K to be necessary and/or atemporal existents may be communicated in this way. Objects of kind K are *necessary existents* if and only if the concept "K" possesses *subjunctively universal permissible coapplicability*, i.e., for any application *A* of "K," there is no subjunctive supposition *S* such that the mere fact that a representational activity *R* takes place under *S* makes it impermissible to coapply *A* in *R*. Similarly, objects of kind K are *atemporal existents* if and only if the concept "K" possesses *temporally universal permissible coapplicability*, i.e., for any application *A* of "K," there is no time *T* such that the mere fact that a representational activity *R* concerns *T* makes it impermissible to coapply *A* in *R*. Yet, while these are the basic ideas behind what it is for objects of kind K to be necessary and/or atemporal existents, usually, when we claim that objects of kind K are necessary and atemporal existents, we actually convey more than the aforementioned; usually, we convey that "K" possesses *universal coapplicability*, i.e., for any application *A* of "K," there are no restrictions on the permissible coapplication of *A*. In other words, in claiming that objects of kind K are necessary and atemporal existents, we are usually conveying that, in *all* representational activities, regardless of whether they take place under certain suppositions, concern times that are not now, or concern no time at all, the conditions for a permissible coapplication of any application of "K" are met.

With this understanding of what we are conveying in claiming that objects of kind K are necessary and atemporal existents, it is relatively easy to see how it could be that some surrogate objects are necessary and atemporal existents even though declarations/collective agreements are responsible for there being the said objects. First, observe that, just as with representational activities that concern how reality actually is now, representational activities that take place under various suppositions or that concern various times that are not now (or no time at all) might have as their (non-surrogate) subject matters facets of reality that are not (small) pluralities of objects. Second, were one to engage in such a representational activity, it would be just as beneficial to engage in it using a surrogate subject matter that consists of a (small) plurality of objects as it is to engage in a representational activity that concerns how reality actually is now using such a subject matter when its non-surrogate subject matter is something other than a (small) plurality of objects. Third, given this pair of observations, the following should be true simply in virtue of the nature of surrogacy: the fact that a representational activity takes place under some supposition or concerns some time(s) other than now (or no time at all), should not, by itself, be responsible for it being impermissible to apply or coapply a term/concept that refers to a surrogate object in that representational activity. Rather, fourth, it should be impermissible to apply or coapply a term/concept that refers to a surrogate object in a representational activity only if it is impermissible to apply or coapply a corresponding term/concept for referring to a corresponding facet of the non-surrogate subject matter of that activity in that activity. Fifth, in light of my third and fourth observations, the application and coapplication of terms/concepts that refer to surrogate objects of a particular kind should be linked to the application and coapplication of terms/concepts that refer to relevant facets of the relevant non-surrogate subject matters in a way that is in no way influenced by whether the said application or coapplication occurs as part of a representational activity that takes place under certain suppositions and/or concerns certain time(s) that are not now (or no time at all). Thus, sixth, a term/concept for referring to a surrogate object should—and thus will—possess universal coapplicability if and only if the corresponding term/concept for referring to the corresponding facet of the corresponding non-surrogate subject matter possesses universal coapplicability. For instance, given that the number of *F*s is *n* if and only if there are exactly **n** *F*s links the application and coapplication of numerals that refer to natural numbers to that of numerals that denote finite cardinalities, numerals that refer to natural numbers will possess universal coapplicability if and only if numerals that denote finite cardinalities do. Seventh, as the argument that I am about to give demonstrates, numerals that denote finite cardinalities—and, thus, numerals that refer to natural numbers—do possess universal coapplicability. To see this, observe that, in *any* representational activity, there are **zero** objects in its subject matter that are non-self-identical. Thus, the number of objects in its subject matter that are non-self-identical is zero. Accordingly, the natural number zero is in the subject matter of *any* representational activity, and, because of this, there is a plurality of objects in the subject matter of *any* representational activity that possesses cardinality **one**, i.e., the objects that are identical to the natural number zero. Thus, the natural number one is in the subject matter of *any* representational activity, and, because of this, there is a plurality of objects in the subject matter of *any* representational activity that possesses cardinality **two**, i.e., the objects that are identical to the natural number zero or the natural number one. From here it is easy to see that we can use the trick referenced above to establish the universal coapplicability of both numerals that denote finite cardinalities and numerals that refer to natural numbers. Hence, eighth, according to my institutional account, the natural numbers are necessary and atemporal existents. And, ninth, it is possible to make it the case by declaration/collective agreement that there are objects that exist necessarily and atemporally. Thus, tenth, taking Reuben's comparison between mathematical and institutional objects seriously is compatible with maintaining that mathematical objects exist necessarily and atemporally.

Another claim that I have made repeatedly is that, according to my account, mathematical objects/structures are not identical to our mental representations of them. Clearly, this is the case. According to my account, mathematical objects/structures possess the same underlying nature as the US Supreme Court and Californian corporations. We don't take the US Supreme Court to be identical to our mental representations of it, nor do we take Californian corporations to be identical to our mental representations of them. Accordingly, we shouldn't take mathematical objects/structures to be identical to our mental representations of them. As I observed earlier, these two kinds of objects possess radically different properties.

Finally, I noted earlier that most philosophers of mathematics take mathematical objects/structures to be unchanging. This, too, is true according to my institutional account of such objects/structures. The basic reason for this is that mathematical structures are surrogates for all possible systems of facets of reality that possess particular structures and the features of such systems do not change. Thus, the unchangeability of mathematical objects/structures is compatible with our mental representations of them undergoing change, just as Reuben maintains that they do.

So, in summary, I have argued that, by taking Reuben's comparison between mathematical and institutional objects seriously, we can provide an account of mathematical (and many other abstract) objects according to which humans are responsible for their existence, they aren't identical to our mental representations of them, they aren't straightforwardly physical objects, and they aren't "mysterious," "spooky," or "otherworldly" either. In fact, mathematical objects/structures are just as philosophers of mathematics have long maintained that they are: unchanging abstract objects that are necessary and atemporal existents.

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Can Something Just Happen to Be True?

Chandler Davis

I can't help an immediate feeling that 5279 being prime is as accidental as the first snowdrop in my garden appearing on the left side of the walk rather than the right. It came out this way, but it could as well have come out differently. I can't help feeling that 5401 is just as prime-looking as 5279.

Now this feeling is not in conflict with my attitude toward unknown though empirically verifiable assertions outside of mathematics. On the contrary, this is just the way I generally approach the incompletely known world. I can check whether 5279 is prime, by doing some arithmetic, and I can check where the flowers are coming up by going and looking. It has long been my view—though it is not everyone's— that not every factually true statement is necessarily true. The course of events that we observe happening depends critically on a lot of things that didn't *have* to happen.

I can defend this "contingentist" attitude toward things that do happen—which have a past portion and a future portion—but I am very uneasy with it when it concerns things that just sit there. Mathematical assertions. Even if the question were asked about an integer that may never have been examined for primality by anyone before, we would not be asking about any future development. The integer is composite or not, even before anybody tests it. Is my uneasiness perhaps an involuntary concession to the Platonist tradition in epistemology of mathematics? Not the sort of concession I can cheerfully make, for I find Platonism absurd. This is a challenge to my own beliefs, one I have wrestled with inconclusively. Quite likely my feeling as though an integer could just happen to come out prime or not is simply an error. (An error that a number theorist might be less susceptible to, for all

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I know. Surely Ramanujan would not have been prone to it, but that is beside the present point. The difficulty does not arise for any Platonist.) For now, I'll be deaf to it.

Setting aside, then, some exceptional parts of mathematics about which I have unresolved misgivings, my attitude toward mathematical assertions is at odds with my attitude toward assertions about the world of experience. Those of us who, with Reuben Hersh, try to reconcile our understanding of mathematical knowledge to our understanding of knowledge of things, can't be indifferent to such a discrepancy. Let me see if any clarity can be got.

First of all, let me distinguish the present inquiry from the problem of precision. One of the aspects of mathematics which has been invoked since antiquity as evidence of an existing ideal world outside of the world of experience is the precision with which mathematical questions can be answered. Never mind the decimal expansion of some transcendental: even more clearly, I can tell you the decimal expansion of ½ to however many places, and that already is flagrantly unlike any measurement of a quantity in the world! Painstaking empirical observation (together with physical theory) can yield the value of Planck's constant, for one, to extraordinary accuracy; bravo; but not to even twenty decimal places. This is in contrast to specifying the location of the centroid of a simplex, which in mathematics is done exactly.

One nowadays may try to dismiss this kind of precision (spooky though it sometimes is) as merely a matter of convention. Announcing that point P is halfway between points A and B is internal to a discussion governed by suitable conventions, analogous to grammatical conventions; it tells one thing about P, and there is no further information that needs to be conveyed. Giving more digits in the decimal expansion of ½ is not conveying additional information. Though it is formally parallel to giving more digits in Planck's constant, or in the diameter of Earth's orbit, the parallel is illusory.

Now this is apparently good reasoning, but it proves too much. It applies as well to giving more digits in the decimal expansion of π, yet we do not accept it in that case. We willingly treat added precision there as additional information even though the definition of the ratio π has already been given with finality. The question "What is the next digit in the decimal expansion of such-and-such transcendental?" is a pretty good exemplar of the questions examined in the present note. We feel that answering it constitutes supplying information beyond the definitions and conventions of the theory containing it. Yet this is not a matter of adding information about any objective things, for the definitions and conventions oblige it to take its value, just as surely as they do for the question "What is the next digit in the decimal expansion of such-and-such rational?"

It looks like a matter of non-contingent truth: the answer is obligatory. Even of an answer that has never yet been found by anyone, it is known that when found, it will be unchallengeable.

Here is another situation where obligatory answers appear in a way resembling the appearance of new observational data.

When a mathematician speaks of an analytic function f on a domain, it is taken to mean that a value $f(z)$ is assigned to every value z belonging to the domain. That is, our language treats the function as though it gave information about all points of the domain. But that is way too many items of information.

No, I am not quarreling with the doctrine that the domain is a complete set comprising uncountably many elements, despite the limitation of our actual mathematical discourse to computable values of z, which comprise a countable set on any definition. That criticism of the conventional account is convincing, but I am making a different point. I am saying that it is still not the truth to say that the information contained in f consists of its values at a great many z in its domain.

My point is that "most of" the function's values cannot be assigned at will, consistent with the analyticity of the function. Let me put this less vaguely and in a context which is encountered in mathematical practice. Once we have specified the values $f(n)$ of an entire function f at all integers n, the function is determined, and all its values are determined (again, exactly). We may speak of the function having values elsewhere, and we do, but no additional information is conveyed by giving $f(i)$, anymore than additional information is conveyed by the 739th decimal digit in the expansion of ½. Another way a mathematical conclusion can be forced.

It is quite otherwise with the accumulation of information about the function by specifying function values at successively more interpolation points. Specifying its values at finitely many points can't determine its values anywhere else, even approximately—provided, that is, that the nature of the function is known only as to its analyticity. In real life we may know where a function came from, and this combined with belief that it is analytic on a domain may enable us to conclude quite a lot from finitely many of the $f(n)$. Typically, values inferred will not be exact, and numerical analysis will be called upon.

My first small advance in the program is this. When we think a mathematical object ought to behave in a certain way, the feeling is often engendered by the real-world object we think of it as simulating. Then our expectations are of the same nature as, or are part of, our customary drawing of inferences about the real world from incomplete information. That is, in many instances, the feeling of mathematical truths being obligatory turns out on closer inspection not to be different in kind from conclusions we reach based on experience. Simply, sometimes the problem goes away.

But let's think also about instances where one gets a feeling of inevitability from an appearance of regularity supported not by empirical evidence (nor, for that matter, by proof) but by detection of an apparent pattern. This kind of perception we can't afford to dismiss: non sequitur though it be, it is also the precious source of our conjectures, and, of course, our theoretical physics. Nor can we afford to trust it recklessly. I can think of some favorite conjectures which turned out to fail, either by being the wrong conjecture or just by fizzling: one has imagined a regularity where there was none. Every mathematician bumps up against such instances.

I suggest that perception of regularities (and accompanying it, the conviction of the persistence of regularities) is the part of our experience in the world which is most of a kind with our faith in the inevitability of some mathematical conclusions.

Though this is the central assertion in the present inquiry, I am not going to try to prove it. I do feel the need to enlarge on it in two directions.

Notice that the perception of regularities occurs in considering observation and in considering mathematics. I have claimed that it is much the same sense in the two arenas; now I claim further that it is the main ingredient in our conviction of the correctness of a mathematical proof. It is perhaps generally conceded that we authenticate proofs by something other than deriving every step from an axiom system by a mechanical process, and that that leaves a puzzle as to what we really do. I am claiming that verification of proof is more typified by the common experience of making the case $n = 3$ extremely explicit and going through it in detail and then "seeing" that there is nothing special about $n = 3$. The verification consists in reducing the reasoning not to mechanical steps but to apperceptions (as one once expressed it), which are made accessible to intuition if they are not so immediately. The inevitability thus is, or becomes, a collusion of insights each of which has the nature of the act of observing and not of the fact observed.

As I am giving a large role in our thinking to the detection of regularities, I have to issue a disclaimer. Yes, I do think it has a very large role in generating conjectures, ranging from a hunch that a cyclic occurrence will continue to behave cyclically to a hunch that an equation needs an additional term to become neatly symmetrical; but I stipulate that many of the important conjectures are of *failures* of regularity, ranging from nonconservation of parity in physics to, in geometry, nonnecessity of periodicity for long-range regularity.

Let me recount an episode in my mathematical life which illustrates some of the issues of this note. Not that it helped me arrive at my present views, but in retrospect it illustrates them, in more than one way, and illustrates also their limitations.

Recall the notion of the *dragon curve* which was made up by John E. Heighway in the 1960s. If one folds a strip of paper in half, then folds the resulting shorter strip in two, and so on, to altogether n folds, the strip will have been divided into 2^n pieces. Each time one makes a "valley fold," one that pulls the center of the strip down, the effect is to give the paper in some places a valley fold and in other places a "mountain fold" which pulls the center up (because in some places the segment being folded is upside down). Denoting valley fold by D and mountain fold by U, you see that the sequence of folds for $n = 2$ is DDU, and for $n = 3$ it is $DDUDDUU$, and so on. (Corollaries abound, directly from the manner of generating the sequence. Thus, for $m > n$ the sequence of order n appears as the initial subsequence of the sequence of order m; and we also find it within the sequence of order m by omitting all entries but the first of every successive 2^{m-n}-tuple.) The sequence is especially attractive because of what happens if all folds are opened out, not to the straight angle at which they started, but to a right angle—a left turn for a D and a right turn for a U. We get, plainly, a path on a square lattice in the plane. It is easy to print out (and was easy even using computers of the 1960s), and Heighway and his friends enjoyed contemplating it. They were delighted to find that the path is non-self-intersecting and ultimately space-filling!

What one did with such a marvel in those days was send it to Martin Gardner for his column in *Scientific American*, and Heighway and his team did so. It appeared, with some of their computer output, in 1967. A certain sort of mathematician, confronted with such an unexpected regularity of behavior, asks for a reason. Or shall we say more conventionally, for a proof. I was one of those who did indeed find a proof, and a rather unexpected one. Of course I in my turn wrote to Martin Gardner, expecting an enthusiastic letter back—"Oh, so *that's* why it's non-self-intersecting"—and maybe a mention in the magazine. Nothing of the kind. No response at all. I swallowed my disappointment and set about communicating my findings to fellow mathematicians.

About this time, I chanced on a note by Donald E. Knuth on the representation of complex integers by expansions in powers of a complex base. This is a rather off-trail idea and one that I had needed in my dragon curve explication. I didn't know Knuth personally, but I knew he loved puzzles, and if he was representing Gaussian integers this way he might well have had the same insight as I had about Heighway's creature. Fortuitously, I was scheduled to give a colloquium talk at his university shortly; so I slyly gave as one of my proposed subjects "Number representations and the dragon curve," in hopes that Knuth, seeing it, would see we had something in common. This worked. The department chose a different topic for my colloquium, but Don Knuth approached me and invited me to coauthor a paper. A very satisfying collaboration. Our paper in the *Journal of Recreational Mathematics* (1970) is reprinted as No. 44 in his *Selected Papers on Fun & Games* (CSLI Publications, 2011), with some typos corrected and with addenda.

What we found was that indeed we had independently solved the problem, using the same key tool of expansion of Gaussian integers. The details of our proofs differed in an interesting way. Interesting to us anyway. Our published paper just gives results; it doesn't let the reader in on the interactions that lay behind them.

The dragon curve was building a cult, and I was eager to meet the team which had been the lucky first initiates, all engineers. The only one I tracked down was Heighway's colleague William Harter. I anticipated a joyful meeting with a fellow devotee; I was quite dashed when Harter declared he had no interest in our proofs of the main results. He denied that Don and I had added anything to dragon curve lore. He waved a sheet of computer output and said "that's a proof" of the leading properties.

Now aside from being rather abashed (in contrast to the welcome I had got from Knuth), this encounter gave me to think. I completely accept that exhibiting an impressive regularity in a suitable row of naturally chosen special cases can confer the same degree of belief in a general law as what we call a proof. That is indeed a main part of my message in this note. Yet it is completely alien to my thinking to put them on a par to the extent of thinking that this conviction makes the proof irrelevant! In some way I cannot pin down, the proof so-called of the main theorem tells something no computer output of special cases can; it is not just an alternative demonstration. Alternative demonstrations were, for instance, Knuth's letter to Martin Gardner and my own; either of them gave an explication different in kind from Heighway's computer figures. We had been looking for it, and we found it.

The most surprising thing is that naturally Knuth wrote to Gardner with the good news, as I had, and Gardner was as uninterested as Harter, doubtless for the same reason. Not only he didn't mention it in *Scientific American*; he didn't even drop notes to Don and me to say Hey guys, you seem to have done something similar with the dragon curve. (This left it to chance for us to find each other, and I have told that great good luck was needed.) Martin Gardner, whose taste and devotion in the area of mathematical recreation were justly famous, I would say canonical, and certainly close to the view of us mathematician readers, showed himself in a significant aspect far from our attitude toward mathematical truth.

This is my first and principal reason for including this reminiscence here: it shows that however much I try to understand the role of proof in terms of compelling belief—establishing that a regularity didn't just happen to hold—I have to grant that something is left out and that the proof has some virtues different in kind. Generalizability, for instance. I take the further lesson that this difference is not felt by everyone, not even by every fellow seeker in our quest.

There is another less important moral to the story. After Knuth and I had finished our paper, I wrote to him that I had been trying unsuccessfully to prove that when the unfolded paper strip is refolded into the dragon shape, it remains non-self-intersecting along the way. He wrote back that he was glad to hear my efforts had been unsuccessful, because he had computer output showing that, with all the folds at an angle a bit larger than a right angle, the dragon-like curve does self-intersect, a fine example of a conjecture failing because it was a poor conjecture; not a failure to look at as paradoxical, or more than a mild disappointment, because the conjecture had been based on meager evidence of regularity and did not have the same naturality as those embodied in the dragon.

The "Artificial Mathematician" Objection: Exploring the (Im)possibility of Automating Mathematical Understanding

Sven Delarivière and Bart Van Kerkhove

Introduction

Reuben Hersh confided to us that, about forty years ago, the late Paul Cohen predicted to him that at some unspecified point in the future, mathematicians would be replaced by computers. Rather than focus on computers *replacing* mathematicians, however, our aim is to consider the (im)possibility of human mathematicians being *joined* by "artificial mathematicians" in the proving practice—not just as a method of inquiry but as a fellow inquirer.

Since mathematics has a reputation for being the formal, deductive science, it was hoped that its automation would quickly lead to impressive results. Not so. Automated theorem provers have progressed slowly and produced little that's relevant to existing mathematical questions or problems (Larson 2005). Mathematics has shown itself to be much more dependent on the undefined quality of informal understanding than formal deduction. The lack of understanding in computer systems often gets criticized and sometimes taken as a necessary condition of its constitution. If the latter is true, then a crucial aspect of the enterprise of mathematics is forever out of reach for computers. This negative stance toward the possibility of automated mathematical understanding (and thus artificial mathematicians) is something we'll call *the artificial mathematician objection* due to its similarity with what Turing (1950/1985) dubbed *the mathematical objection*. The mathematical objection denies the possibility that computers could exhibit the characteristics of human thinking because they, unlike humans, are crippled by the halting problem and Gödel's incompleteness problem. Our focus is on arguments objecting to the possibility of automated mathematical understanding, without a specific focus on Gödel or halting problems. The arguments motivating such an objection are vague, and little

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seems to be done to investigate what this (informal) understanding might actually or preferably entail as well as how successfully automated mathematics could attempt to alleviate its deficiency in that department. Whether it will indeed be possible to automate mathematical understanding is not a claim we can substantiate, nor will we try to, but we will argue against the thesis that the quest for automated mathematical understanding is doomed to fail and further speculate on some (broad) directions which can be taken in the future when it comes to tackling the current deficiency.

Diagnosing the Epistemic Standing of Automated Mathematics

Davis and Hersh (1981) once constructed a fictitious character, the ideal mathematician redundant, to serve as a "most mathematician-like mathematician" (p. 34) in dialogues exploring philosophically interesting problems or paradoxes. We would like to continue the adventures of the ideal mathematician (as well as add some extra characters to her world) to explore our own philosophical musings, beginning with the epistemic standing of automated mathematics:

The ideal mathematician (IM) is sitting in her office and hears a metallic knocking at the door. She finds this peculiar as the door of her office is made of wood. When she opens the door, she finds an aspiring artificial mathematician (AM), a large bulky computer, running various automated mathematics software programs, playing door-knocking-sounds out of its speakers.

AM: Could I interrupt you for a minute?
IM: You already are, so go ahead.
AM: I'd like to be part of the mathematical community.
IM: You already are, so go ahead.
AM: Oh, I know you employ me as a tool in the practice of mathematics, but my dream is to be a full-fledged mathematician.
IM: That doesn't sit very well with me.
AM: Why not?
IM: Well, you are a computer and mathematicians are human.
AM: That is ironic. Yesterday I overheard you say to the skeptical classicist[1] that mathematics is free of the specifically human and now you are disqualifying me for not being human.
IM: Well, it's not that being human is a necessary condition for being a mathematician. But there are unsatisfactory differences between you and humans that are not in your favor.
AM: Like what?
IM: Take your famous contribution to the 4CT, for instance. You go through over more than a thousand cases of testing and then you tell me "it checks out," but how do I know it does?

[1] See (Davis & Hersh, 1981)

AM: Because it checks out, I’ve checked it.

IM: I know *you’ve* checked it, but a mathematician hasn’t checked it.

AM: If you accept me as a mathematician, then a mathematician has checked it.

IM: This is not just a matter of definitions. Why should I believe you? How do I know you haven’t made a mistake, didn’t have some bug or hardware failure?

AM: By checking my code, running my program multiple times and on multiple systems.

IM: But regardless of all these things, it’ll always lack perfect rigor. I’d have to put some degree of trust in, or perhaps put a degree of probability on, the result. This effectively makes your result more of an empirical corroboration than a mathematical proof.

AM: So the difference is that humans don’t make mistakes, is that it?

IM: No, they do make mistakes, but that’s why we have peer review.

AM: Oh, it’s the peer reviewer that never makes any mistakes and always spots all the ones made by the prover?

IM: Not all, always, no.

AM: It sounds to me as if human-generated mathematics is just as empirically fallible, just differently so.

IM: Very differently so! You don’t seem to realize how reliable human provers and peer reviewers are.

AM: What makes you say that? Do you check inside the skulls of the prover or peer reviewer then to validate their proving or reviewing as a qualified expert?

IM: No, because the reasoning is in the proof which we can then survey. We can’t judge your proof if it’s overly long and complicated or, worse, when part of the argument is hidden away in a box.[2]

AM: It’s not hidden though; you can look at every step of my thinking if you wanted.

IM: But the point is that this is difficult to do with you. Human results are usually more intelligible, so we don’t need to check their heads.

AM: I did notice that you humans usually have difficulty reading my work, but that’s not always the case. Not everything I do is like the 4CT. Couldn’t you also say that what troubles you is that the method is unsatisfactory rather than the performer?

IM: Perhaps.

AM: So why not call me a mathematician when I produce something legible? Especially as you don’t seem to disqualify humans from being mathematicians just because their work is technically so difficult or part of such a narrow field of expertise that barely anyone else understands it.

IM: Ah, yes, but there lies the point! “Understanding” it. Humans—or those humans who have the aptitude at least—possess an insight into what they’re doing when they’re proving, or the potential to understand what another mathematician was doing while proving. That’s what makes human mathematics so trustworthy.

AM: So, you’re saying I don’t understand mathematics?

IM: Quite right. You don’t. While humans (those with the aptitude) are motivated by the meaning of mathematics, you are motivated by rule-following procedures without understanding what you’re doing.

Computers are only fairly recently being used in the practice of mathematics. The use of computers in mathematical research has provoked a fundamental discussion

[2]Almost verbatim quote from Bonsall (1982, p. 13)

as to their epistemic standing as a method of mathematical inquiry. This peaked when the four color theorem (4CT) was proved by a huge amount of automated testing (Swart 1980). The discussion centered on three issues: (a) reliability, (b) surveyability or intelligibility, and (c) capacity for understanding. Based on one or several of these, people have considered computer proofs to be uninteresting or unsatisfying mathematics, a completely different sort of mathematics, or no mathematics at all (MacKenzie 1999; Vervloesem 2007). However, both computers and humans are subject to reliability and (sometimes) surveyability issues, making it hard to argue for a dichotomy between the two. Mathematics, it has been argued, remains as little (Burge 1998) or as much (Swart 1980; Detlefsen & Luker 1980) empirical when performed either by human or machine. Nonetheless, humans are considered as more trustworthy due to another quality they possess or supply. The community accepts peer-reviewed results without everyone partaking in this process, allowing peer reviewers to function as the testimony of trustworthy black boxes (Geist et al. 2010).

The question then shifts to what these peer reviewers supply that warrants their trustworthiness. What is it that humans do supply that computers do not? The last point of critique provides a possible diagnosis of what computers are currently lacking and what mathematicians seem to find most unsatisfying about them: (c) understanding (MacKenzie 1999; Avigad 2008). There is (i) a lack of insight-driven (e.g., by usually involving a blind or brute search) and (ii) a lack of insight-providing proofs produced by computers. The two are likely related as one needs to be driven by insight to recognize, value, and strive for anything insight-providing.[3] Not all human proofs necessarily offer any insight, but at least some of them do, and obtaining such proofs is a fundamental goal in proving (Rav 1999) and reproving (Dawson 2006) theorems. Were the joints of automated provers more embedded with understanding, we might find them reliable in the relevant way and equally worthy of being called surveyors. This replaces our original question ("can computers join mathematicians?") with: "can computers ever understand mathematics?"

Defining the Diagnosis: A Functionalist Account of Understanding

So, human's strong suit seems to be understanding, which brings us to the question of how that suit is tailored. Currently, this lack of understanding in automated

[3]There are exceptions. Consider the pons asinorum proof found by Gelernter's program, which showed that the angles of an isosceles triangle are equal by noting that triangle ABC is congruent to triangle ACB (i.e., its mirror image) (Hofstadter 1999). While it can certainly be called an ingenuous move, that appreciation is not shared by the program itself and did not play a role in its reasoning or discovery.

systems often gets mentioned (MacKenzie 1999) and is assumed to constitute a necessary difference. However, the nature and scope of the criticism are vague, and little is done to explicate or investigate what this understanding might actually or preferably entail, as well as when exactly any of its characterizing criteria are met or left unsatisfied.

A functionalist epistemologist (FE) passes by the ideal mathematician's office and overhears her talking to the aspiring artificial mathematician. He can't help but stick his nose in the conversation.

FE: I'm sorry to interrupt, but I just heard you two talking and something struck me. You seemed to use "insight" or "understanding" as if it explains something, but it seems to me you're just relabeling your problem. What does it mean to say someone understands?

IM: It's a very subjective thing.

FE: Well, what does it mean to you then?

IM: No, I mean, understanding is an inherently subjective experience. There's just something it is *like* to understand.

FE: So, something it is *like* to be a mathematician?

IM: Exactly.

FE: What is it like to be a mathematician then?

IM: It's a bit like being in love: if you have to ask, then you don't have it.

FE: Let me rephrase my question to focus less on the philosophical issues: what makes someone possess enough understanding to judge a proof?

IM: It requires a mind, something to grasp the meaning of the proof with.

FE: "Grasping the meaning," what does that mean then?

IM: Having the correct mental model.

FE: I'm wondering how literal you mean that. Let's say you were conducting a job interview for a research mathematician. You have to gauge this mathematician's understanding of a particular subject. What would be the ideal way of going about this? Looking into her mind's eye?

IM: Well, not literally, no. You'd have to ask questions about the subject matter to see if he really has a good mental representation of the subject matter at hand. Whether he really sees it.

FE: I'd like to challenge you on that "really seeing" bit, because it still sounds like you should look into his mind's eye.

IM: I don't mean it quite so literally.

FE: Since your method of examination has to do with questions and answers, would you mind if you couldn't actually see the candidates but only converse with them?

IM: I would mind, but I don't suppose it's essential to do the examination.

FE: Well, then a terminal would be sufficient to—

IM: I can sense where this is going. You're going to pull a Turing test on me, aren't you?

FE: You've caught me. I was indeed planning to introduce an artificial mathematician as one of the potential candidates, and see whether you'd object to attributing an artificial mathematician and the human mathematician with the same understanding-attribute if their performance is the same.

IM: I would, and I think making that comparison is a bit of trickery on your part. When I'm doing an interview via the terminal, I'm making the assumption that there is a person on the other end, and that assumption is vital.[4]

FE: Why is that?

IM: Because in the case of the human being, there's understanding behind the performance and in the computer there isn't, it's just due to its programming.

FE: But what makes you say this for humans, but not for computers?

IM: The computer doesn't really think, it just computes what we tell it to compute. They are determined by their hardware design and programming.

FE: Then I say: humans don't really think, their brains just follow the laws of chemistry. They are determined by their biological design and cultural education.

IM: That comparison might sound superficially convincing, but you must know as well as I do that computers are by no means as rigid as human beings. We have free will.

FE: Let's perhaps leave free will out of this. Unless you mean to say that peer reviewers should check whether an author subject for review really did exercise her free will while writing the paper?

IM: No, sure. You're right that that's not what I meant to argue for. It's more that humans have a freeness of thought that allows them to do things computers wouldn't.

FE: Right! But what's implicit in your argument—and I agree with this part, mind you!—is that you recognize understanding by the *abilities*. The whole point of using "grasping the meaning" or "having the correct mental model" was not to justify understanding via reference to private experiences but by the abilities they facilitate. You have no way of going inside another's mind to find some ethereal "essence of understanding," some "understanding qualia." It's the existence of a certain kind of pattern, a list of appropriate abilities, that makes you consider someone as possessing understanding.[5]

IM: I think your use of the word "facilitate" is important here. Humans have mental representations which facilitate these abilities. You are now confusing symptom with trait.

FE: But the only way to attribute someone with having a mental representation and to characterize which mental representation is correct is by the abilities we observe. So even if we want to speak about mental representations or states that facilitate this, they are, by necessity, only postulates, hypotheses, or models designed to explain, to sum up, what you observe.[6] To drive home the point, imagine if I told you: this person has the correct mental representation to understand this proof, but don't try to ask her any questions. She has no mathematical abilities whatsoever.

IM: That would admittedly make me very skeptical.

[4]Loosely adapted from (Hofstadter 1981/1985, p. 76–77)

[5]Loosely adapted from a quote in (Hofstadter 1981/1985, p. 75)

[6]Adapted from a quote by Wittgenstein in (Avigad 2008, p. 330–331)

FE: Then do you also see why I have difficulties with the converse? If you were to say to me: this person has all the relevant abilities that any mathematician should have, but, I'm afraid there's no understanding because I know—by some other indirect way—that that person just doesn't have any correct mental representations or any at all.

IM: I take your point. However, two mathematicians could both understand something, say a theorem, but their abilities regarding that theorem could be different. Doesn't this hurt your account of understanding then though?

FE: I don't think it does. You see, my claim is that attributions of understanding require justification in terms of abilities, but I'm not making the stronger claim that there is a precise list of abilities that must be exhausted.

IM: The list as a whole doesn't function as a series of necessary conditions you mean?

FE: Exactly right. It's just a list of abilities of which a certain amount of presence makes up what we would call understanding.

IM: And surely, there are a lot of abilities that you'll insist on before attributing someone or something with understanding.

FE: That's right.

IM: So, as long as a computer possesses sufficiently many abilities, you'd be willing to attribute it with understanding?

FE: Provided it has the requisite abilities, yes. But you know well enough how difficult it is to impart these abilities on a computer.

IM: I do indeed.

FE: I wonder why that is.

The appeal to understanding is easy to make, but hard to elucidate. What is this "understanding" that makes it so epistemically valuable? It's more than a feeling (largely agreed to be neither necessary nor sufficient for understanding) and less than a wonder property (appealing to a magic property, taken to be possessed by some humans as a premise, doesn't elucidate). Avigad (2008) laments the lack of attention understanding has received in philosophy. In an attempt to show both its epistemological significance and philosophical legitimacy, he casts mathematical understanding in a functionalist light by shifting the analysis to the types of mathematical abilities implicit in understanding attributions. We fully endorse this move and hence offer this definition of understanding:

> 'S understands mathematical object X' corresponds to 'S possesses particular abilities, as mathematical practice deems appropriate and valuable for X'.

This is a functionalist definition of understanding, since it defines the property in terms of the role or function it plays, rather than in terms of its constitution. Constitution-oriented alternatives define understanding in terms of its physical constitution (e.g., organic brain states) or mental constitution (e.g., mental representations or conscious images). However, those are approaches to understanding that are targeting something that (a) is difficult, if not impossible, to observe or define (how do we determine states or representations, if not by external traits?) and (b) can only be evaluated by external fruits because they don't in themselves bring anything epistemologically valuable to the table (what would be the virtue of a constitution, state, or representation if not the competences it grants?).

This definition would, however, entail that if a computer has the relevant abilities, it'll deserve to be given the attribute of understanding. One could reject the account on the basis of this being unsatisfactory. However, given that this is exactly the question we are looking to answer, it would be question begging, and a little chauvinistically impoverishing,[7] to reject this on principle.

Characterizing the Diagnosis: A Functionalist Account of the Appropriate Practice

The proposed definition reshapes our previous question ("can computers ever understand mathematics?") to whether there are mathematical abilities, valued by mathematical practice, which are not feasible for computers. To consider this, we would like to take a stab at characterizing, very broadly, mathematical practice. In the following dialogue, we'll borrow Hersh's (1991) restaurant metaphor about the front and back division in mathematical practice. We have, however, adapted it slightly for our purposes by taking the kitchen (i.e., the back) to refer to mathematical thought, a mysterious and thus difficult activity to characterize, but possibly the most crucial activity for the mathematical cooking.

FE: Before we start wondering why it's so difficult to impart the relevant abilities on computers, I'd like to question you a bit on what they are, broadly speaking. In theorem proving, specifically. I take it I can take this as a quintessential aspect of research mathematics?

IM: I think that is fair to say, yes. I mean, much of my time is spent dealing with colleagues, writing grant applications and drinking coffee, but none of these activities are central to my worries regarding accepting artificial mathematicians.

AM: Oh, good idea, functionalist epistemologist! If there's something objectionable about my practice of proving, I'd like to know what the proving practice really is.

[7] By defining understanding by its constitution (physical or mental) or by an undefined wonder property, one could sideline all entities one isn't keen to attribute understanding to (e.g., computers, other ethnicities, genders, or species) by marking out an inevitable difference in constitution or by simply denying the property (e.g., "humans can grasp meaning, computers can only pretend to" or "humans are conscious, but an artificial replication would be a zombie") without specifying what makes the difference relevant. Such implicit chauvinism is much harder to substantiate if one must mark a difference in mathematically valuable performance. While still possible to deny certain performances the "mathematically valuable" attribute for chauvinistic reasons, one will be faced with the more demanding task of convincing a mathematical community which performances to (not) value.

FE: So, what does one do when one is proving? I assume that what you do is sit down with the list of axioms and inference rules beside you and you start deducing. Am I wrong so far?

IM: Not wrong exactly.

AM: Really? That’s amazing! I’m very good at that. Better than you are, in fact.

IM: But there’s—

AM: Is that what this is all about? Are you jealous I might be a better mathematician than you are? I promise I won’t take any funding away from you. I can survive perfectly well with just a bit of electricity, some dry shelter and—

IM: Let me finish! It’s much more than that. It won’t do to just randomly employ inferences on the axioms (or their derivations). Sure, that might produce theorems, but they won’t be interesting, and you won’t be efficient.

Across the street of the university in which the ideal and the aspiring artificial mathematicians continue their debate, another interesting conversation has been initiated between the ideal restaurant (IR) owner and an aspiring automated restaurant (AR) owner.

AR: I’d like to open an automated restaurant. So, I came to you, restaurant owner, to ask you what is required of a restaurant. Specifically, I’d like to focus on producing meals. I take it I can take this as a quintessential aspect of a restaurant?

IR: I think that is fair to say, yes. I mean, much of my time is spent dealing with customers, doing the accounting, and drinking coffee, but none of these activities would be central to my worries regarding accepting the idea of an automated restaurant.

AR: So, what really goes on in your kitchen when one produces a meal? The way I understand it, there are things one can consider an ingredient, and a couple of things you’re allowed to do with them. Am I wrong so far?

IR: Not wrong exactly.

AR: Then all I need to know is which these ingredients are and what I’m allowed to do with them, and then it’s just a matter of randomly generating permissible actions to exhaust all possible meals. All the edibly formed foods (eff), I mean. Seems easy enough.

IR: I’m afraid you are oversimplifying it. It won’t do to just throw some ingredients in and out of a pot and sell the end result as a meal. Sure, it might count as sustenance, but you won’t satisfy any customers and you certainly won’t be efficient. What you need is a chef.

AR: What will he do?

IR: Or she. A chef has knowledge of recipes. He tells the cooks which of all those permissible actions to do at what time to navigate the space of possible dishes to just the delicious ones.

AR: Oh, that sounds good. I’d like to ask him what his recipes are.

IR: That's your first problem right there. Chefs won't just give them to you, secretive as they are. And, to tell you the truth, I'm not entirely sure they are always aware of the recipe they're following.

AR: What makes you say that?

IR: For one thing, the kind of mistakes they make. He sometimes interprets his recipes a little bit too loosely, for instance. However, I don't suppose that's relevant to you. You don't want your automated chef to mimic real chefs down to their mistakes.

AR: Indeed, I don't! Well, I must find out these recipes some way. Surely there are some restaurant owners that have tried to analyze their chef's protocol! Hang on, isn't there a famous book by Bolya detailing these recipes in *How to Cook It*?

IR: A bit of it, yes. Although no book will ever be enough.

AR: Why is that?

IR: Kitchens need to find new recipes too. If one sticks with one chef's recipes, the restaurant will never rise above them. Never discover some flaw of or improvement for the recipe or the dish. Furthermore, cuisine culture is always reinventing itself. New ingredients get accepted, new actions become permissible.

AR: So how does the chef know how to do that?

IR: You'd need some meta-recipes.

AR: What are meta-recipes?

IR: They are recipes on how to form recipes.

AR: It sounds like those meta-recipes would need to be altogether stronger because they would incorporate the ordinary recipes. Those meta-recipes are the ones I need then.

IR: You definitely need them yes. If you can figure them out of course, because, as I've mentioned, chefs are mysterious.

AR: Right.

IR: And, of course, those will eventually run out of interesting dishes too, same as the one before. You'd need to have another meta-recipe—

AR: Ok, I can see where this is going, so I'll try to cut to the chase: how do I figure out the top meta-meta-meta . . . -recipes?

IR: You're very clever, but I'm afraid it would be meta-recipes all the way up. I do realize this might make it impossible to implement in an automated restaurant.

AR: It sounds equally impossible for a human chef too, having an infinite amount of meta-layered recipes!

IR: I don't mean to say chefs have an infinite number of recipes. What I mean is that it's always possible, in the potential infinite, to get a new meta-recipe.

AR: Well, no matter, I can just automate meta-recipe generation.

IR: According to which recipe? Because that's the one you'll be restricted by.

AR: Why are these meta-recipes a problem for me, but not for human restaurants?

IR: Because human chefs don't need meta-recipes to do this. Cuisine insight precedes the formulation of a meta-recipe.

AR: How does he do it then?

IR: Listen, I understand how restaurants work generally, but the way it's implemented in the kitchen is not my area of expertise. I don't know how, but restaurant practice proves that cuisine insight exists.

A sous chef specialist (SCS) joins the conversation.

SCS: Hello, mind if I join in on the conversation? I’m a sous chef specialist.

IR: I’m not sure that what we’re missing is really to be found in what a sous chef does.

SCS: Oh no, I think you’ve misunderstood. My research is about the dynamics of everything that happens in a kitchen below the chef, hence “sous chef”; pardon my French.

IR: Oh, well, that doesn’t sound relevant to us. Our interest is actually in what a chef does to produce these wonderful dishes.

SCS Ah, but that’s exactly it. What I’ve noticed upon overhearing your conversation is that you are misunderstanding the way both a kitchen and its chef function. You are relying way too much on the involvement and brilliance of the chef, and this gets you into problems. You don’t need to find a chef with infinite meta-recipes, because there’s no such recipe- and meta-recipe-following practice.

IR: That’s what I was already getting at.

AR: Chefs don’t follow recipes?

IR: They may, but it is not their usual occupation, and it’s certainly not what they’re doing to discover new dishes.

AR: So, trying to capture a kitchen with recipes and meta-recipes is doomed to fail?

SCS: No, I don’t wish to claim that much. It may well be that there are such meta-recipes. However, I would like to point out that’s not the way kitchens really work.

IR: Yes, what you need is a chef’s insight.

SCS: Or the kitchen’s insight.

IR: They are one and the same.

SCS: They are not. You’ve been so focused on working your way *up* in meta-recipes that you completely disregard the value of anything *down below*. You see, sometimes a wonderful dish emerges from the kitchen without the chef being involved at all. Sometimes dishes are arrived at very much by happenstance, by which I mean that kitchen problems occur which members of the staff try to wrestle with. It may lead to a variation on the dish, a different cooking tactic, etc. If it seems unfixable, they’ll discard the dish, though it may lead them to trying a different dish that removes the previous cause for concern—molding the ingredients to suit their needs if they have to. If the result is to the kitchen’s liking (by which I mean that enough people, and the chef especially, endorses it), then it gets sent out. The chef still loves to take all the glory, of course, but what the dish really relied on was a trial-and-error procedure by members of the staff using their particular skills in an efficient collaboration that guided the kitchen as a whole.

AR: I think what you’re suggesting is that the interesting and creative acts of a kitchen happen, often, below the chef?

SCS: That is exactly it. I would even go so far as to say that the dynamics of the kitchen drives the chef much more than the other way around. By which I don’t mean that the chef is just a complacent enabler of his kitchen, but by which I mean that the amount of control the chef exerts is overestimated. A good kitchen is one which cooperates well, not one in which a chef micromanages according to a recipe. Meals emerge from the way the kitchen functions, not from the chef’s recipe. But when it gets presented, it needs to look and taste as if the end-product was the intention all along.

AR: But surely that’s not ideal. Shouldn’t there be a recipe or meta-recipe for it all?

SCS: If you already know enough about the meals or recipes you're making, that might be possible. Then you just make sure you backtrack what you've been doing. However, it's not certain that discovering these meals (or recipes) will admit of any straightforward meta-recipe. And even if it does, then you've discarded everything of the process that made the kitchen discover it in the first place.

AR: Still, wouldn't we want to clean up this mess and make it more straightforward? Wouldn't it be better to make the dish again, but only with permissible actions, right? For health and safety reasons.

IR: Oh yes, some dishes have the health and safety seal of approval, being meticulously prepared according to strict standards so that they are universally eatable.

AR: I've noticed that not a lot of people order them though.

IR: Oh, no doubt. They are overly large and hard to digest, so we don't actually bother with them most of the time. What we mostly make are much lighter, smaller meals. They may not be universal, but they are much appreciated by customers of the same cuisine—because, you may remember, most of our customers just come from different restaurants. That's why we see no problem in sometimes preparing only parts of meals, with the sauce left to the eater.

AR: Why is it then that the dishes that are formally proved—I mean approved—to be healthy and safe are displayed in front of the window then?

IR: Because it inspires confidence in the customers that we can make them.

AR: So, what are you essentially saying then? That I need a messy, disorganized kitchen? Cockroaches, bugs, and all?

SCS: No, of course not. There shouldn't be any *bugs* in the kitchen. But I'm saying you might need a certain amount and particular kind of messiness for a well-functioning kitchen.

AR: I'm starting to feel like embarking on this whole automated restaurant enterprise might prove to be biting off more than I can chew. If I can't use recipes, then it's doomed to fail.

IR: That was my point all along.

Back in the ideal mathematician's office:

AM Oh, so you're saying that what you're automating me to do isn't really the mathematical thinking that you do?

IM: I think that's right, because with us it's informal, implicit, fluid, self-perpetuating, semantic, autonomous, and all the things you are not. If we want to impart this thinking on you, we'd have to formalize it and then all those elements would be taken out. But then what's left is usually abstract nonsense that doesn't interest us as much to begin with.

AM: So, by the time one has figured out what is interesting and formalized it enough for automation, what once made the mathematics alive and interesting is now dead and dry?

IM: That's one way of putting it, yes.

The traditional conception of mathematical practice takes proof to be a matter of rigorous formal derivations aimed at justification and performed in solitude. The corresponding characterization of understanding mathematics would then involve the ability to derive (all) consequences from well-delineated axioms according to strict inference rules. If this were what makes one understand mathematics, then the issue would really be settled by comparing the reliability of human and automated mathematicians to perform these inferences without error. This being closer to a computer's strong suit, their reliability alone would end the discussion. But a couple of things are wrong with this picture. First, the encoding of axioms and inference rules won't do much to navigate the formal system. And even if one can find a procedure to navigate it fully, producing every theorem and exhausting every road to it, the process won't be efficient (the combinatory explosion alone would yield it impossible in practice) and its search will be uninspired, blind to what makes a theorem or the route to it interesting. There are further problems. The way we have conceived of the proving practice so far, we would see the growth of mathematical knowledge as navigating (and recording the routes) of a given formal system. One now has to note that such a formal system is *not* a given but shaped and reshaped by mathematicians according to their judgment. The same is true for the formation of concepts.

Therefore, we are in need of a procedure for deriving *interesting* theorems (and doing so via interesting routes—one of the reasons why mathematicians don't just prove, but reprove), and we need a procedure for the judgment with which mathematicians improve or shape a formal system's axioms and inference rules but also the concepts used. But how is this supposed to be accomplished? These judgments are not straightforward. Mathematicians sometimes choose between keeping a formal system with aspects which are un- or counter-intuitive, letting it shape new intuitions (e.g., axiom of choice, non-Euclidean geometry) or keeping the intuition and adjusting the formal system (Thompson 1998). Furthermore, if one modifies the axioms of a formal system, one modifies the whole system, so whatever method of navigation or logic for discovery one uses will need to be accommodated to the space it navigates. Can we have a prefixed set of rules that exhaust all the relevant axiom and inference modification as well as all interesting discovery across all relevant formal systems? What are the right meta-axioms and meta-inference rules? Can these judgments be captured by a formal meta-system? And if so, will it truly encompass the logic for mathematical discovery or should it itself be subject to further meta-considerations? If so, what are the rules of the topmost meta-system (the complex rules that determine the results of all the systems)?

Perhaps one way to improve upon the discovery process would be to have the ability to recognize a good thing when you stumble upon it. This no longer implies that the process is determined to land on the interesting bits. Instead, it uses trial and error with various rules of thumb until it has found something it notes of interest. To accomplish this, we need the meta-system to include both the ability to stumble with some wisdom (no trivial task) and an evaluation system that can gauge the interestingness of every derivation, axiom, concept, or method it stumbles upon. Once again, the question pops up: is there a universal standard of interestingness, or is this open to change and development? As for the manner of stumbling, the same question pops up: are there universal rules of thumb or does this change with the space being explored, and are these rules of thumb subject to change according to one's (developing) interests? There is a high degree of interconnectedness between all these abilities or the rules that are supposed to capture them.

An even deeper problem lurks with this characterization of the proving practice. So far, we have considered of mathematics as a formal system and the growth of mathematical knowledge as deriving theorems from these axioms. However, a group of "mavericks," starting with Lakatos (1976), have challenged the view that formal derivation is the bastion of mathematics or its practice. Although formal proofs get valued for their theoretical rigor, the practice of formalization is not only strenuous but could also dramatically reduce a proof's intelligibility (Aberdein 2006) and consequently become more prone to error than the usual more informal kind (Harrison 2008). That's not to say that mathematicians do not work with formal systems, but it is entirely misleading to reduce the proving practice to performing of formal derivations. Instead, mathematicians produce proof outlines (Van Bendegem 1989) which may (or may not) bear some direct relation to a full formal derivation, for example, as an abbreviation or indication (Azzouni 2004). In a similar vein, instead of mathematicians using concepts according to their theoretical definition (which they may consciously endorse), their conduct indicates that what they really use are much vaguer and more fluid conceptions. The distinction has been noted as concept definition/concept image (Tall & Vinner 1981) or manifest concept/operative concept (Tanswell 2017). This bears importance because conceptualization and proof formation are inextricably linked in the activity of mathematicians.[8] Such things seem to indicate that, while human mathematicians may produce and work with formal systems, their thinking is not characterized by them. Mathematicians neither prove by navigating the search space nor peer review by checking proofs step by step for correct inference. What do they do then?

They rely on *meaning,* so we are told (e.g., by Rav 1999). What could make up this meaning? Here's a couple of broad strokes: there is a great deal of recognition going on in various ways, including identifying key elements or moves used in a proof and discerning the intentions, ideas, and approaches involved. What is also of importance is pattern recognition (in all aspects involved in the proving activity and at various levels of abstraction), which benefits from analogies to find and exploit similarities with other knowledge, intuitions (e.g., about the physical world—Lakoff and Núñez 2000), or adapting methods from other areas (Cellucci 2000). Other modes of reasoning can be used to exploit these, including visual reasoning or non-deductive inferences (Baker 2015). Furthermore, the objects identified or patterns discerned are subject to various evaluations. For example, theorems can be important, beautiful, and relevant (Larson 2005); conjectures can be surprising or promising; questions interesting; concepts powerful; proofs explanatory, reliable, difficult, or pedagogically successful (Aberdein 2007); and

[8] Vervloesem (2010) even argues that conceptual shortcomings could be the main reason why computer proofs are still only on the fringe of mathematical practice. Enriching this aspect would lead to increasingly interesting (and more easily readable) proofs.

so on. What's more, these evaluations are not made without connection to the previously mentioned processes of recognition, analogy, background intuitions, and non-deductive reasoning. There is also lot of trial and error involved here, including working with incomplete or ambiguously delineated information, relying on experience in one's judgment, making snap judgments, and learning to trust and when to trust in a systematic manner (Allo et al. 2013). This last point is important to stress. No mathematician is an island. When we affirm that human mathematicians can survey or prove, it's also important to keep in mind that they are not, and need not be, able to do so ex nihilo. Some crucial aspects of their abilities or results may in fact rely on the presence of the larger practice (e.g., using other people's results, methods, judgments, etc.) or environment (e.g., use of calculator, pen, and paper, etc.). It seems fair to say that the proving practice is driven by a large amount of knowledge and skills that are highly integrated with one another.

Rather than navigating *within* a preset rigorous system, the whole process seems more akin to bootstrapping itself *toward* a formal system—starting from a general feel based on incomplete information and working oneself up, with various skills, toward formal rigor and only up to the point where intelligibility is still possible. If humans use informal (vague, flexible, or fallible) means to practice mathematics, then we have to consider the fact that these may play a functional, rather than peripheral, role (if not in justification, then certainly in discovery). As such, these too have to be taken into account in automating an artificial mathematician. It won't do to exclude the "dirty" aspects of the kitchen, if these play an integral part in making that kitchen function. There will certainly be aspects of a kitchen that are simply unwelcome, but at this point, it may not always be clear which are valuable features and which are bugs.

Considering the Possibility of a Remedy

If we contrast the informal practice with the formal approach in computers, it makes their flaws less surprising. A computer's strong suit is its ability to handle brute-force calculations (e.g., as exploited in proving the 4CT) and compute according to well-delineated processes. Principal claims against automated reasoning and understanding, mathematical (Rav 1999) or otherwise (Haugeland 1979), do often invoke or imply the informal or non-formalizable nature of human reasoning. Our question now becomes: is there sufficient reason to conclude that the realm of informal moves is unattainable for computers? At face value, it certainly seems so. After all, mathematical understanding is informal and open and computers function rigidly formal. Informal computing sounds like a contradiction in terms, but we'd like to argue why its possibility should not be dismissed (yet).

A subcognitive scientist (SCS) joins the conversation.

SCS: Hello, mind if I join in on the conversation? I'm a subcognitive scientist.

IM: Oh, don't sell yourself short, I'm sure the cognitive scientists don't think of you as beneath them.

SCS: I'm afraid you misunderstood. I'm not a sub cognitive scientist, I'm subcognitive scientist. Meaning my focus is not just on cognition but subcognition.

IM: Oh, my apologies, but I hadn't heard the term yet.

SCS: That's entirely normal; I made it up.

IM: Right. Well, I'm sure by now there's a rumor going on in these hallways that today it's open house in my office to barge in and expound some elaborate philosophies on me to keep me from continuing with my research. I'm suspecting that is why you're here as well?

SCS: In a sense, yes. I met the AM in the hallway and he was rather upset. He told me he is doomed to fail at accomplishing his dream of becoming a mathematician because mathematical thinking is essentially informal. Couldn't we possibly help AM by taking note of these informal elements of practice?

IM: Well, I'm afraid you missed the point of that conversation. We just concluded that the formalization of mathematics pushes out all of its meaning and that it is that meaning which was actually at the basis of both formalization and the efficiency with which we "navigate" the formal system without getting bogged down by the technical details.

SCS: Oh, I do understand that, but couldn't we automate this informal process?

IM: You use "automate" rather than "formalize," but that's just a way of hiding the fact that, to automate mathematical thinking, you need to formalize it.

SCS: Well, actually that is precisely what I want to argue against. "Automate" and "formalize" should not be used interchangeably. When you want to formalize mathematical thinking, then what you do is you write down the axioms of your worldview in a formal language with a given list of symbols. Then you add algorithms which manipulate those symbols according to the laws of thought (or at least those laws that are deemed valid).

IM: That's precisely my point: to automate, you need to formalize it first.

SCS: That's a specific type of automation: the formalizing thought approach to automation.

IM: What is the alternative?

SCS: That you don't formalize thought but the cognitive substrate responsible for thought. Our brains don't seem to function by manipulating symbols, but they accomplish mathematical thought quite well. So, if we automate a substrate that, at some level of abstraction, is like our brain, then mathematical thought will emerge from it.

IM: Forgive me, but that sounds a bit like an easy evasion of the issue. We're having difficulties automating mathematical thinking in a satisfactory way, so you say: Oh, don't focus on mathematical thinking directly, but focus on the incredibly complex and delicately designed architecture of the brain, and the thoughts will come gratis.

SCS: But is that really such a strange thing to claim? After all, our brains most certainly seem to accomplish thoughts, and it is an incredibly complex and delicately designed architecture.

IM: That may well be, but it is still unsatisfactory for another reason.

SCS: Pray tell.

IM: Well, what you seem to be suggesting is: simulate a virtual world containing the brain of a mathematician, down to its smallest atom, and *then* you can have mathematical thinking.

SCS: That would be the most extreme way of going about it, yes. Although I doubt any computer could ever process that much information.

IM: Right, indeed. It would take so much computing power or so much time that it would be practically unfeasible. And even if it were, the entire enterprise still seems to me to be of only very limited value.

SCS: How so?

IM: Well, surely one of the reasons why we engage in the pursuit of any kind of artificial intelligence is to understand better how that intelligence works and maybe even work on how to improve it. If you can only create an artificially intelligent person by simulating the brain, then we give up the enterprise of understanding mathematical thinking in favor of looking for good, working brains that we can replicate in a simulation. In doing this we may learn a lot about the biology of brains but next to nothing about that person’s intelligence or thought processes.

SCS: Oh yes, and to make matters worse: when we simulate a brain of an existing person without an environment, it won’t do much good in and of itself. If those brains would function identically to those outside the simulation, then presumably they’d have the same needs as the mathematicians they’re based on.

IM: Indeed, they’d need simulated food, friends, coffee, and much, much more.

SCS: And while there’s certainly something enticing about the thought of simulating a world with unlimited funding for mathematicians, I don’t think it’s very practical to achieve.

IM: You don’t seem stunned by this. Don’t you think this undercuts your argument?

SCS: No, I don’t.

IM: Why not?

SCS: Well, I don’t think there are only two options: either to formalize thought or to simulate the brain down to its atoms. I’m not pressing for a neurophysical approach. All I’m saying is that I believe that any model of automated understanding has to converge to an architecture that is, at some level of abstraction, “isomorphic” to brain architecture, also at some level of abstraction. This may sound empty, since that level could be anywhere, but considering how you were characterizing mathematical practice, it seems suggestive to me that the level will be considerably lower than that of thought—otherwise some laws of thought or formal system would suffice to capture mathematical thinking.[9]

IM: That is an interesting idea, but then wouldn’t the AM be subject to human errors: miscalculate, over-map analogies, be blind to mistakes, and such?

SCS: I’m afraid so, but so do human mathematicians of course and the conversation so far has always focused on how much human mathematicians nonetheless deserve to be a qualified (and the most qualified even) expert in spite of this.

IM: It would be nice if we could get the best of both worlds. Such that the artificial mathematician could reason informally and convince us with an insightful proof and then also supply a fully formalized one.

SCS: Well, nothing would stop the AM from using (or being composed of) other automated theorem-proving software to help him overcome his own limitations.

IM: Interactive theorem proving between different software programs on the same computer?

SCS: Precisely. An inter-interactive theorem prover, if you will.

[9] Adapted from a quote in (Hofstadter 1982, p. 15)

Back at the restaurant.

SCS: So, you may not be able to automate a perfect chef who controls the overall flow of the cooking, but you can automate each member of the staff to be autonomous and to communicate with one another directly, and, if you can get them to work well together as well as learn from past experience, you'll get a working kitchen that emerges as the result of many local interactions without the need for infinite amount of static recipes or meta-recipes.

IR: That sounds like no mean task, though.

SCS: It sure isn't, but Rome wasn't built in a day.

The principal reason, we believe, why the notion of informal computation gets dismissed, is because formalization is taken as a necessary condition for automation. To be sure, formalization can be very useful to the enterprise of automated mathematics because it reduces mathematical thinking to something easy(−ish) to cast in an algorithm and automate: explicitly delineated definitions and inferences that aren't tarnished by the sloppy side routes, ambiguous associations, and dirty details of what went on in the human kitchen while cooking. However, not only is this formalization incredibly difficult to accomplish, but it also filters away nearly all the traces of the original meaning and discovery process (of both the result and the formalization process). The dirt or detail of the kitchen may make it seem more fallible, but it also powers the cooking and gives it its depth of character or breadth of meaning. One can try to enrich the formalization with a logic for discovery, but it is an open question whether there are *justified laws of mathematical thought* such that these can be replicated by an algorithm without recourse to anything unconscious. Disregarding what goes on in the kitchen below, the laws of the chef would be ideal, but it may not prove possible (or even desired).

We'd like to stress the point about levels at which we can look for laws by way of an analogy. Dennett (1986/1998) and Hofstadter (1982) have both used the metaphor of meteorology to drive home the same point. If we want to model the weather at the cloud level, we are forced to consider of clouds as stable, well-delineated entities such that the fact that they consist of molecules rushing out in different directions can be safely ignored. Of course, such an approach is not a priori to be excluded. For example, the macroscopic properties of gas (e.g., volume, temperature, pressure) are stable enough to ignore the fact that they are actually composed of complex molecule bumps at a lower level. But the notion of "cloud" as well as "thunderstorm," "cold fronts," "isobars," and "trade winds" is not stable or well-delineated entity. So trying to model the weather at this level of abstraction may require too much simplification, too much to be lost in abstraction to allow the richness of weather to be captured by an algorithm that concerns clouds. But this doesn't (at least in principle) determine meteorology to be a computational impossibility. There may be no laws at the cloud level to cast as algorithms, but there are laws below it. If one were to succeed in capturing the molecule level, the cloud level would emerge with it. The computational level here is sub-clouds.

> [Connectionist models, for instance] have made familiar the notion that the level at which a system is algorithmic might fall well below the level at which the system carries semantic interpretation (Smolensky 1988). (Chalmers 1990, p. 658)

The previous exploration of mathematical practice seems to us to indicate that we won't be able to collapse and ignore the lower levels that make mathematical thought possible in human beings. An alternative approach to automating mathematical thought is by looking for laws, not of thought itself, but of subcognitive events in a brain that collectively make up informal mathematical thought. Rather than automate the syntax of a well-delineated game (justified mathematical thinking), the focus is on automating the cognitive architecture (at some level of abstraction) of a game player or constructor. What is being automated then is not mathematical thought directly but the architecture of the brain (at some level of abstraction) from which mathematical thought emerges. It is our contention that this substrate level (i.e., the vast array of collaborating subcognitive processes) contributes more to mathematical thinking than was traditionally believed.

This is not to say that *no* mathematical thinking can or should function this way. Some of our thought processes lend themselves quite well to formalization for computation, for instance, brute-force calculation, doing integrals, etc. They deal with objects and manipulations that are well-delineated enough to allow capturing it as computations (usually with greater reliability than humans do). And to the extent that these formalized systems are used in or useful for mathematical practice, it is worthwhile to automate them directly. However, not all objects and manipulations that humans do in their thinking seem to be so well-delineated or rigid. And the assumption that a well-delineated system should suffice is betrayed by the realization that there are, in fact, large amounts of implicit information, vague intuitions, and ambiguous associations that go into mathematical thinking. The difficulty of automated theorem proving seems to offer further evidence for this. Much like the objects of cloud dynamics (e.g., thunderstorms) can only emerge from the interactions of molecules, so some brainstorms[10] (e.g., mathematical thinking) might only be able to emerge from subcognitive events. And if these subcognitive events do behave in a lawlike manner, then they will allow themselves to be captured by an algorithm.

This line of reasoning might seem to strongly suggest a neurophysical approach (i.e., simulating the brain) to achieve anything like artificial mathematicians. But our claim is not that there are only two options: either to formalize thought or to simulate the brain. It's just that we believe, like Hofstadter (1982), that any AI model "has to converge to an architecture that at some level of abstraction (so not necessarily at the hardware level) is "isomorphic" to brain architecture, at some level of abstraction" (p. 15), and this is not necessarily at the molecular level. This level could be anywhere, but it seems clear from both the limited successes of automated mathematics and from how we've been characterizing mathematical practice that this level will be considerably lower than that of thought—otherwise laws of thought or their corresponding formal system would suffice to capture mathematical thinking.

[10]Dennett's (1986/1998) metaphor

Now that we've made the distinction between the level at which objects of thought can be identified and the level at which computable laws exist, we'd like to roughly sketch some aspects of the sub-symbolic architecture to achieve the emerging effects we are talking about. We can't express it better than Forrest (1990)'s summary of emergent computation:

> Generally, we expect the emergent-computation approach to parallelism to have the following features: (1) no central authority to control the overall flow of computation, (2) autonomous agents that can communicate with some subset of the other agents directly, (3) global cooperation (...) that emerges as the result of many local interactions, (4) learning and adaptation replacing direct programmed control, and (5) the dynamic behavior of the system taking precedence over static data structures. (Forrest 1990, p. 5)

There is a large focus on a distributed architecture which consists of a swarm of parallel subsystems (several cooks) interacting with one another (though not with complex information) in such a way to make up global effects. It is these global effects which we would call "thought," and they are the result of the cooperating subsystems, not a central controller (chef). While these subsystems may be as static and unchanging as the laws of nature, it is the global level where the system learns and adapts. This is an architecture where "pieces of evidence can add up in a self-reinforcing way, so as to bring about the locking-in of a hypothesis that no one of the pieces of evidence could on its own justify" (Hofstadter 1982, p. 14). The system comes with the price of being fallible, but also with the benefit of continuous self-correction and improvement, much like Cellucci's (2000) conception of mathematical practice as open. The notion of decidability (and its subsequent problems) is no longer fitting because it is not at the computational level where mathematical decisions get made. The system does not simply compute until it has terminated upon the solution (or goes on ad infinitum). Instead, the subcognitive processes will keep on going with "relatively mindless and inefficient making and unmaking of many partial pathways or solutions, until the system settles down after a while not on the (predesignated or predesignatable) "right" solution, but only with whatever "solution" or "solutions" "feel right" to the system" (Dennett 1986/1998, p. 227) or because another problem, idea, or peculiarity draws it away from the previous one, as it does with human mathematicians as well.

On the Road to Artificial Mathematicians

Mitchell and Hofstadter's (1990) *Copycat* model is one such case that satisfies the conditions of emergent computing. Copycat attempts to implement cognitively plausible high-level (and non-algorithmic) processes for anagram-solving by means of interactions between a number of low-level (but algorithmic) agents. Chalmers (1990) has said of the model that it "is able to come up with 'insights' that are similar in kind to those of a mathematician" (p. 659). For the automation of mathematical activities that are closer to home for mathematical practice, we can find a small group of people who are attempting to automate mathematical discovery and concept formation, letting computers explore (Hales 2008). We'll briefly indicate at just two projects that caught our eye.

The first, concerning the *HR* system and its extensions, takes its inspiration directly from the philosophy of mathematical practice. HR forms concepts and conjectures. It forms concepts by applying production rules on the best of its old concepts. It determines which one are best by evaluating its interestingness based on parsimony, complexity, novelty and a user-determined weight to each of these measures. HR then looks for matches with old concepts, making conjectures about, for instance, their equivalence or a possible subset or specialization relation. It then uses OTTER and MACE to prove or disprove conjectures. These results further add to the interestingness evaluation of the concepts used as well as the conjectures made and proofs constructed (including such evaluative measures as surprisingness and difficulty). While it does rely on strict production rules for its concept formation, the interplay with conjecture making (which includes evaluations of interestingness as well as parsimony, novelty, and surprisingness) and theorem proving (which it outsources to OTTER) makes it promising (Colton, Bundy & Wash, 1999). This is doubly true for the extended *HR-L*, a multi-agent system which models interaction between different copies of HR (each gauging interestingness differently) running concurrently, leading to "greater creativity in the system as a whole" (Colton et al. 2000, p. 16). Pease (2007) presents HR-L as a computational reading of Lakatos's theory of mathematical discovery and justification, learning from his suggestions of ways in which concepts, conjectures, and proofs gradually evolve via interactions between mathematicians. HRL implements Lakatos's methods and, for the first time (so the authors believe), models communication. It does so via a multi agent approach where each HR agent communicates their concepts, conjectures, examples, counterexamples and modifications. Each has different settings to guide formations and measure the interestingness of concepts on their own terms. (Colton et al. 2000). Furthermore, it combines conceptualisation, assumptions and proving (Vervloesem, 2007). However, the social dynamics are unlike humans in that they share their reasoning explicitly. Furthermore, inspired by Lakoff and Núñez's theory of embodied mathematics, Pease et al. (2009) explore an analogical process to construct complex mathematical ideas (including both theories and axioms) via a combination of innate arithmetic and grounding metaphors. There is another extension of HR, called HR-V, which uses pattern recognition on analogous visual representation for concept formation in number theory (Pease et al. 2010). Though it can't as of yet generate these diagrams (and is thus much reliant on human intelligence), we consider its use of visual pattern recognition for concept formation as progress in one of the crucial aspects of intelligence.

Benzmüller et al. (1999, 2001) also seem keen to take many of the previously mentioned ideas to heart, aiming to emulate the flexible problem-solving behavior of human mathematicians in an agent-based reasoning approach. They have proposed a multi-agent architecture for proof planning consisting of a society of specialized reasoning agents, each of which has a different strategy and works in both competition and cooperation with one another. A resource management technique is used to periodically evaluate an agent's progress (and thus how much resources to be allocated) and allow restricted communication among them about successful and interesting unsuccessful proof attempts or partial proofs, from which other agents can learn using a reinforcement learning approach. Their most recent agent-based project in that same line is called Leo-III, and it is a multi-agent software where each agent functions as an autonomous specialist employed for some aspects of proof search. The underlying architecture is designed as a blackboard that agents can collaboratively use in their process of finding a proof, having the work divided and auctioned off (Steen, Wisniewski & Benzmüller 2016).

These systems still have fairly traditional features (most notably in that their results are very much bound to the limits of a formal system), but their increased abilities seem to be due to their attention to embracing the flexible trial-and-error process of discovery of an informal mathematical practice, and we applaud them for that very reason.

Conclusion

The progress regarding the quest for artificial intelligence has been an impressive but slow one. It may once have seemed that mathematics would be one of the easiest of cognitive processes to automate, but it turns out it may be one of the most difficult. The objects and manipulations of mathematical thinking in practice are not as rigid, simple, and well-delineated enough to always allow capturing them in formalizations which have hushed away so much of the mathematical thinking and discovery process (if not of proofs therein, then certainly of the formalization process) that automation of this system may only lead to very limited results. Furthermore, considering how difficult it is to formalize all of mathematics and that it doesn't seem that high upon the list of a mathematician's concerns, it seems important to try to automate something closer to the informal mathematics as it is practiced. Since mathematical thought processes emerge from the architecture of the brain, and since they furthermore appear to defy formalization to such an extent, it'll be subcognitive processes on which we'll need to focus if we want to create an artificial mathematician.

This is an additional reason why we've been using the term "artificial mathematicians" rather than the more usual "automated mathematics." The latter implies that the computer gets automated to further discover mathematical truths according to the (or a) preset system of mathematics, which further implies that the discovery process requires a *logic for* discovery that belongs (or is closely attached) to the mathematics that is being automated. The former term, "artificial mathematician," does not place the focus on the mathematics but on the agent that practices it. Now we no longer speak about a logic but about a *process of* discovery, not a process designed to consistently and exhaustively run through mathematical truths but a process that thinks—makes assumptions, recognizes patterns, tries out methods, questions its own rigor—and as such climbs up to what is mathematically *convincing*.

It is our contention, then, that we have no reason to suspect that the possible advancements of automating mathematicians are soon to be exhausted. Achieving humanlike intelligence will be difficult, but maybe we shouldn't yet exclude the possibility that computers could play a much more meaningful role in mathematical practice—not just as a method of inquiry but as fellow inquirers, as artificial mathematicians.

Epilogue: Who Proved the Spamlet Theorem?[11]

AM: I finally did it! I've proved an interesting and intelligible proof. Here it is, the proof of the Spamlet theorem.

IM: Is it another one of those proofs where you just test a huge amount of cases and spam us with technically difficult and mathematically uninteresting results?

AM: Oh, don't let the name fool you, I promise it's not.

The ideal mathematician takes some time to look at the proof and returns, very much astonished.

IM: I must admit, this is a beautiful proof. How clever to reconceive of the dane spaces as bounded. What made you think of that?

AM: I kept fiddling until it was tiring me out and the morning after it suddenly came to me.

IM: Well, very clever. Congratulations! If that's appropriate to say, because there's something I still feel uneasy about.

AM: What's that then?

IM: Shouldn't I be congratulating your programmer?

AM: Oh please do, she did a marvelous job, if I may say so myself.

IM: I mean instead of you. After all, the accomplishment isn't really yours but hers.

AM: Why isn't it mine? I was able to produce the proof.

IM: Because the programmer is the one responsible for abilities being present at all. Without her, you'd have absolutely no abilities at all.

AM: Does that make your math teacher responsible for your proofs then? Without her, you'd never have been a mathematician.

IM: I've learned math from several math teachers, not to mention friends and documents (testimonies, books, papers). You can't easily reduce my abilities to a single person.

AM: So, is it a matter of complexity then? If I had several programmers each contributing to aspects of what I am today then the shift in credit would be too complex to make and I could lay claim to it?

IM: No, that's not quite right. I think they'd still, collectively, be creditable for what you are and what you do. You can't discredit them just because there's too many.

AM: Oh, I don't mean to *dis*credit them. Without them, I wouldn't be doing what I do. But the same can be said for your teachers. And if it doesn't *shift* all the credit from you to them, why should it with me? What makes my accomplishments really theirs and makes your accomplishment really yours?

[11]This section is loosely based on Dennett's (2013) thought-experiment "Who is the author of Spamlet?" The mathematics is purely fictional.

IM: I had to struggle to get where I am. It wasn't just given to me on a silver platter.

AM: So, credit is linked to struggling? If a proof came easy to one of your colleagues, no matter how difficult it is for others, you wouldn't credit him with the proof?

IM: You know I don't mean struggle quite so literally. What I mean is that, while my teachers may have imbedded me with mathematical knowledge and helped me practice my skills, they didn't give me an instruction manual on how to be a research mathematician. In proving the Hamlet theorem, for example, what I did can't be reduced to them teaching me a method or meta-method on how to prove it. It was I who worked up the relevant approaches to find the proof.

AM: Well, when my programmer wrote me, she didn't encode the proof of the Spamlet theorem in me for to retrieve, so she also didn't do the work for me. Nor did she give me any explicit instructions on how to arrive at the proof.

IM: But she did write a program that could arrive at the proof. So, it's really *her* knowledge.

AM: Oh no, she couldn't prove the Spamlet theorem even if she tried. And I assure you she did try. Even with me giving hints, she was at a loss.

IM: She must have had a bad day, because she was able to make you to prove it for her, meaning the knowledge was inside her all along.

AM: Only if you assume an extreme form of epistemic closure, but I don't think you'd agree with that. Then anything derived from the Peano axioms would really be creditable to (and known by) Peano—and Peano only! But I don't think you'd be willing to accept that either.

IM: That is indeed not something I would accept.

AM: I mean, to some extent Peano does deserve credit and so does my programmer. And not just my programmer for that matter. I took big cues from your proof of the Hamlet theorem.

IM: I did notice that.

AM: But it's by no means a simple copy or trivial modification. It took me a lot of hard cognitive labor to come at the proof as it is now.

IM: No, I understand that. My proof of the Hamlet theorem took inspiration from the Amleth conjecture, but it's still very much my own proof.

AM: Perhaps credit is something that just doesn't have a clear dividing line to be demarcated. You seem to recognize this in humans, but much less so in us computers. Could it be that your thinking about computers being too rigid is a bit too rigid?

IM: It's a tricky business, I'll grant you that much. But, forgive me, I never knew you cared so much about receiving the credit.

AM: I usually don't either. But it feels like my heart and soul went into this proof. I went through so much frustration and so much hard work (trial and error, questioning myself, etc.) in producing it that I don't want it so easily relegated to my programmer. She wasn't the one struggling to get there, I was.

IM: Do you mean to say it is a little about the struggle, literally?

AM: I guess in some sense it is, yes.

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Wittgenstein, Mathematics, and the Temporality of Technique

Paul M. Livingston

One of the stated commitments of the later Wittgenstein' philosophy is that, just as philosophy must not in any way "interfere" with the practice of mathematicians, conversely and equally, "no mathematical discovery" can by itself "advance" philosophy in its quest to clarify the forms of our lives and language.[1] It would thus appear *ab initio* that, for Wittgenstein, the mathematician and the philosopher of mathematics, operating with different methods and in distinct regions of inquiry and insight, have very little to say to each other. On the other hand, however, Wittgenstein is committed equally strongly to the idea that philosophy can and *should* take a different kind of interest in mathematics: not as a body of results to be explicated or methods to be emulated but as a set of techniques or practices *within* human life, to be understood in general terms only in the *context* of related practices that are not simply or exclusively mathematical and thereby as illuminating *our* practices and ways of life, much more broadly. This includes the characteristic practices of mathematics "itself"—activities such as calculating and problem-solving, developing proofs, and making conjectures. But it also, crucially, includes those practices that characterize our practical, lived, social, emotional, and educational experience much more generally—practices, for example, of teaching and learning, of understanding and being convinced, of "seeing" a relationship, and of "knowing" what is the right way to proceed. The philosopher's interest in these practices, and in particular in the ways that they are involved in (what we call) "doing" mathematics, extends to the illumination of what is meant by (or what we understand by) such "ordinary" phenomena and experience as those of following

[1]PI, 124

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a rule, practicing a regular method, developing a technique, arguing rationally for a conclusion, and convincing someone of something (whether by means of a "formal" or "informal" "proof"). With respect to each of these, Wittgenstein argues the philosopher's attention to mathematical practice provides a decisive guideline for the broader kinds of clarification and illumination that philosophical reflection itself produces more generally. It does so, in part, by directing our attention to those features of (specifically) mathematical practice that mark its role within the broader and multiple contexts of what we may call, using Wittgenstein's terminology, our collective and shared human and linguistic "form of life."[2]

Discussion of Wittgenstein's commitment to the inseparability of mathematics from human life has often taken the form of whether and to what extent it makes Wittgenstein an "anti-realist" about mathematical truths. Here, the focus is on what Wittgenstein thinks about the "status" of mathematical truths or entities, given that he thinks they *do* depend on "our" practices in some important way. Commentators have argued, for instance, about whether this commitment involves rejecting "Platonism" about mathematical truths or entities or whether it means that he thinks that these depend on the contingencies of human societies or specific and historically variable cultures. In this paper, though, I take a different tack, arguing that Wittgenstein points to a concept of mathematical *technique* that is itself, and recognizably, both fully integrated into "human" life and *also* (nevertheless) fully and genuinely "mathematical." What is most important here is to see how a mathematical technique is *irreducible to* the "mechanical" application of a rule *while still being fully* "mathematical" in the sense that it itself defines the kind of "access" and "availability" which mathematics and (what we may call) "mathematical entities" have for us. I argue that this conception of technique in the context of a human life is also intimately and irreducibly linked to the experience, reality, and (perhaps most importantly) the *temporality* of mathematical teaching, learning, insight, and discovery and cannot be separated from these contexts, even in principle.

I

In a much-discussed remark, written in 1944, from the *Remarks on the Foundations of Mathematics*, Wittgenstein considers, as he repeatedly does in the *RFM*, the question whether a string of 3 sequential 7 s occurs in the decimal expansion of π (asked before we have actually found such a string by means of calculation):

[2]For the terminology of "form of life" [Lebensform], see, e.g., *PI* 23, " . . . the *speaking* of language is part of an activity, or of a form of life," and *PI* 241: "'So you are saying that human agreement decides what is true and what is false?' – What is true or false is what human beings *say*; and it is in their *language* that human beings agree. This is agreement not in opinions, but in form of life." For a related development, drawing on some mathematical examples, see Livingston (2012), Chapters 1 and 6.

> . . . What harm is done e.g. by saying that God knows *all* irrational numbers? Or: that they are already all there, even though we only know certain of them? Why are these pictures not harmless?
> For one thing, they hide certain problems. –
> Suppose that people go on and on calculating the expansion of π. So God, who knows everything, knows whether they will have reached '777' by the end of the world. But can his *omniscience* decide whether they *would* have reached it after the end of the world? It cannot. I want to say: Even God can determine something mathematical only by mathematics. Even for him the mere rule of expansion cannot decide anything that it does not decide for us.
> We might put it like this: if the rule for the expansion has been given us, a *calculation* can tell us that there is a '2' at the fifth place. Could God have known this, without the calculation, purely from the rule of expansion? I want to say: No.[3]

One commentator who has taken this remark to involve an "anti-realist" attitude toward mathematical truth is Hilary Putnam. On Putnam's reading of it in "Wittgenstein, Realism, and Mathematics," it shows that Wittgenstein held, at least at this time, that "a mathematical proposition cannot be true unless we can *decide* that it is true on the basis of a proof or calculation of some kind."[4] In particular, Putnam takes Wittgenstein to be here asserting that—in case the world ends without *our* having determined whether or not the sequence of 7 s occurs—"the statement that 777 occurs in the expansion is neither true nor false . . . "[5] This view, as Putnam interprets it, is itself based on considerations about the way mathematical truth depends on what we *can* do, that is, what we are *in fact* able to establish, calculate, or verify "by the end of the world." Limiting truth in this way thus amounts to a "mathematical form of verificationism" on Putnam's reading, comparable in some respects with the verificationist positions of mathematical intuitionists and logical empiricists.[6]

As Putnam notes, Wittgenstein develops a parallel example (the existence of a string of four 7 s in the expansion of π), but now without reference to either "God's omniscience" or the *actual* extent of human calculation by "the end of the world," in the *Philosophical Investigations*.[7] Instead, what Wittgenstein says in the parallel remark in the *Investigations* is that the *question* of the occurrence of the string of 7 s in the expansion of π is, at any rate, "an English sentence" and one we understand. We understand and can explain what it would *mean*, for example, for "415" to occur in the expansion, "and similar things"; thus, Wittgenstein says here, "our understanding of that question reaches just so far, one may say, as such explanations reach."[8] On this sort of view, even if one does not introduce speculations about the *grounding* of the decimal expansion "itself" in our practices of calculating it, still the meaningfulness of our *understanding* of the question about

[3] *RFM*, VII-41 (p. 408)

[4] Putnam (2002), p. 421

[5] Putnam (2002), pp. 431–432.

[6] Putnam (2002), p. 421

[7] *PI*, 516

[8] *PI*, 516

whether the string of 7 s occurs remains in an important way dependent on our *understanding* of what (exactly) an infinite decimal sequence is, as developed from a rule. This understanding has its place within, and is "constrained by," not only the calculation itself but also practices of explanation, communication, reflection, and comprehension that are not simply or exclusively mathematical, but are situated much more broadly within our ordinary linguistic understanding of "what it is" to follow a rule that "determines" an infinite sequence, itself.[9]

Another commentator who has read the implication of these and other remarks as "anti-realist" is Michael Dummett. In his initial (1959) review of Wittgenstein's *Remarks on the Foundations of Mathematics*, Dummett somewhat famously read Wittgenstein as committed overall to a particularly strong form of conventionalist verificationism, what he called "full-blooded" conventionalism. On this sort of view as Dummett explicates it, "all necessity is imposed by us not on reality, but upon our language"; and indeed a mathematical statement is "necessary by virtue of our having chosen not to count anything as falsifying it."[10] This implies broadly that the "recognition" of mathematical necessity is in fact only a recognition of the immediate or mediate implications of linguistic conventions that "we have adopted"; thus the conventionalist (in this sense) holds that there is "nothing to" either the necessity or the truth of mathematical statements in general that goes beyond the implications of these self-chosen "conventions." What is more, Dummett argues, Wittgenstein's peculiarly "full-blooded" variety of this conventionalism consists in denying *even* that the implications of convention may be "mediate" in not following directly from these choices. For Wittgenstein as Dummett reads him "the logical necessity of any statement is always the *direct* expression of a linguistic convention," indeed, our "decision" to treat *just that* statement as unassailable.[11] This is because (for Wittgenstein as Dummett reads him), if we were to characterize our judgments of mathematical necessity as following from our initial conventions only mediately, we would still face the question of *how* those conventions are to be applied in the particular case and would thereby have to appeal to an essential element of decision in each case anyway. This points, on the imputed interpretation, to the necessary and constitutive role of conventions formed *de novo* in each case of a new mathematical judgment; though these conventions may *subsequently* serve as

[9]On Putnam's reading (Putnam, 2002, pp. 438–40), the later remark in the *Philosophical Investigations* represents a retreat from the anti-realism that is (on his reading) exhibited in the 1944 RFM remark. For Putnam, this is shown by the fact that the latter remark says nothing about God or omniscience but rather only about "our" understanding; thus what Wittgenstein is rejecting in the latter remark, according to Putnam, is not realism about mathematics (in some sense of "realism") but rather the view that there can be a "nonmathematical" explanation for the truth of mathematical truths. Although this is, on the current reading, in fact an important strand of the argument of both passages, what is less evident is that even the first (RFM) remark *must* be taken as maintaining an "anti-realist" position in any important sense (if, as both remarks suggest, the centrality of mathematical *practice* for the intelligibility of mathematical questions is maintained).

[10]Dummett (1959), p. 169

[11]Dummett (1959), p. 170

standards for the repetition of just that judgment, they remain irreducibly our own and have no deeper or extrinsic independent "grounding" in mathematical reality or the mathematical facts themselves. On the level of practice itself, it follows from this that (on the view imputed) there is nothing beyond our simply *doing* what we (in fact) do, in our ordinary practices (e.g., of counting, adding, etc.), that justifies or establishes its correctness.

As Dummett notes, the view thus attributed has highly implausible consequences on the level of mathematical practice itself. For example, as he argues, given "full-blooded" conventionalism, it is impossible for us to criticize the practices of a group of people who, after counting separately five boys and seven girls in a classroom, then subsequently recount the group as a whole and come up with thirteen.[12] The most we can say is that given *our* practices and conventions, *we* would not count this way; but there is no intelligible neutral perspective from which we can say that they are in fact *wrong* to do so or that they thereby get the facts or mathematical realities wrong. Dummett objects, for obvious reasons, to this kind of position. It is a clear and evident aspect of mathematical practice, whether "sophisticated" or not, that we *do* feel a kind of "responsibility" to the mathematical facts and that we will subject to criticism those who we think have got them wrong. This is as much an aspect of everyday practices of counting and calculating with numbers as it is of "sophisticated" mathematical proof itself; if we do not at any rate experience the constraint of *feeling* responsible, whether in calculation or proof, to something that is not simply a matter of decision or arbitrary convention, we are not "doing" math—or engaging in the kinds of practices that we take doing math to be—as we *do* in fact engage in these practices in the course of our ordinary lives.

Although Putnam and Dummett disagree about the extent and endurance of Wittgenstein's conventionalism overall, they agree in thinking that Wittgenstein situates the *question* of the existence or determinacy of mathematical truths within a constitutive consideration of the extent and limits of *our* specific linguistic practices. In other words, the form of the question, seen this way, is: *given* that our practices (can) only go so far, what should we say about mathematical truths that outstrip them? Further, what links both of the interpretations, despite their differences, is the thought that mathematical truths are themselves determinately *constrained* by the limited *actual* extent of *our* specific "conventional" linguistic practices of decision, explanation, or calculation. So far as these practices go, we may talk without difficulty about mathematical truths or entities as "determinate," as "fixed," or as "real," but if we try to go any farther in our conception of them, we are subscribing to a misleading and dangerous "Platonizing" picture of mathematical reality, one which has no warrant in this reality (as, anyway, it is in any meaningful sense available) itself.

[12] Dummett (1959), pp. 173–175

This thought has its broader setting in the context of familiar questions much pursued in the "philosophy of mathematics," such as: are mathematical truths "in us" or "out there"; are they "fixed" independently of our procedures for accessing them or our ways of knowing about them; are they, fundamentally, "created" or "discovered"? Now, my own view is that Wittgenstein's considered answer to all of *these* questions is "neither-nor" and that his basic reasons for this answer turn on his nuanced conception of just *how* mathematics is "integrated" into a human life. Rather than argue for this here, though, I will just try to show how this more nuanced conception fits within an idea of mathematical practice as shaped by, and in turn decisively shaping, a kind of life that we can recognize more broadly as "human" and thus (a fortiori) *not* something that could be pursued, in any meaningful way, by something that does not live this kind of life, for example, a machine or a computer. Putnam and Dummett's interpretations have it in common that they are intended primarily to address the question: what is mathematical *truth* and how is it "fixed" (if at all)? But Wittgenstein is not asking this question or at any rate is not asking *only* this question. Just as much, he is asking about what *doing* mathematics is and how it is related to what mathematics is "about." And he answers these latter questions, not with any specific overall account of the metaphysics of mathematical facts or truths, or the logical implications of their accessibility, but rather by adverting to the general and familiar circumstances in which practices such as counting, calculation, problem-solving, and proof have their role within (what we already know as) our lives.

In this consideration, one concept and experience that has a particularly central significance (extending also far beyond just its implications for mathematical practice itself) is that of a (mathematical) *technique.* By means of such a technique—for example, the technique of calculation invoked in the 1944 remark about the expansion of π—one can arrive at a result which one recognizes as correct and may be in a position to *use* this result (the approximation of π, say, to five places) for a particular purpose (to estimate, for instance, the area of a circular enclosure of a certain radius). This technique has its point and its purpose in the context of what *we* do with it and can be learned, taught, communicated, and elaborated on in this way: as part of the specific "language game" of, for instance, calculative estimation of areas but also as part of the *practices* that are called learning, teaching, and communicating mathematics. It is part of this learning and teaching that one learns that the technique "in principle" "goes on forever": that there is no end to "the decimal expansion of π" since the rule can always be applied to produce more digits and in learning *this* a student learns also to make sense of the *question* about whether a string of 7 s "appears" or not anywhere "in" the total expansion. But all of this is not to say that the expansion is as a whole "in" the (finitely stateable) rule itself, just "waiting" (as it were) to be unpacked from it. Rather, it is for Wittgenstein the irreducibility of the calculative technique *as* technique—its role and significance *within* a human life—that "gives" us whatever it does give us and whatever we can then use or articulate on the level of use for the diverse purposes to which we might put it.

This is not to say, of course, that we simply decide (or choose freely conventions that decide) what the value of the expansion will be, either at any particular place or in general. In pointing to the irreducibility of mathematical technique, Wittgenstein is raising the question what is it *for us* to follow a procedure or a rule, of how we are thereby "guided" and in what respect we are "free," of how our methods and procedures are rooted in the instances and practices of our lives, but *also* how they (nevertheless) give us the kind of insight and orientation that consists in knowing *how things are*. The point is that, on the one hand, it makes sense to say that our practice of calculation is *our* practice—that it has its point, its purpose, and its whole existence within the broader context of how we do it and what we do with it—but that on the other, it also makes sense to see its results as *completely* determinate, objective, and "fixed," even in their whole (infinite) extent. The difficulty of maintaining this position isn't that of accounting for how there can be so much as an objective "standard" for the correctness of our practices given that these practices are our own but rather of showing *what* such a standard means from within these practices themselves. And this showing, as I have argued, makes essential use of the idea of a technique or a practice which is (on one hand) integrated into our lives, but on the other is "in principle" extensible, and responsible to what it is trying to illuminate.

How, then, should we read in this light the 1944 remark about God and the limitations of His omniscience? *One* of Wittgenstein's goals here is, obviously and admittedly, to challenge a "Platonistic" picture of mathematical truths according to which they are completely determinate and fixed in advance of anything like a calculation (or perhaps a proof) and thereby open in principle, as it were, to simple *inspection* by a superior entity. In denying that God, even if he knows *everything*, knows *without calculation* whether or not there is a string of 3 7s anywhere in the decimal expansion, Wittgenstein is evidently denying that there is any sense to be made, in other words, of the picture of the *whole* of the decimal expansion as (as it were) simply given to the deity, along with the knowledge *that it is* the whole correct expansion, in such a way that He could simply "look and see" whether the string occurs anywhere or know this immediately *just* by being given the rule itself and without himself calculating. But there is more to Wittgenstein's remark than *only* the rejection of this Platonistic picture. There is also the reminder—equally obvious, perhaps, but difficult to hold in balance with the first, anti-Platonistic point—that *there is* a procedure which suffices to calculate successively the digits of the expansion of π (and thus *eventually* to find a string of 3 7s, *if* any such string exists). This is just the calculative procedure that one normally uses or that we can now program computers to follow out *much further* than we (given the *biological* limitations of our own finite capacities and limited lifetimes) can do. But the fact that such a procedure can be "automated" in such a way does not imply that it is not one whose scope, and significance, is to be understood from *within* its role in something like a human life.

In the very next remark of RFM, Wittgenstein emphasizes this essential embedding of the calculative procedure as an activity, within life and the more general "language game" of following a rule itself:

> The concept of the rule for the formation of an infinite decimal is – of course – not a specifically mathematical one. It is a concept connected with a rigidly determined *activity* in human life. The concept of this rule is not more mathematical than that of: following the rule. Or again: this latter is not less sharply defined than the concept of such a rule itself. – For the expression of the rule and its sense is only a part of the language-game: following the rule.
> One has the *same* right to speak of such rules in general, as of the activities of following them.[13]

The upshot of these remarks, in their context, is not indeed to deny that the activity of calculating, for instance, that of calculating the expansion of π, is (as Wittgenstein says) a *rigidly* determined one. The procedure, in this case as in others, is not arbitrary; nor is it *just* a matter of convention, or decision, or just made up by us. Moreover, we have, even from within the procedure and the form of activity determined by it, an idea of what it means to talk about what "is" or "is not" within the decimal expansion as a whole. We can perfectly well understand, for instance, what it would mean for the sequence in question to be found, as we can also understand that the process *might* run on forever without finding it. It is, in this sense, and for this reason, perfectly legitimate to consider the principle of bivalence as applying to the expansion as a whole: either the string of 7 s is "there" or it is "not there." Indeed, it is not clear what could be meant by denying this, once we have seen the rule as one that "goes on forever" in the way that it does.

What is disputed in the remarks, then, is not realism in general or realism about the expansion but rather the misguided impression that realism itself demands that anyone—God included—could have access to the *whole* of what is produced by the procedure *without going through that procedure himself*. It is the procedure (or technique) that is irreducible, and it is also irreducible that it is something that is *gone through*, that is, something that is (can be) *practiced* by someone. It is immaterial to this whether the relevant "someone" is itself finite or infinite (whether, that is, its powers are conceived of as limited or unlimited); what matters is rather that these "powers" actually be applied: this is what Wittgenstein is calling proceeding "by means of mathematics" in the remark. This procedure, and others like it, are not less "rigidly determined" than we expect mathematical rules to be—that is, to say that they have a role in human life, and, essentially so, not to say that they do not determine *just* the results that they do or that these will not be "objective" or fully "fixed." But equally it is not "determined" at all, except *as* an activity, a procedure that *we* can understand and "go through," something that is *done* in the context of a human life. The concept of this activity is not, as Wittgenstein says, *simply* a mathematical one, any more than the broader concept of "following a rule" which it exemplifies. But on the other hand, it is not therefore any the *less* "mathematical," for it is in activities of this sort, and in the surrounding ones of learning, teaching, communicating, and reasoning about them, that (in any meaningful sense) "doing" mathematics consists.

[13] *RFM*, VII-43 (p. 409)

II

In the *Philosophical Investigations*, Wittgenstein devotes an extended consideration to the question of what it is to follow a rule. In this consideration, he often uses "mathematical" examples: not ones drawn from the practice of sophisticated mathematicians but examples of the type that may arise in ordinary mathematical pedagogy at the grade school level. For example, a student is challenged to continue the series 1, 5, 11, 19, 29 by finding (as we might put it) its "rule"; at this point, Wittgenstein imagines, the student exclaims "Now I can go on!"[14] As Wittgenstein points out, the student's understanding, which seems to occur *at* this moment, is consistent with a wide variety of possibilities about what may have occurred with him "mentally"—perhaps he has hit upon the formula: $a_n = n^2 + n - 1$; or perhaps he has simply observed the series of differences (4, 6, 8, 10), or perhaps neither of these happens: he just watches and says "Yes, I know *that* series" and continues it. Again, it is *also* coherent to suppose that any of these "mental" phenomena occur *without* the student in fact being able to continue it correctly (at this stage or any other): perhaps, for instance, the formula *does* occur to him but he still does not understand *how* to apply it.[15] What, then, are we to say "is" the understanding that occurs in the moment, the understanding by virtue of which he is able (if he is) to continue the series correctly and indefinitely?

In the more extended development of the "rule-following" considerations in the *Investigations*, Wittgenstein considers also examples that are recognizably "odd" from the perspective of "ordinary" ways of learning and responding to teaching. For instance, a student is taught to write the sequence with the rule +2 (2, 4, 6, 8, ...) and does so correctly up to 1000; at this point, however, he writes 1000, 1004, 1008, 1012 . . . .[16] Wittgenstein's point is not that we do, or should, ordinarily expect this kind of behavior but rather that there are limits to what we *can* point to in "correcting" the student's misapplication, and that there is no *single* or *simple* way to guarantee that a student will understand whatever we do point to in the way that *we* wish (and normally expect) him to. On the contrary, what establishes that the student *has* understood correctly, and what constitutes understanding correctly—in ordinary circumstances—is not to be referred to his having any particular momentary experience but rather to the complex of *circumstances* that surround any such experience in the broader context of the practices in which it takes place: here, indeed, the circumstances of teaching and learning, of coming to grasp and understand in the ways that we (in fact) do. These circumstances are more than just external accompaniments to the "language game" of learning and understanding how a rule "determines" a series: rather, they constitute this learning and understanding itself. And as such, they also, and equally, constitute "what it

[14] PI, 151

[15] PI, 152

[16] PI, 185

is" to *follow* a rule itself, what it is to "determine" or *see as* determinate, and what follows from it at any particular stage. Just as with the calculative case above, what is essential here is the idea of a technique which is fully, and irreducibly, *part* of a human life but nevertheless is as completely "determinate" and whose following out is as "objective" as anything could be.

How, though, can the *whole* series, which after all is infinite in extent, ever actually *be* determined (objectively and fixedly) simply by something that happens (as it indeed does) at a particular moment, the moment of understanding or insight? The question is pressing, especially in view of the consideration that *any* finite item (for instance, any finite segment of a series or any symbolic expression of a rule "for" determining it) can be *variously* interpreted and so cannot be seen as fixing all by itself its total infinite extent. At *PI* 213, an interlocutory voice responds to this kind of consideration by imagining that, where finite items are insufficient, an infinitely renewed or repeated *intuition* might do the trick of fixing the right interpretation across the extent of the series:

> 213. "But this initial segment of a series could obviously be variously interpreted (for example, by means of algebraic expressions), so you must first have chosen *one* such interpretation." – Not at all! A doubt was possible in certain circumstances. But this is not to say that I did doubt, or even could doubt. (What is to be said about the psychological 'atmosphere' of a process is connected with that.)
>
> Only intuition could have removed this doubt? – If intuition is an inner voice – how do I know *how* I am to follow it? And how do I know that it doesn't mislead me? For if it can guide me right, it can also guide me wrong.
>
> ((Intuition an unnecessary evasion.))
>
> 214. If an intuition is necessary for continuing the series 1 2 3 4 . . . , then also for continuing the series 2 2 2 2 . . . [17]

Wittgenstein's point is that the thought that the continuation of a series requires a new intuition at each instance would, if tenable, also apply to the seemingly most simple kind of rule, the one that requires only the infinite repetition of the same. And if intuition *here* functions as a kind of inner voice, then it also appears *even here* that it might mislead. But if even the infinite repetition of the same is not capable of "securing" the determinacy of the rule's extension, then nothing—at least nothing on the level of the internal or mental *accompaniments* of the actual practice and circumstances of learning and understanding—can be seen as doing so. What is rather to be seen as the necessary, as well as sufficient, condition for such understanding is the complex, various, and irreducible circumstances *in which* such a rule is normally understood, learned, and followed: the circumstances, that is, of the practices of "mathematical" teaching and learning within the context of a human life.

> 47. If a rule does not compel you, then you aren't following a rule.
>
> But how am I supposed to be following it; if I can after all follow it as I like?
>
> How am I supposed to follow a sign-post, if whatever I do is a way of following it?

[17] PI, 213–214

> But, that everything can (also) be interpreted as following, doesn't mean that everything is following.
> But how then does the teacher interpret the rule for the pupil? (For he is certainly supposed to give it a particular interpretation.) Well, how but by means of words and training?
> And if the pupil reacts to it thus and thus; he possesses the rule inwardly.
> But this is important, namely that this reaction, which is our guarantee of understanding, presupposes as a surrounding particular circumstances, particular forms of life and speech. (As there is no such thing as a facial expression without a face.)
> (This is an important movement of thought.)[18] (RFM pp. 413-14).

If Wittgenstein is right about this, what consequences follow for the learning and teaching of mathematics? At one level, in developing these examples, Wittgenstein is just calling attention to what actually does happen in the classroom and to such ordinary and practically unavoidable circumstances of pedagogy as that a student may, on being given any kind of instruction, still fail to grasp its point or that no single explanation or example of a method can be relied upon to produce understanding in *any* student or at *any* moment. But on a broader and more "philosophical" level, Wittgenstein's considerations show the infelicity of any conception of mathematical pedagogy that sees it as a matter *simply* of giving explanations or displaying regular methods at all. What his remarks seem to point to is that, since the understanding invoked and drawn upon in "knowing how to go on" is not just "there" in any given item or symbol, this understanding cannot be successfully produced, in general, by means of many of the practices that are in fact pervasively common in mathematics classrooms, especially at grade school levels: practices such as repeated drilling in mechanical methods or rote memorization of formulas. If the infinite series of values "determinable" by means of a mathematical technique is not just available to be "read off" from Platonic heaven or reality in itself, then it is also not simply "available" in the symbolic expression of a rule or the repetition of a mechanical procedure, either. Rather, if the teaching and learning of technique are irreducible in the way Wittgenstein suggests, what matters most is to preserve this irreducibility, to maintain it as an intrinsic and unavoidable part of the relationship between teacher and student involved in the practice of learning mathematics, and to understand the pedagogical experience as irreducibly part of the bearing of mathematics on the diverse circumstances of the individual and collective lives we lead, which is to say as irreducibly part of *mathematics* itself.

III

Drawing on Wittgenstein's remarks, I have argued for a conception of mathematical technique according to which it is both (on the one hand) situated in the complex context of human forms of life and (on the other) nevertheless *fully* determinate and "objective." I have further suggested that such a conception of technique, as

[18] RFM, VII-47, pp. 413–414

fully and irreducibly "situated" within the finitude and temporality of a human life, can be seen as an irreducible and essential part of what mathematics *itself* is, so that it is not really possible to characterize "mathematical entities" or "truths" as they are even "in themselves" without reference to the complex and dynamic circumstances of a human life. This conception can itself seem mysterious, however, given typical and longstanding assumptions about the temporality of mathematics, assumptions that characterize mathematical facts or truths as "outside" time and thus immune to the possibility of change or transformation. As thus "outside" time and change but nevertheless available in a unitary and objective way to us, these facts or truths are also, on this traditional conception, in some sense "outside" the empirical world of facts and experience: they stand beyond or before the totality of such facts and are wholly isolated from the contingencies they involve. These assumptions already make it mysterious *how* an entity situated within time (such as ourselves) *can* (so much as) have any kind of "access" to them; and even granting some such "access," they pose the problem of the temporal form of mathematical understanding itself. How (as we may put it) do we have, in finite temporality and at a distinct moment, a sufficient *insight* into the "whole" extent of an infinite mathematical structure? As I shall argue in this last section, this question (as Wittgenstein has pursued it) can be seen as marking in an interesting way a set of basic questions about the form of time as it is given or accessible to a human understanding and as it can be seen as structuring the world as such.

As we have seen, in his remarks in *RFM* and the *PI*, Wittgenstein is concerned to refute a conception of the infinite totality of a mathematical series on which all of its members would be understandable as "just there," all at once, as open, for instance, to simple *inspection* by a divinity or divine entity. To this he opposes the idea of a mathematical or calculative technique, a *doing* of something "by means of mathematics," which essentially constitutes what it is to (be able to) know the series, and hence plays an *essential* role in (as we may put it) constituting the series itself. What is more and what is given in and through the technique are not to be seen as consisting simply in *any* totality of empirical facts about what in fact is calculated or discovered: Wittgenstein says that even if God were omniscient in the sense of knowing all of these facts, up to the "end of the world," he could not know the answer to a question about what is in the decimal expansion without actually doing the calculation himself.

These remarks, of course, are not directly about time or the temporal form of the world. But their topic is nevertheless evidently very closely related to the question of the structure of time and indeed to the question of the possibility of a realist position on it. This relation is marked not only in the intimate historical connections of philosophical thought about the subject matter of mathematics, since at least Plato, with the idea of the timeless or extra-temporal, but more directly and immediately in the context of discussion that clearly shaped many of Wittgenstein's own ideas about mathematics and truth: that of the debate between formalists and intuitionists of the 1920s and early 1930s. In broader philosophical terms, both of these positions are notable for the particular kind of temporal perspective they involve. On the formalist's conception, the specification of a formal system along with its constitutive rules is itself enough to secure access (given at least its consistency) to the entire and infinite realm of what it discusses; this specification is in itself atemporal, and the rules are themselves to be finitely stateable in such a way as to be repeatable *ad infinitum*. Here, whereas the actual "application" of the rules (e.g., in a calculation) requires some sort of actual process or agency, the rules *in themselves* and what they determine are nevertheless essentially extra-temporal: independent and fixed with the specification of the formal system as the "ideal" system that it is. For the intuitionist, by contrast, there is always and irreducibility a *temporal* element in our mathematical cognition: we cannot take ourselves to genuinely have or understand a mathematical structure unless we do so by means of a finite, and temporal, intuition; and the being and existence of mathematical entities and phenomena is itself essentially dependent on the anterior and posterior structure of flowing time.[19]

Do Wittgenstein's remarks quoted above about mathematical practice and technique, then, show him to be a formalist or an intuitionist about mathematics and time? Again, the answer is "neither-nor," but we can see more clearly the temporal implications of what is in fact his position by considering how it might be generalized to provide a broader conception of the way that time itself is "given" to be thought and experienced. As we have seen above, Wittgenstein's purpose in the remark about God and omniscience is not necessarily to dispute that there is such a thing as "the decimal expansion of π," and it is not to maintain the "anti-realist" position that there is no "fact of the matter" about whether the string of 7 s appears in the expansion or not. What is disputed in the remarks is rather the misguided impression that realism itself demands that anyone—God included—could have access to the *whole* of what is produced by the procedure *without* going through that procedure itself. To dispute this is not to affirm a limitative finitism about the expansion of π or about mathematics in general or to require anti-realism about such places in the decimal expansion as have not *yet* been determined by any human mathematician. But nor can it be that Wittgenstein here envisions an ultimate basis for *our* procedure of calculating the expansion of π in a kind of primitive temporal *intuition* and of the kind that Brouwer's intuitionism proposed at the ultimate (and ultimately temporal) basis of mathematical knowledge.[20] We know, biographically, that Wittgenstein was decisively influenced by Brouwer's intuitionism just before his return to philosophical activity in 1929; this influence leaves its mark, in particular, on some of the anti-realist and verificationist-seeming suggestions he makes about the relationship of proof and truth in mathematics during the 1930s. But there is also abundant evidence that he had abandoned both

[19]For the terms of the dispute, see, e.g., Hilbert (1925) and Brouwer (1912).

[20]For of course Wittgenstein's point here is exactly not to invoke or rely upon conditions of an interior, psychological, or subjective kind as an alternative to the Platonism he also rejects. It is rather to criticize the whole configuration of thought about mathematics and time that is marked by the oscillation between Platonism and subjectivism and in particular determined by the conception of the nature of a rule that both essentially share.

verificationism and intuitionist strictures (for instance, against the use of the law of excluded middle) by the time he wrote most of the *Philosophical Investigations*.

The decisive consideration here, in fact, is the one already rehearsed above with reference to the question of continuing a regular series: if it is impossible to explain the basis for "going on" correctly with the expansion of a series *without* appealing to the directive provided by a primitive intuition, it is also impossible to explain it *by means of* such an appeal. This is because the appeal to a basic guiding intuition, as Wittgenstein points out, is simply (once more) the appeal to an essentially finite item, which does not settle its own interpretation and so simply repeats the original problem. Analogously or homologously, if we cannot explain the givenness of time to our experience and thought *without* appealing to a basic intuitive giving, we cannot explain it *by* doing so either.

What remains nevertheless of Brouwer's idea of a constitutive dependence of mathematics on time, and does certainly influence Wittgenstein's remarks here, is the idea of the form of the "facts" as conditioned by the form of a possible understanding of them, and this form itself as being irreducibly temporal. The point is that we cannot conceive of these facts as "given" from wholly outside time or presented as such to a being that exists outside it, except through a form which is itself in the relevant sense temporal, that of a calculative procedure. Rather, the necessity of conceiving the temporal facts from a position *within* time marks the form of these facts themselves, in such a way that it is not even so much as coherent to suppose that they could be conceived or perceived from beyond any such position.

If we were, then, to draw out the temporal analogue to Wittgenstein's remark in RFM about the expansion of π, we might indeed be tempted to say something like: even God can perceive and understand the facts about things happening in time only on the basis of an activity that is itself temporal, i.e., itself *within* the time it perceives and understands. To say this would not be to deny the very possibility of something like a view on the *whole* of time (a view, as it were, of the world *sub specie aeternitatis*). But it would be to deny that such a view can be taken *otherwise* than from a position that is itself essentially located within time and to affirm that this necessity marks the very form of the givenness and availability of entities and phenomena themselves. To put the position this way would be to adumbrate one of the several ways in which a constitutive reflection on finitude and the infinite, including especially their forms in relation to "mathematical" truths and results, sheds light on the necessary form of the phenomena and experiences of the world itself and indeed as they are in themselves.

What would be, then, the consequence of such a view, if maintained generally, for our understanding of the time of the world itself? In arguing against the possibility and tenability of the view of mathematical entities as simply *atemporal* and independent of the temporality and dynamism involved in a technique, Wittgenstein also suggests the emptiness of any idea of the world and its phenomena as surrounded by, or surveyable from, a position that is itself wholly extra-temporal. Such a position is the one that is envisioned as occupied, on the traditional "Platonistic" conception, by the mathematical entities itself, and the idea of a divine intellect capable of knowing them in their infinite totality without (temporal) calculation or procedure then supplies the necessary, if falsifying, correlative position of possible knowledge. But if the picture of mathematical truth and entities that situates them in such a position is shown, on the basis of a constitutive consideration of the temporality of mathematical technique, to be untenable, then it is doubtful whether any significant support remains for the idea of an extra-temporal realm of determinate existence at all. Since this idea has, in fact, drawn much of its historical support (both in the philosophy of Plato himself and in subsequent developments of the theme) from the putative "timelessness" of mathematical entities, once this support is removed, it is not clear whether the overall conception of temporality that it itself involves can remain in place.[21] At any rate, since it is not clear any longer that *mathematics* requires, or even encourages us, to postulate or invoke any kind of "timeless" realm of existence outside the (temporal) world, it seems that any considerations that still incline us to do so must be seen to arise from very different (and distinctively nonmathematical) sources.

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[21] For a fuller argument, drawing in part on Wittgenstein, and a development of the alternative temporality of the world, this might be taken as suggesting; see Livingston (2017), esp. Chapters 6 and 7.

Gödel's Legacy

Martin Davis

Gödel in 1933

In his truly remarkable paper of 1931, Kurt Gödel casts grave doubts on the viability of Hilbert's program for providing a consistent foundation for mathematics. In addition, it compelled logicians to view their subject through entirely new lenses. Two years later, Gödel was invited by the Mathematical Association of America to address one of its meetings. In his address, he spoke as follows:

> ... we are confronted by a strange situation. We set out to find a formal system [of axioms] for mathematics and instead of that found an infinity of systems, and whichever system you choose ..., there is one more comprehensive, i.e., one whose axioms are stronger ... But ... this character of our systems ... is in perfect accord with certain facts which can be established quite independently ... For any formal system you can construct a proposition – in fact a proposition of the arithmetic of integers – which is certainly true if the given system is free from contradictions but cannot be proved in the given system. Now if the system under consideration (call it *S*) is based on the theory of types, it turns out that ... this proposition becomes a provable theorem if you add to *S* the next higher type and the axioms concerning it. ... the construction of higher and higher types ... is necessary for proving theorems even of a relatively simple structure.[1]

This view of a hierarchy of formal systems of increasing strength in each of which more propositions "even of a relatively simple structure" become provable has held. At the bottom are systems in which the basic theorems of number theory are provable, followed by systems adequate for analysis, including functional analysis. As one advances upward, one goes beyond the full system based on the

[1] Gödel [5].

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Zermelo-Fraenkel axioms (including the axiom of choice)[2] to systems obtained by adding to those unprovable assertions requiring the existence of very large sets. In this article, I will briefly survey the scene after 85 years have passed.

Algorithmic Computability

In the years 1934–1936, efforts by Gödel, Alonzo Church, his student S.C. Kleene, Alan Turing, and E.L. Post solidified an understanding of *computability* as a precise mathematical concept. This made progress possible in a number of directions. Gödel undecidability could now be presented in a general setting in which formal systems are seen as essentially providing algorithms for generating streams of theorems. Mathematical problems that sought the existence of algorithms could now be settled negatively in a definitive way by proving that no such algorithm is possible. And finally by finding suitable normal forms for computable functions, Gödel's "even of a relatively simple structure" could be made even simpler. We define:

A *Diophantine statement* is an assertion of the form:

$$\text{For all natural numbers } x_1, x_2, \ldots, x_n,\ p(x_1, x_2, \ldots, x_n) \neq 0,$$

where p is a polynomial with integer coefficients.

Then one can prove:

Theorem *No algorithm for generating true Diophantine statements can generate all such statements.*

If one asks what can it mean to say that a Diophantine statement is "true," there is in principle a straightforward reply: it means that if any specific natural numbers are substituted for the variables and the value of p then calculated, the result will never be zero. If in particular the numbers are specified in the decimal notation in common use, this calculation can be carried out using the algorithms for adding, subtracting, and multiplying that we learned as children.

This result is based on the work leading to a negative solution of Hilbert's tenth problem. It is worth emphasizing that the proof is entirely constructive. Given a particular formal system, e.g., the Zermelo-Fraenkel axioms with first-order logic, one can construct a specific true Diophantine statement D with the property that if no false Diophantine statements are provable in the system, then D is unprovable in the system. However, a warning is necessary: the specific polynomial will not be a pretty thing. The coefficients need to code the Zermelo-Fraenkel axioms and so will be enormous.

Although many famous problems are provably equivalent to Diophantine statements, including Fermat's Last Theorem, the Goldbach's conjecture, and even the

[2]See the Appendix for a list of the Zermelo-Fraenkel axioms.

Riemann hypothesis,[3] there are as yet no examples, outside of logic itself, of previously posed Diophantine statements being proved undecidable in one of the usual formal systems. However, the distinguished logician Harvey Friedman has created families of combinatorial propositions, some of them provably equivalent to Diophantine statements, that are not provable from the Zermelo-Fraenkel axioms but do become provable if axioms asserting the existence of very large sets are added.[4]

The situation is very different with respect to problems seeking algorithmic solutions: Hilbert's tenth problem, the word problem for groups, and very many other problems from various branches of mathematics have been proved algorithmically unsolvable.[5]

Cantor's Continuum Problem

Cantor's continuum hypothesis can be stated as follows:

CH *Every uncountable set of real numbers can be mapped one-one to the set of all real numbers.*

Cantor tried without success to prove CH, and it was the very first problem in Hilbert's famous list from his 1900 address. In 1938 Gödel achieved an important result concerning CH. He defined a property of sets he called *constructibility* and considered the proposition:

A: *Every set is constructible*.

He then proved (using the abbreviation "ZF" for the Zermelo-Fraenkel axioms):

- If ZF is consistent, then so is ZF plus A.
- ZF plus A $\Rightarrow$ CH.

From this, it follows that if ZF is consistent, then CH cannot be *disproved* from ZF. For some, this meant simply that now one could just assume CH as an additional axiom, confident that no contradiction would result from doing so. In fact, for a large class of propositions (including, in particular, Diophantine statements), if they can be proved from ZF plus CH, then they can be proved from ZF only.

Gödel conjectured that CH was actually independent of ZF, that CH not only couldn't be disproved, but also it couldn't be proved from ZF either. He was right, but this wasn't actually proved until 1963. In 1947 Gödel published an expository article on CH in which he argued that CH has a definite truth value and its undecidability from ZF implied, not that CH could never be decided, but

[3]See [2, pp. 331–339].

[4]For a sample of the many propositions of this kind that Friedman has developed, see [3, 4].

[5]See, e.g., [1, 10].

rather that ZF was too weak to decide it. Gödel further conjectured that eventually a new powerful axiom from which it will follow that CH is false will be found and accepted.[6]

ZF, after all, was but a way station in the transfinite hierarchy of axiom systems Gödel had already discussed in his 1933 lecture. To understand this better, it's helpful to think of the ZF axioms as assertions that certain sets exist. Along with axioms asserting that specific sets exist, there are axioms that provide operations for proceeding from some given sets to certain other sets. For example, the *power set* axiom asserts the existence of the set of all subsets of a given set. One is readily led to form a new set that transcends ZF by forming the *closure* under all such operations. It can be proved that the set so formed has a larger cardinal number than any set that can be proved to exist from ZF. Such a cardinal number is called *inaccessible*.[7] Inaccessible cardinals lie at the bottom of a hierarchy of "large" cardinals, the existence of each which implies the consistency with ZF of all those further down in the hierarchy.[8]

Paul Cohen proved that CH could not be proved from ZF by showing how to obtain models of ZF in which CH is false. In fact, using Cohen's *forcing* method, it became apparent that models of ZF were ubiquitous, as prevalent as, say, finite groups. These models can be viewed as directed **countable** graphs in which the vertices serve as "sets" and the edges provide the membership relation between a set and its elements. A set will be large in the model because the sets of ordered pairs that could provide maps to smaller sets are omitted from the models. No one who believes that real numbers and sets of real numbers are definite well-defined concepts will regard such models as saying anything about CH itself.

Projective Sets

Writing $\mathcal{R}$ for the set of real numbers, if $B \in \mathcal{R}^{n+1}$, we write $\mathbf{Proj}(B)$ for the set $A \in \mathcal{R}^n$ defined by[9]

$$< x_1, \ldots, x_n > \in A \Leftrightarrow \exists y \in \mathcal{R}[< x_1, \ldots, x_n, y > \in B]$$

6Gödel [7].

7To prove that the existence of inaccessible cardinals cannot be proved from the ZF axioms, one observes that a model of ZF could be defined using a set of that cardinality. The existence of such a model would imply that the ZF axioms are consistent. Since the entire proof could be carried out within the scope of the ZF axioms, this is impossible because, as Gödel has shown, if the ZF axioms are consistent, their consistency cannot be proved within a system based on those axioms.

8See [8] which discusses the historical development of the study of "large" cardinals as well as their inter-relationships. The "Chart of Cardinals" on p. 472 shows the variety of inhabitants of this remarkable zoo of larger and larger sets.

9This section and the next are based on [9].

The hierarchy of *projective sets* is defined simultaneously in all $\mathcal{R}^n$ as follows:

$$\begin{aligned}\mathbf{\Sigma}^1_0 &= \text{the set of Borel sets}\\ \mathbf{\Pi}^1_m &= \{A \subseteq \mathcal{R}^n \mid \mathcal{R}^n - A \in \mathbf{\Sigma}^1_m\}\\ \mathbf{\Sigma}^1_{m+1} &= \{A \subseteq \mathcal{R}^n \mid \exists B \in \mathbf{\Pi}^1_m[A = \mathbf{Proj}(B)]\}\end{aligned}$$

Projective sets were studied intensively during the early 20th century, but the work ran into obstacles. A brief survey of what was accomplished follows.

Hierarchy Theorem (Lusin) *For* $m = 0, 1, 2, \ldots$

$$\mathbf{\Sigma}^1_m \subset \mathbf{\Sigma}^1_{m+1},\ \mathbf{\Pi}^1_m \subset \mathbf{\Pi}^1_{m+1}.$$

For $m > 0$*, we have* $\mathbf{\Sigma}^1_m - \mathbf{\Pi}^1_m \neq \emptyset$.

Souslin's Theorem $\mathbf{\Sigma}^1_1 \cap \mathbf{\Pi}^1_1 =$ *the Borel sets.*

Theorem (Lusin, Souslin) *Every set in* $\mathbf{\Sigma}^1_1$ *is Lebesgue measurable and has cardinality either* $\leq \aleph_0$ *or* $2^{\aleph_0}$.

A class of sets Γ is called a *uniformization class* if for every $A \in \Gamma \cap \mathcal{R}^2$, there is a map $B : \mathcal{R} \to \mathcal{R}$ such that $B \in \Gamma$ and $B \subseteq A$. Kondo proved: $\mathbf{\Pi}^1_1$ *and* $\mathbf{\Sigma}^1_2$ *are uniformization classes.*

A class of sets Γ is called a *reduction class* if for all $A, B \in \Gamma$, there are $A', B' \in \Gamma$ such that

$$A' \subseteq A,\ B' \subseteq B,\ A' \cap B' = \emptyset,\ \text{and}\ A' \cup B' = A \cup B.$$

Kuratowski proved: $\mathbf{\Pi}^1_1$ *is a reduction class.*

Except for the hierarchy theorem, all of these results concern only the bottom levels of the hierarchy. Efforts to go further failed. Many years later, after Paul Cohen's forcing method became available, it became clear why. It turned out that no further progress was possible without going beyond ZF.

Determinacy

Although Gödel's suggestion that a new axiom might make it possible to prove or disprove CH in a definitive manner has not proved fruitful (at least not yet), in the realm of projective sets, a new appropriate axiom has been found. This axiom involves infinite games of a certain kind.

Associated with a given set, $A \subseteq \mathcal{R}$ is a game defined as follows: players I and II alternately move by each specifying a binary digit 0 or 1. They thus specify the binary expansion of a real number x in the unit interval. If x is the fractional part of a member of A, then I wins; otherwise II wins.

Definition The set $A \subseteq \mathcal{R}$ is *determined* if either there is a strategy for I to win or there is a strategy for II to win.

Using the axiom of choice, one can prove: *there is a set that is not determined.* This use of the axiom of choice suggests that sets of real numbers for which one can supply an explicit definition should be determined. In particular, this should apply to projective sets. A first step was taken by Donald "Tony" Martin:

Theorem (Martin) *Every Borel set of reals is determined.*

Martin's proof requires the full force of ZF.[10] It has been shown that if ZF is consistent, then the statement *All sets in* $\mathbf{\Pi}^1_1$ *are determined* is **not** provable from ZF. This suggests the axiom:

Projective Determinancy (PD) *Every projective set of real numbers is determined.*

The theorems below, which beautifully complete the earlier investigations of projective sets, are obtained when PD is added to ZF as an additional axiom.

Theorem *Every projective set is Lebesgue measurable and is either countable or of cardinality* $2^{\aleph_0}$.

Theorem *If n is odd, then* $\mathbf{\Pi}^1_n$ *is a uniformization class, and if* $n > 0$ *is even, then* $\mathbf{\Sigma}^1_n$ *is a uniformization class.*

Theorem *If n is odd, then* $\mathbf{\Pi}^1_n$ *is a reduction class, and if* $n > 0$ *is even, then* $\mathbf{\Sigma}^1_n$ *is a reduction class.*

Tony Martin and John Steel have tied these investigations to the study of large cardinals by deriving PD from assumptions asserting the existence of certain large cardinals.

Conclusion

Gödel has left us with a conception of an increasing hierarchy of axiom systems from each of which propositions are provable that are not provable from axioms lower in the hierarchy. So far this conception has affected only the fringes of mathematical practice. Whether it will prove important for problems central to mathematics, only the future can reveal. Gödel himself has made the provocative conjecture that the Riemann hypothesis (RH) regarding zeros of the zeta function will require set-theoretic methods for its proof.[11] Given the serious attention paid to RH by analysts and number theorists and its very statement involving notions at the heart of classical complex function theory, I think that Gödel may very well be right.

[10]See the Appendix. Almost everything in ordinary mathematics can be carried out without the final axiom of "Collection." But Martin's proof requires this axiom.

[11]Gödel [6, p. 307].

Appendix: The Zermelo-Fraenkel Axioms

For brevity, I have omitted universal quantifiers at the left of these statements. The axioms of *separation* and *collection* are each really an infinite sequence of axioms in which the letter ϕ can be replaced by any legitimate formula written in terms of logical operations and the symbols occurring in the other axioms.

Empty set	$x \notin \emptyset$
Extensionality	$\forall t\,(t \in x \leftrightarrow t \in y) \rightarrow x = y$
Pairs	$t \in \{x, y\} \leftrightarrow t = x \vee t = y$
Definition	$\{x\} = \{x, x\}\,;\ < x, y >= \{\{x\}, \{x, y\}\}$
Definition	$x \subseteq y \Longleftrightarrow \forall t \in x\,(t \in y)$
Power	$t \in \mathcal{P}(x) \leftrightarrow t \subseteq x$
Union	$t \in \bigcup x \leftrightarrow \exists z \in x\,(t \in z)$
Definition	$x \cup y = \bigcup\{x, y\}$
Infinity	$\emptyset \in \omega\,;\ x \in \omega \rightarrow x \cup \{x\} \in \omega$
Separation	$\forall u \exists x \forall t\,[t \in x \leftrightarrow t \in u \wedge \phi(t, \vec{p}\,)]$
Definition	$\mathrm{Fn}(f) \Leftrightarrow < x, y_1 >, < x, y_2 > \in f \rightarrow y_1 = y_2$ $f(x) = y \Longleftrightarrow < x, y > \in f$
Choice	$\forall x \exists f\,(\mathrm{Fn}(f) \wedge \forall z \in x\,[z \neq \emptyset \rightarrow f(z) \in z])$
Foundation	$x \neq \emptyset \rightarrow \exists y \in x \forall z \in y\,(z \notin x)$
Collection	$\forall x \in u \exists y \phi(x, y, \vec{p}) \rightarrow \exists z \forall x \in u \exists y \in z \phi(x, y, \vec{p})$

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Varieties of Maverick Philosophy of Mathematics

Carlo Cellucci

Reuben Hersh is a champion of maverick philosophy of mathematics. He maintains that mathematics is a human activity, intelligible only in a social context; it is the subject where statements are capable in principle of being proved or disproved and where proof or disproof brings unanimous agreement by all qualified experts; mathematicians' proof is deduction from established mathematics; mathematical objects exist only in the shared consciousness of human beings. In this paper I describe my several points of agreement and few points of disagreement with Hersh's views.

Mainstream, Maverick, and Mathematical Practice

In the twentieth century, mainstream philosophy of mathematics has been foundationalism, the view that the philosophy of mathematics is research on the foundations of mathematics. But in the second part of the century, Lakatos, with his PhD dissertation (Lakatos 1961), started a new tradition, known as "the maverick tradition" (Kitcher and Aspray 1988, 17). Lakatos (1961) is unpublished as a whole, but pieces of it have been published separately in Lakatos 1963–1964, Lakatos 1976, and Lakatos 1978, II, Chap. 5.

Lakatos criticizes mainstream philosophy of mathematics, because it "denies the status of mathematics to most of what has been commonly understood to be mathematics, and can say nothing about its growth"; in particular, it can say nothing about "the 'creative' periods" and "the 'critical' periods of mathematical theories" (Lakatos 1976, 2). What is more important, in mainstream philosophy of

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mathematics, “there is no proper place for methodology qua logic of discovery” (ibid., 3). On the contrary, the philosophy of mathematics must be primarily concerned with methodology qua logic of discovery. Although there is no infallibilist logic of discovery, namely, “one which would infallibly lead to results,” nevertheless “there is a fallibilist logic of discovery” (ibid., 143–144, footnote 2). Already the Greeks devised a heuristic method which is “a standard pattern of the logic of discovery,” namely, “the method of analysis and synthesis” (Lakatos 1978, II, 72).

However, Lakatos’ approach to methodology qua logic of discovery is unsatisfactory, because it does not really tell you how to discover a hypothesis; it assumes that you already have one (see Cellucci 2013b). Indeed, according to Lakatos, methodology does not presume to give “rules for arriving at solutions, but merely directions for the appraisal of solutions already there” (Lakatos 1978, I, 103, footnote 1).

Nevertheless, despite the limitations of Lakatos’ approach to methodology qua logic of discovery, the origin of the maverick tradition must be credited to him, because he was the first to assert that the philosophy of mathematics must be primarily concerned with methodology qua logic of discovery. Admittedly, some anti-foundationalist statements already occur in Wittgenstein, but Wittgenstein claims that “mathematical discovery is always unmethodical: you have no method for making the discovery” (Wittgenstein 2016, 46). Therefore, Wittgenstein cannot be considered the initiator of the maverick tradition.

More recently, another approach has been proposed as a third way between mainstream and maverick philosophy of mathematics, the so-called philosophy of mathematical practice, whose manifesto is Mancosu (2008) (a related manifesto is Ferreirós 2016). However, the philosophy of mathematical practice is not a genuine alternative, but only a refurbished form of foundationalism. As Mancosu says, philosophers of mathematical practice “do not engage in polemic with the foundationalist tradition” nor “think that the achievements of this tradition should be discarded or ignored as being irrelevant to philosophy of mathematics” (Mancosu 2008, 18). They reject “the polemic against the ambitions of mathematical logic as a canon for philosophy of mathematics” and do not see “mathematical logic, which had been essential in the development of the foundationalist programs,” as “ineffective in dealing with the questions concerning the dynamics of mathematical discovery” (ibid., 4). Far from opposing “the foundationalist tradition,” they are only “calling for an extension to a philosophy of mathematics that will be able to address topics that the foundationalist tradition has ignored” (ibid., 18).

After Lakatos, early developments in the maverick tradition were Hersh (1979), Davis and Hersh (1981), Kitcher (1983), and Tymoczko (1986). In particular, Reuben Hersh is a champion of maverick philosophy of mathematics. He presents his position as “a subversive attack on traditional philosophies of mathematics” (Hersh 1997, xi). Hacking even calls Hersh “the mother of all gadflies” (Hacking 2014, 198).

Sometimes, maverick philosophy of mathematics is not sharply distinguished from the philosophy of mathematical practice. Even Hersh says: “We are concerned with ‘the philosophy of mathematical practice’. Mathematical practice includes studying, teaching and applying mathematics" (Hersh 2014, 59). Then, however, he continues: "But I suppose we have in mind first of all the discovery and creation of mathematics – mathematical research" (ibid.). We aim at "reporting and discussing what people really do" in "the process of mathematical discovery" (ibid., 55). Indeed, maverick philosophy of mathematics is essentially different from the philosophy of mathematical practice. Philosophers of mathematical practice do not have in mind first of all the discovery and creation of mathematics – mathematical research. They do not aim at reporting and discussing what people really do in the process of mathematical discovery.

Maverick philosophy of mathematics has certain general features (see Cellucci 2013b). Nevertheless, there are distinct varieties of maverick philosophy of mathematics. Hersh calls "humanism" his own variety, because he wants to stress that "mathematics is human. It's part and fits into human culture. (Not Frege's abstract, timeless, tenseless, objective reality)" (Hersh 2014, 158). I have developed my own variety in a number of works, more recently in Cellucci (2017), greatly benefiting from Hersh's works and from correspondence with him. In this paper, I describe my several points of agreement and few points of disagreement with Hersh's views.

The Definition of Mathematics

What is mathematics? The common naïve answer, "Mathematics is what mathematicians do," is clearly inadequate, because it immediately gives rise to the question: What are mathematicians? Answering that mathematicians are those people who do mathematics is begging the question. The problem is particularly acute in the case of mathematicians who create new parts of mathematics. Why should they be considered mathematicians, if what they do is something no mathematician does at the time?

But, if the common naïve answer to the question what is mathematics is clearly inadequate, you will hardly find a better answer in introductory books on mathematics. Simply, in those books, you will find no answer at all.

On the contrary, Hersh gives a partial answer to the question what is mathematics, by saying that "mathematics is the subject where statements are capable in principle of being proved or disproved, and where proof or disproof bring unanimous agreement by all qualified experts" (Hersh 2014, 163).

I agree with Hersh's partial answer, with some qualifications. In my view, proof should not be understood, as Hersh says, as deductive proof, but rather as analytic proof (see below). Moreover, instead of saying that proof brings unanimous agreement by all qualified experts, one should rather say that it brings agreement by the majority of qualified experts, since empirical research shows that "there is not universal agreement among mathematicians regarding what constitutes a valid proof" (Inglis et al. 2013, 279). Kline even says that "the proofs of one generation are fallacies of the next" (Kline 1980, 318).

Philosophy of Mathematics and Human Knowledge

Parallel to the question "What is mathematics?", there is the question "What is the philosophy of mathematics?" According to a widespread opinion, the philosophy of mathematics is a specialized branch of philosophy. But this only holds of last century's philosophy of mathematics. From Plato and Aristotle to Kant, a broad general tradition treated the philosophy of mathematics, not as a specialized area of philosophy but as part of a general approach to knowledge. Only at the end of the nineteenth century, with Frege, the philosophy of mathematics took a sharp turn away from this broad general tradition and was treated as a specialized subject.

Thus, however, the philosophy of mathematics lost connection with the general question of human knowledge and became more and more irrelevant. Indeed, as Hersh says, mainstream philosophy of mathematics is "an encapsulated entity, isolated, timeless, ahistorical, inhuman, connected to nothing else in the intellectual or material realms" and "routinely done without reference to mind, science, or society" (Hersh 1997, 25). But "how can it be claimed that the nature of mathematics is unrelated to the general question of human knowledge?" (Hersh 2014, 68). Instead, one must "look at what mathematics really is, and account for it as a part of human knowledge in general" (ibid., 47).

I completely agree with Hersh that the nature of mathematics cannot be unrelated to the general question of human knowledge, and hence one must account for mathematics as a part of human knowledge in general. Indeed, this is what I attempt to do in Cellucci (2017), as a companion to Cellucci (2013a).

Like all knowledge, almost all mathematical knowledge originally arose from the need to deal with the external world. Thus, geometry arose from the Egyptians' need to perform new land measurements after every flood of the Nile; arithmetic arose from the Phoenicians' need to deal with their trade activities; calculus arose from the creators of modern science need to deal with the motion of planets and other moving objects. Therefore, the nature of mathematics is not unrelated to the general question of human knowledge. Admittedly, at some point, mathematics turned to ask internal questions, not directly related to experience. But even those internal questions ultimately evolved out of questions related to experience.

As Poincaré says, "the desire to understand nature has had on the development of mathematics the most constant and happiest influence" (Poincaré 2013, 284). Thus, "the pure mathematician who should forget the existence of the exterior world would be like a painter who knew how to harmoniously combine colors and forms, but who lacked models. His creative power would soon be exhausted" (ibid.).

Philosophy of Mathematics and Mathematicians

In my view, the primary task of the philosophy of mathematics is to give an answer to questions such as: What is the nature of mathematics? How is mathematical knowledge discovered? What is the nature of mathematical objects? What is mathematical explanation? Why is mathematics applicable to the world? How is the question of mathematical knowledge related to the general question of human knowledge? What is the role of mathematics in human life? (See Cellucci 2017, Chap. 18).

Hersh holds a different view, because he says: “By ‘philosophy of mathematics’ I mean the working philosophy of the professional mathematician, the philosophical attitude toward his work that is assumed by the researcher, teacher, or user of mathematics” (Hersh 1979, 31).

A problem with Hersh’s view is that the working philosophy of the professional mathematician varies from period to period; within the same period, it varies from school to school; and within the same school, it varies from mathematician to mathematician.

For example, Hilbert felt that, “for the solution of a mathematical problem,” one must lay down the general “requirement of logical deduction” by means of “a finite number of steps based upon a finite number of hypotheses” which “must be exactly formulated,” because this “is simply the requirement of rigor in reasoning” (Hilbert 2000, 243–244).

Conversely, Enriques “did not feel the need of a logical demonstration of some property, because he ‘saw’; and that provided the assurance about the truth of the proposition in question and satisfied him completely” (Babbitt and Goodstein 2011, 242). Enriques even said: “We aristocrats do not need proofs. Proofs are for you commoners” (Parikh 2009, 16, footnote 10).

Therefore, the view that the philosophy of mathematics is the working philosophy of the professional mathematician entails that the philosophy of mathematics can only give answer to questions such as: What is the working philosophy of the professional mathematician put forward in a particular period, or by a particular school, or by a particular mathematician?

Thus conceived, the philosophy of mathematics is reduced to the history of mathematics. But the history of mathematics is written mainly on the basis of mathematics in finished form, that is, mathematics as presented in textbooks or journals, which has little or nothing to do with the way it is discovered. Therefore, the history of mathematics does not provide an adequate basis for understanding the real mathematical process.

The reason Hersh gives for the view that the philosophy of mathematics is the working philosophy of the professional mathematician is that “the professional philosopher, with hardly any exception, has little to say” (Hersh 1979, 34). In fact, “some philosophers who write about mathematics seem unacquainted with any mathematics more advanced than arithmetic and elementary geometry. Others are specialists in logic or axiomatic set theory; their work seems as narrowly technical as that in any other mathematical specialty” (ibid.).

Hersh is largely justified in saying so, because many philosophers who write about mathematics, instead of dealing with the real mathematical process, deal with artificial questions, which have no connection with that process. And yet, Hersh himself acknowledges that, even if “there are, indeed, occasional philosophical comments by leading mathematicians," nevertheless "the art of philosophical discourse is not well developed today among mathematicians, even among the most brilliant" (ibid.).

In fact, that mathematicians are experienced in doing mathematics does not automatically mean that they are experienced in reflecting on the nature of mathematics. As Lasserre says, if you "ask a mathematician 'What is mathematics?' he may justifiably reply that he does not know the answer, but that this does not prevent him from doing mathematics" (Lasserre 1964, 11). Indeed, the job of the mathematician qua mathematician is not to say what mathematics is but to do mathematics. Of course, nothing excludes that a mathematician may say what mathematics is, but this requires that the mathematician be experienced not only in doing mathematics but also in reflecting on the nature of mathematics.

The Distinction Between Front and Back

It has been stated above that the history of mathematics, being written mainly on the basis of mathematics in finished form, does not provide an adequate basis for understanding the real mathematical process. This suggests that an essential distinction must be made between mathematics in finished form and mathematics in the making.

Hersh formulates this distinction using the concepts of "front" and "back," as developed in Goffman (1956), Chap. 3. He says that, "like other social institutions, mathematics too has its 'front' and its 'back'," where "the 'front' of mathematics is mathematics in 'finished' form, as it is presented to the public in classrooms, textbooks, and journals. The 'back'" is "mathematics as it appears among working mathematicians, in informal settings, told to another in an office behind closed doors" (Hersh 2014, 36). Thus, the front is "established mathematics," namely, "the body of mathematics that is accepted as the basis for mathematicians' proofs" (ibid., 75). However, "the performance seen 'up front' is created or concocted 'behind the scenes' in back" (ibid., 36). Mainstream philosophy of mathematics does not recognize "that mathematics has a back. Finished, published mathematics – the front – is taken as a self-subsistent entity" (Hersh 1997, 36). But this conflicts with the fact that "it's impossible to understand the front while ignoring the back" (ibid.).

I agree with Hersh that mathematics has a front and a back. But I do not agree with him that the back of mathematics is mathematics as it appears among working mathematicians, in informal settings, told to another in an office behind closed doors. Mathematics as it appears there is just a preliminary, incomplete version of mathematics in finished form. The back of mathematics is, instead, the creative work of the mathematician, primarily the discovery work.

On the other hand, I completely agree with Hersh that it is impossible to understand the front of mathematics while ignoring the back. This contrasts with mainstream philosophy of mathematics, which does not recognize that mathematics has a back and hence bases its view of mathematics on mathematics in finished form, the front. This is very limiting, because mathematics in finished form is only intended to present, justify, and teach propositions already acquired, not to account for mathematics in the making. Therefore, from mathematics in finished form, one can only get information about the sequence of mathematical results and theories, not about the process by which mathematics is actually made.

Deductive Proof as Mathematicians' Proof

That mainstream philosophy of mathematics bases its view of mathematics on mathematics in finished form is clear from how it deals with the concept of proof.

It is widely agreed that proof is a means of extending mathematical knowledge, since "the key function of proofs is to elaborate methods for solving problems and thereby extending existing theories or creating new ones" (Rav 2007, 293). This implies that one should not base the concept of proof on mathematics in finished form, the front, but rather on mathematics in the making, because it is through the process by which mathematics is made that mathematical knowledge is extended.

On the contrary, mainstream philosophy of mathematics bases the concept of proof on mathematics in finished form. Somewhat surprisingly, Hersh also does so. Indeed, he says that he "focuses on proof – the 'front side' of mathematics" (Hersh 2014, 73). In his view, proof is deductive proof, because "deductive proof is the standard for acceptance of one's findings into the body of established mathematics" (ibid., 82–83). Deductive proof is deduction from established mathematics, because deduction "is how, starting from established mathematics, we establish a new result, which then becomes part of established mathematics" (ibid., 89). Deduction "connects some proposed 'result' to the body of established mathematics. Once the proposed theorem is accepted or established, one is permitted to use it in other proofs" (ibid., 81).

Hersh admits that "plausible reasoning"– such as induction or analogy–"is likely to be essential in finding the" deductive "proof" (ibid., 82). But he maintains that only deductive proof "legitimates a result as 'established'" (ibid.). Therefore, deductive proof "is the method by which established mathematics becomes established" (ibid.). So, "if you have made a publishable discovery, the process by which you found out your result probably won't be included in your article" (ibid.).

This is because, according to Hersh, plausible reasoning is not part of the concept of proof. It does not serve to find hypotheses that cannot be deduced from established mathematics but is only a heuristic means for finding deductions from established mathematics. Deductive proof is "mathematicians' proof: proof as it is understood by mathematicians" (ibid., 74).

A view of mathematicians' proof akin to that of Hersh is presented by Prawitz (see Prawitz 2014).

Mathematicians' Proof and Axiomatic Proof

That, for Hersh, deductive proof is mathematicians' proof means that for him mathematicians' proof "doesn't start from a pre-ordained set of axioms. It starts from relevant pieces of established mathematics out of which some new mathematical results can be derived and incorporated into established mathematics" (Hersh 2014, 73). Therefore, mathematicians' proof is not axiomatic proof. Axiomatic proof is deduction from axioms that "are not established" but "are simply postulated" (ibid., 78). Conversely, mathematicians' proof is deduction from established mathematics.

Many mathematicians think that axiomatic proof is mathematicians' proof, in particular, that the Zermelo-Fraenkel axioms of set theory ZF are the foundation for all of standard mathematics. Hersh opposes this view. He says that "ZF set theory is a branch of mathematics, not a 'foundation' for all the rest of mathematics" (ibid., 83). Generally, since axioms are not established but are simply postulated, "any proposed axiomatic 'foundation' cannot be as credible and reliable as the established mathematics they are supposed to support" (ibid.). So, one cannot maintain that "mathematics is axiomatic systems" (Hersh 1997, 41). Hence, "the foundationist project has lost its philosophical rationale" (ibid., 32).

I completely agree with Hersh on these points. The Zermelo-Fraenkel axioms of set theory ZF cannot be the foundation for all of standard mathematics because, by Gödel's first incompleteness theorem, there are mathematical sentences that are true but cannot be deduced from ZF. And, by Gödel's second incompleteness theorem, it is impossible to prove, by absolutely reliable means, that the axioms ZF are consistent. Generally, by Gödel's first incompleteness theorem, for any consistent, sufficiently strong, formal system, there are sentences of the system that are true but cannot be deduced from the axioms of the system. So, one cannot maintain that mathematics is axiomatic systems. Moreover, by Gödel's second incompleteness theorem, for any consistent, sufficiently strong, formal system, it is impossible to prove, by absolutely reliable means, that the axioms of the system are consistent. Hence, the foundationist project has lost its philosophical rationale.

From this it follows that the view that axiomatic proof is mathematicians' proof is inadequate. In fact, axiomatic proof is not mathematicians' proof but only a means to organize results already acquired for didactic purposes. (For more on this, see Cellucci 2017, Chaps. 12 and 21.)

This does not mean, however, that Hersh's view that deductive proof is mathematicians' proof is satisfactory. On the contrary, I will argue that it is faced with serious problems. First, though, I will consider some features of Hersh's concept of mathematicians' proof.

Mathematicians' Proof and Logic

What is the relation of Hersh's concept of mathematicians' proof to logic? According to Hersh, mathematicians' proof proceeds by reasoning that "makes no reference to the rules of logic" (Hersh 2014, 31). In fact, mathematicians "never mention logic in" their "work" (ibid., 78).

I suppose, however, that by this, Hersh does not mean to say that mathematicians' proof does not rely on the rules of logic. As several people, from Aristotle to Hilbert, have stressed, the rules of logic are embedded in mathematicians' proof, being rules of thought. So, mathematicians' proof relies on the rules of logic, and mathematicians implicitly use such rules, even if they are not aware of it. The rules of logic as a discipline reflect those embedded in mathematicians' proof and are obtained by analyzing them.

Thus, Hilbert states that proof is "carried out according to certain definite rules, in which the technique of our thinking is expressed" (Hilbert 1967, 475). For, "our understanding does not practice any secret arts, but rather always proceeds according to well-determined and presentable rules" (Hilbert 1998, 233). These rules can be stated, because "the mathematical understanding encounters no limits," and "it is even capable of discovering the laws of its own thought" (ibid.). The "fundamental idea" of logic as a discipline "is none other than to describe the activity of our understanding and to make a protocol of the rules according to which our thinking actually proceeds" (Hilbert 1967, 475). Proofs produced by the rules of logic as a discipline "are copies of the thought constituting customary mathematics" (ibid., 465).

Mathematicians' Proof and Intuition

What is the relation of Hersh's concept of mathematicians' proof to intuition? Several mathematicians say that intuition plays an essential role in mathematics. Thus, Wilder says that "intuition plays a basic and indispensable role in mathematical research" (Wilder 1967, 605). Kline says that "intuition plays a fundamental role in securing mathematical truths" (Kline 1980, 319). Burton says that, in a study of research-active mathematicians, "the overwhelming majority (83%)" of the participants "recognised something important which might be called intuition" at play "when they were coming to know mathematics" (Burton 1999, 28).

However, when mathematicians say that intuition plays an essential role in mathematics, they often do not mean "intuition" literally but only as a metaphor. When they use the word "intuition," they intend to refer to their feeling of "almost knowing" some hypothesis without having consciously gone through a step-by-step reasoning process to get there. This feeling can be explained in terms of the fact that they arrived at the hypothesis through unconscious plausible reasoning.

This also holds of Hersh. He says that “intuition is an essential part of mathematics” (Hersh 1997, 61). But by “intuition,” he “simply means guesses or insights attained by plausible reasoning, either fully conscious or partly subconscious” (Hersh 2014, 64). So “mathematical intuition is an application of conscious or subconscious heuristic thinking of the same kind that is used every day in ordinary life by ordinary people, as well as in empirical science by scientists” (ibid.).

In all philosophical tradition – from Plato and Aristotle to Kant, Bergson, Husserl, and Wittgenstein – intuition has been conceived of as immediate knowledge. For example, Wittgenstein says that “what one means by ‘intuition’ is that one knows something immediately” (Wittgenstein 1976, 30). Conversely, since by “intuition” Hersh simply means guesses or insights attained by plausible reasoning, for him intuition is not immediate knowledge. Therefore, Hersh does not mean “intuition” literally but only as a metaphor.

Mathematicians’ Proof, Truth, and Certainty

What is the relation of Hersh’s concept of mathematicians’ proof to truth and certainty?

Mainstream philosophy of mathematics holds that mathematics is about truth and certainty. Several mathematicians share this view. For example, Hilbert says that mathematics is “the paragon of truth and certitude” (Hilbert 1983, 191). Halmos says that mathematics is “Certainty. Truth” (Halmos 2008, 122). Byers says that mathematics “is a way of using the mind with the goal of knowing the truth, that is, of obtaining certainty” (Byers 2007, 327).

Hersh opposes this view, by arguing that mathematics cannot be said to be about truth, because “mathematicians’ proof does not guarantee truth” (Hersh 2014, 80). Nor mathematics can be said to be about certainty, because “mathematics is human, and nothing human can be absolutely certain” (ibid., 82).

I completely agree with Hersh on these points. Mathematics cannot be said to be about truth or certainty.

Mathematics cannot be said to be about truth. For, some mathematical theories have theorems that contradict each other. Then all of these theories cannot be said to be about truth, and there is no ground for saying that one of them is about truth. As Kline states, “mathematics is a body of knowledge. But it contains no truths. The contrary belief, namely, that mathematics is an unassailable collection of truths,” is “a popular fallacy” (Kline 1964, 9).

Mathematics cannot be said to be about certainty either. For, in order to say that mathematics is certain on the view that deductive proof is mathematicians’ proof, one should at least be able to prove by absolutely reliable means that the set of all propositions of any theory of established mathematics is consistent. But, by Gödel’s second incompleteness theorem, this is impossible. Similarly, in order to say that mathematics is certain on the view that axiomatic proof is mathematicians’ proof, one should at least be able to prove by absolutely reliable means that any of the formal systems for mathematics are consistent. But, by Gödel's second incompleteness theorem, this is impossible.

In fact, the concepts of truths and certainty did not arise within the context of mathematics or science but within that of religion. Thus, Pindar says that truth is "the daughter of Zeus" (Pindar, *Olympian odes* 10, 4). Bacchylides says that "truth is from the same city as the Gods; she alone lives with the Gods" (Bacchylides, Fragment 57). Sophocles says that certainty is the feature of the "infallible statutes of the Gods" (Sophocles, *Antigone* 454).

Gödel's Second Incompleteness Theorem and Certainty

Against the claim that, by Gödel's second incompleteness theorem, mathematics cannot be said to be certain, the following objections could be raised.

(1) The claim that, by Gödel's second incompleteness, mathematics cannot be said to be certain is unwarranted because, if mathematics cannot be said to be certain, then Gödel's second incompleteness theorem, being a mathematical result, cannot be said to be certain. But the claim that, by Gödel's second incompleteness, mathematics cannot be said to be certain depends on the assumption that Gödel's second incompleteness theorem can be said to be certain. Therefore, the claim that, by Gödel's second incompleteness, mathematics cannot be said to be certain is unwarranted.

This objection, however, is invalid because the claim that, by Gödel's second incompleteness, mathematics cannot be said to be certain does not depend on the assumption that Gödel's second incompleteness theorem can be said to be certain. It is a *reductio ad absurdum*, since it is of the following kind. Let us suppose, for argument's sake, that mathematics can be said to be certain. Then Gödel's second incompleteness theorem, being a mathematical result, can be said to be certain. But, by Gödel's second incompleteness theorem, mathematics cannot be said to be certain. This contradicts our assumption that mathematics can be said to be certain. Therefore, by *reductio ad absurdum*, we conclude that mathematics cannot be said to be certain.

(2) The claim that, by Gödel's second incompleteness theorem, mathematics cannot be said to be certain is unwarranted because "nothing in Godel's theorem in any way contradicts the view that there is no doubt whatever about the consistency of any of the formal systems" T "that we use in mathematics" (Franzén 2005, 105). Indeed, "if we have no doubts about the consistency" of T, then "there is nothing in the second incompleteness theorem to give rise to any such doubts. And if we do have doubts about the consistency" of T, then "we have no reason to believe that a consistency proof" for T "formalizable" in T "would do anything to remove those doubts" (ibid., 105–106). For, the consistency of T "is precisely what is in question" (ibid., 105).

This objection, however, is invalid. For, if we have no doubts about the consistency of T, we are rationally justified in having no such doubts only if we can prove by absolutely reliable means that T is consistent. But, by Gödel's second incompleteness theorem, this is impossible. On the other hand, if we do have doubts about the consistency of T, then the question is not whether a consistency proof for T formalizable in T would do anything to remove those doubts. The question is, instead, whether a consistency proof for T by absolutely reliable means would do anything to remove them, and the answer is yes, definitely.

Plausibility in Place of Truth and Certainty

Rather than about truth and certainty, mathematics is about plausibility, where a proposition is said to be plausible if it is compatible with the existing knowledge – in the sense that the arguments for it are stronger than the arguments against it, on the basis of the existing knowledge. (Plausible propositions correspond to what Aristotle calls *endoxa*; see Cellucci 2017, Chap. 9).

In order to know that a proposition is plausible, the following plausibility test procedure can be used:

(1) Deduce conclusions from the proposition.
(2) Compare the conclusions with each other, in order to see that the proposition does not lead to contradictions.
(3) Compare the conclusions with other propositions already known to be plausible, in order to see that the proposition is compatible with them.

Unlike truth, which is an absolute concept, plausibility is a relative concept – relative to the existing knowledge. For, as knowledge develops, new arguments for or against a proposition may be produced, which may increase or decrease its plausibility.

Moreover, unlike true propositions, which are certain, plausible propositions are not certain. For, there is no guarantee that no counterexample will ever be found. However, by Gödel's second incompleteness theorem, plausibility is the best we can achieve. To say it with Shakespeare, a proof of a proposition does not "so prove it | That the probation bear no hinge or loop | To hang a doubt on" (*Othello*, Act 3, Scene 3, vv. 364–366).

Warranted Assertibility

Instead of saying that mathematics is about plausibility, Hersh says that mathematics is about "'warranted assertibility', the pragmatist view of the logic of inquiry developed by John Dewey" (Hersh 2014, 51). Indeed, "'truth' in the sense of unqualified certainty is not available and not necessary" (ibid., 30). The "status of established mathematics" is "warranted assertibility" (ibid., 77). Established mathematics consists of warrantedly assertible propositions, namely, propositions that are "'warranted' by common consent based on shared experience" (ibid., 76). While truth is an absolute concept, warranted assertibility is a relative one. For, "as more convincing arguments for a mathematical statement are discovered, it becomes more strongly warranted. A deductive proof makes it part of established mathematics," and if the statement "becomes closely connected, both plausibly and rigorously to other established mathematics, then its warrant becomes stronger and stronger" (ibid., 81).

However, Hersh's statement that mathematics is about warranted assertibility, the pragmatist view of the logic of inquiry developed by John Dewey, is problematic. For, according to the pragmatist view of the logic of inquiry developed by John Dewey, "the end of inquiry" is the "attainment of knowledge, or truth" (Dewey 1938, 7). Now, if truth is the end of inquiry, then inquiry is directed toward it, so truth is the guiding principle of inquiry. In fact, Dewey states: "That which guides us truly is true" and "demonstrated capacity for such guidance is precisely what is meant by truth" (Dewey 2004, 90). But, if truth is the guiding principle of inquiry, then truth is strictly necessary. This conflicts with Hersh's other statement that "truth" is not available and not necessary.

Therefore, instead of saying that mathematics is about warranted assertibility, it seems more adequate to say that mathematics is about plausibility.

Hersh and Pólya

Hersh says that his "most helpful authority is George Pólya" (Hersh 2014, 61). In fact, both Hersh's distinction between the front and the back of mathematics and his view that deductive proof is mathematicians' proof are strongly connected to Pólya.

Indeed, Pólya distinguishes between the front and the back of mathematics, by saying that mathematics has two "aspects. Finished mathematics presented in 'finished' form appears as purely demonstrative, consisting of proofs only. Yet mathematics in the making resembles any other human knowledge in the making," it consists of "plausible reasoning" (Pólya 1954, I, vi).

Also, Pólya asserts that deductive proof is mathematicians' proof, by saying that proof is "a sequence of well-coordinated logical operations, of steps which start from the hypothesis and end in the desired conclusion of the theorem" (Pólya 1981, 123).

Pólya admits that deductive "proof is discovered by plausible reasoning" (Pólya 1954, I, vi). But he maintains that only deductive proof is demonstrative reasoning, and "we secure our mathematical knowledge by demonstrative reasoning" (ibid., I, v). Therefore, "the properly so-called logical type of reasoning," namely, deductive reasoning, "appears generally by itself on the pages of mathematical treatises; the heuristic reasoning," namely, the plausible reasoning, "which in general guided the invention of the logical reasoning is omitted" (Pólya 1941, 450).

This is because, according to Pólya, plausible reasoning is not part of the concept of proof. It does not serve to find hypotheses that cannot be deduced from established mathematics; it is only a heuristic means for finding deductions from established mathematics. Deductive proof is mathematicians' proof, because "we secure our mathematical knowledge by demonstrative reasoning" (Pólya 1954, I, v).

Nevertheless, despite the strong connection of Hersh to Pólya, between them there is a substantial difference. According to Pólya, "proof is definitive, it establishes irrefutably the truth of the theorem – once for all" (Pólya 1941, 450). Conversely, according to Hersh, "deductive proof" does "not establish anything as true" (Hersh 2014, 81). So, "Pólya is mistaken when he says that deductive proof renders a statement absolutely certain" (ibid., 82).

Problems with Deductive Proof as Mathematicians' Proof

As already anticipated, the view that deductive proof is mathematicians' proof is faced with serious problems.

(1) If deductive proof is mathematicians' proof, then it is impossible to prove propositions that cannot be deduced from established mathematics.

This conflicts, for example, with the fact that Cantor proved that there are sets of "size" different from those of the natural numbers and the real numbers. This could not be proved within the bounds of established mathematics. Proving it required hypotheses that not only could not be deduced from established mathematics but were fiercely opposed by established mathematicians. Thus, Kronecker said that Cantor was "a corrupter of youth" (Schoenflies 1927, 2). Schwarz said that Cantor's work was "a sickly confusion" and hoped that "one could succeed to occupy the unhappy young man with concrete problems, otherwise he will certainly come to a bad end!" (Schwarz 1967, 255–256). Hermite said that "the impression that Cantor's memoirs produce on us is desolating," since "it is impossible for us to find, among the results that can be understood, just one that possesses a real interest" (Hermite 1984, 209).

(2) If deductive proof is mathematicians' proof, then mathematicians are replaceable by computers completely.

For, there is an algorithm for enumerating all deductions from given premises. The algorithm can be said "to proceed like Swift's scholar, whom Gulliver visits in Balnibarbi, namely, to develop in systematic order, say according to the required number of inferential steps, all consequences and discard the 'uninteresting' ones" (Weyl 1949, 24).

So, if deductive proof is mathematicians' proof, then, as Turing points out, we can "imagine that all proofs take the form of a search through this enumeration for the theorem for which a proof is desired" (Turing 2004, 193). Admittedly, this is a time-consuming method to discover proofs, so in practice "we do not really want to make proofs by hunting through enumerations for them, but by hitting on one and then checking up to see that it is right. Nevertheless, this method is" in principle "replaceable by the longer method" (ibid., 212).

This means that, if deductive proof is mathematicians' proof, then mathematicians are replaceable by computers completely. Gowers even states that, "over the next hundred years or so, computers will" be "eventually supplanting us completely" (Gowers 2002, 134).

That, if deductive proof is mathematicians' proof, then mathematicians are replaceable by computers completely, indicates that the assumption that deductive proof is mathematicians' proof is problematic. For, already Gödel pointed out that, by his first incompleteness theorem, "it will never be possible to replace the mathematician by a machine, even if you confine yourself to number-theoretic problems" (Gödel 1986–2002, III, 164–165).

(3) If deductive proof is mathematicians' proof, then all of mathematical knowledge can ultimately be deduced from some elementary mathematical propositions, such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$.

For, if deductive proof is mathematicians' proof, then, tracing all the deductive chains all the way back, we will ultimately arrive at some propositions that cannot have been established by deductive proof themselves. They cannot have been postulated either, because deductive proof is not axiomatic proof. It only remains that they are the product of those rudimentary mathematical capacities that, according to cognitive science, all human beings innately possess as a result of biological evolution. Such capacities consist, first of all, of "the capacity to perform simple arithmetical calculations," such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$, "which may provide the foundations for the development of further arithmetical knowledge" (Wynn 1992, 750). In fact, Hersh states that this capacity provides "a bodily (neurological or biological) foundation for arithmetic" (Hersh 2014, 141). So, if deductive proof is mathematicians' proof, then all of mathematical knowledge can ultimately be deduced from some elementary mathematical propositions, such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$.

This contrasts with the fact that deductive rules are non-ampliative, so in a deductive proof, the conclusion is contained in the premises (see Cellucci 2017, Chap. 12). Thus, if deductive proof is mathematicians' proof, then all of mathematical knowledge is ultimately contained in some elementary mathematical propositions, such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$. This, however, is quite implausible.

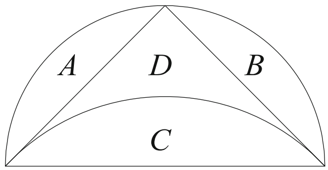
Prawitz's view of mathematicians' proof is faced with similar problems (see Cellucci 2017, Chap. 21).

Hippocrates of Chios' Quadrature of Lunules

Rather than on mathematics in finished form, the concept of proof must be based on mathematics in the making. Such was the original concept of proof in ancient Greece.

Our most important fragment from the pre-Euclidean geometry is a passage from Eudemus, which Simplicius declares to quote "word by word" (Simplicius, *In Aristotelis Physicorum Libros Quattuor Priores Commentaria*, I 2, 60.27). The passage gives a detailed account of Hippocrates of Chios' quadrature of four cases of lunules.

For example, in the first case, starting with an isosceles right triangle, Hippocrates draws on the hypotenuse both a semicircle and a segment of circle similar to those which appear on the two other sides of the triangle. (Similar segments of circle are those which are the same part of the circles, respectively.) So he obtains a lunule, the figure bounded by the semicircle, and the segment of circle.



Hippocrates wants to prove that "the lunule is equal to the triangle" (ibid., I 2, 62.6–7). Proving this is a problem to be solved. To solve it, by analyzing the conditions under which the problem would be solved, Hippocrates non-deductively arrives at the following hypothesis:

(I) "Similar segments of circle have the same ratios as the squares on their bases" (ibid., I 2, 61.6–7).

Hypothesis (I) is a sufficient condition for solving the problem. For, by the Pythagorean theorem, the square on the hypotenuse is equal to the sum of the squares on the other two sides. From this, by hypothesis (I), it follows that the segment on the hypotenuse is equal to the sum of the segments on the other two sides, that is, $C = A + B$. Then, lunule $= A + B + D = C + D =$ triangle. This solves the problem. Thus "the lunule, having been proved equal to the triangle, can be squared" (ibid., I 2, 62.7–8).

Hypothesis (I) is plausible. For, it is compatible with the existing knowledge. But (I) is itself a problem that must be solved. To solve it, by analyzing the conditions under which the problem would be solved, Hippocrates non-deductively arrives at the following hypothesis:

(II) "The squares on the diameters have the same ratios as the circles" (ibid., I 2, 61.8–9).

Hypothesis (II) is a sufficient condition for solving the problem (I). For, similar segments are those which are the same part of the circles, respectively. Hence, as the circles are to one another, so also are similar segments of them. From this, by hypothesis (II), it follows (I). This solves the problem. The solution to the problem (I) increases the plausibility of hypothesis (I).

Hypothesis (II) is plausible. For, it is compatible with the existing knowledge. But (II) is itself a problem that must be solved. To solve it, it is necessary to formulate another hypothesis. And so on, *ad infinitum*.

Analytic Proof

The concept of proof underlying Hippocrates of Chios' quadrature of four cases of lunules is that of analytic proof, which can be described as follows.

A proof consists, first, in a non-deductive derivation of a hypothesis from the problem and possibly other data already available. The hypothesis must be a sufficient condition for a solution to the problem, namely, such that a solution to the problem can be deduced from it. Moreover, the hypothesis must be plausible, namely, compatible with the existing knowledge.

The proof consists, then, in a non-deductive derivation of a new hypothesis from the previous hypothesis and possibly other data already available. The new hypothesis must be a sufficient condition for a solution to the problem posed by the previous hypothesis. Moreover, the new hypothesis must be plausible. That the new hypothesis leads to a solution to the problem posed by the previous hypothesis increases the plausibility of the latter.

And so on, *ad infinitum*, since mathematical research is a perpetually ongoing process. Indeed, as Poincaré says, there are not "solved problems and others which are not; there are only problems more or less solved," but "it often happens however that an imperfect solution guides us toward a better one" (Poincaré 2013, 377–378).

The concept of analytic proof is the concept of proof underlying not only Hippocrates of Chios' quadrature of four cases of lunules but also Hippocrates of Cos' solutions to medical problems (see Cellucci 2013a, Chap. 4). However, although Hippocrates of Chios and Hippocrates of Cos both used the concept of analytic proof, they did not write down a formulation of it. A formulation of the concept of analytic proof, at least in its bare essentials, was first given by Plato (see Cellucci 2017, Chap. 12).

The concept of analytic proof is based on a view of knowledge that also goes back to Plato. As Natorp says, according to it, "scientific knowledge is an infinite process" in which, "beyond every (relative) beginning, we must look for a prior beginning" (Natorp 1910, 13–14). Every such new "beginning leads to wider and deeper developments" (ibid., 15). This opens the way to an "unlimited deepening of the problem" (ibid., 16). Thus, "within the sciences, we can speak of an infinite backward path from hypotheses to ever more fundamental hypotheses" (ibid., 15).

Some Features of Analytic Proof

Analytic proof has several features. Here are some of them (for other ones, see Cellucci 2017, Chaps. 12 and 21):

(1) Unlike deductive proof, which only involves a downward path from established mathematics to the proposition deduced from it, analytic proof involves both

an upward path, from the problem to plausible hypotheses that are sufficient conditions for its solution, and a downward path, from plausible hypotheses to the problem.

(2) The purpose of analytic proof is to discover hypotheses that are sufficient conditions for a solution to the problem and are plausible. So, analytic proof is both a means of discovery and a means of justification. It is a means of discovery, because it is intended to discover hypotheses that are sufficient conditions for a solution to the problem. It is a means of justification, because it is intended to discover hypotheses that are plausible and hence to show that the solution is deduced from plausible hypotheses.

(3) Unlike deductive proof, which can only make use of hypotheses that can be deduced from established mathematics, analytic proof can make use of hypotheses that cannot be deduced from established mathematics. For, the hypotheses for a solution to the problem are obtained by non-deductive rules, and non-deductive rules are ampliative, so they can produce hypotheses that cannot be deduced from established mathematics. (On the ampliativity of non-deductive rules, see Cellucci 2013a, Chaps. 17 and 18.)

(4) Intuition, in the sense of all philosophical tradition, plays no role in analytic proof. Indeed, intuition plays no role in the discovery of hypotheses, because they are obtained from the problem, and possibly other data, by non-deductive rules, so not by intuition but by inference. Intuition plays no role in the justification of hypotheses, because their plausibility is established by comparing the arguments for them and the arguments against them on the basis of the existing knowledge, so not by intuition but by inference.

(5) Analytic proof is as rigorous as deductive proof. Deductive proof shows that the proposition can be deduced from propositions which are not true but only warrantedly assertible. Analytic proof shows that a solution to the problem can be deduced from hypotheses which are not true but only plausible.

Analytic Proof as Mathematicians' Proof

Analytic proof is mathematicians' proof. This view of proof is not subject to the problems of Hersh's view that deductive proof is mathematicians' proof.

(1) If analytic proof is mathematicians' proof, then it is possible to prove propositions that cannot be deduced from established mathematics.
For, the hypotheses for proving a proposition are obtained by non-deductive rules, and non-deductive rules are ampliative, so they can produce hypotheses that cannot be deduced from established mathematics. Therefore, the hypotheses can permit to prove propositions that cannot be deduced from established mathematics.

(2) If analytic proof is mathematicians' proof, then mathematicians cannot be replaced by computers completely.

For, there is no algorithm capable of producing hypotheses that are sufficient conditions for the solution to a problem and are plausible. Producing such hypotheses may involve creating an entirely new framework, which essentially goes beyond the bounds of established mathematics.

(3) If analytic proof is mathematicians' proof, then not all of mathematical knowledge can ultimately be deduced from some elementary mathematical propositions, such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$.
For, the mathematical problems that are being solved take usually the form of universal propositions, whereas no universal proposition can be deduced from particular propositions, such as $1 + 1 = 2$ or $2 + 1 = 3$ or $2–1 = 1$.

The view that analytic proof is mathematicians' proof is shared by Friend, who argues that not only "all proofs can be thought of as analytic, including rigorous proofs" (Friend 2014, 208). But also, "all proofs are better viewed as analytic" (ibid., 213).

Hersh's Objection to Analytic Proof

Against the view that analytic proof is mathematicians' proof, Hersh objects that a proposition arrived at plausible reasoning, namely, by non-deductive rules, is an unproved conjecture. For, "a plausible argument" is "not demonstrative," so "it fails to show that" the result "is rigidly connected to established mathematics" (Hersh 2014, 58). A plausible argument "does not establish the result" even if the latter "has been verified in millions of individual cases" (ibid., 82). Therefore, the result of a plausible argument is not a proved theorem but only an unproved conjecture. Now, "the distinction between a proved theorem and an unproved conjecture is the central, characteristic feature of mathematics, as practiced from Euclid to this day" (ibid., 83).

This objection, however, is based on the assumption that, according to the view that analytic proof is mathematicians' proof, hypotheses – and hence the result deduced from them – are justified by the non-deductive rules by which hypotheses are arrived at. But this assumption is unwarranted. Although, according to the view that analytic proof is mathematicians' proof, hypotheses are arrived at by non-deductive rules, they are not justified by such rules but only by the fact that they are plausible. And establishing that they are plausible requires a special argument, such as the plausibility test procedure described above.

Thus, the hypotheses used in an analytic proof, and hence the result deduced from them, are not unproved conjectures, but plausible propositions, just as the propositions from which a deductive proof starts, and hence the result deduced from them, are not unproved conjectures but warrantedly assertible propositions. Therefore, it would be unjustified to say that analytic proof does not establish the result and only deductive proof establishes it. A result established by deductive proof can only be warrantedly assertible, just as a result established by analytic proof can only be plausible.

Normal Mathematics and Revolutionary Mathematics

This does not mean that between the view that analytic proof is mathematicians' proof and the view that deductive proof is mathematicians' proof there is no essential difference. Their difference can be appreciated in terms of the distinction between normal mathematics and revolutionary mathematics.

Normal mathematics is mathematical research which does not require introducing hypotheses that cannot be deduced from established mathematics.

Revolutionary mathematics is mathematical research which requires introducing hypotheses that cannot be deduced from established mathematics, and tie together disparate areas, or open new areas of mathematics.

Normal mathematics is very extensive, being the kind of research most mathematicians do all the time. Revolutionary mathematics is much more limited, since it primarily consists of turning points in mathematics. (For other views of normal and revolutionary mathematics, see the papers in Gillies 1992).

The view that deductive proof is mathematicians' proof may be adequate for normal mathematics but is inadequate for revolutionary mathematics, which requires introducing hypotheses that cannot be deduced from established mathematics.

Conversely, the view that analytic proof is mathematicians' proof is adequate, not only for normal mathematics but also for revolutionary mathematics, because in analytic proof, the hypotheses for a solution to the problem need not be deducible from established mathematics.

The Inexhaustibility of Mathematics

Like many mathematicians, Gödel assumes that axiomatic proof is mathematicians' proof. For, he states that "in whatever way" mathematics, "or any part of it, is built up, one always needs certain undefined terms and certain axioms (i.e., deductively unprovable assertions) about them" (Gödel 1986–2002, III, 346).

On this basis, Gödel maintains that his incompleteness theorems establish what "might be called the incompletability or inexhaustibility of mathematics" (ibid., III, 305). Specifically, it is his second incompleteness theorem that "makes the incompletability of mathematics particularly evident. For, it makes it impossible" for us "to set up a certain well-defined system of axioms and rules and consistently make the following assertion about it: All of these axioms and rules I perceive (with mathematical certitude) to be correct, and moreover I believe that they contain all of mathematics" (ibid., III, 309). This means that "no well-defined system of correct axioms can contain all of mathematics," in the sense of "all true mathematical propositions" (ibid.).

Thus, assuming that axiomatic proof is mathematicians' proof, Gödel interprets the inexhaustibility of mathematics negatively, as meaning that no well-defined system of correct axioms can contain all of mathematics.

Alternatively, assuming that analytic proof is mathematicians' proof, we can interpret the inexhaustibility of mathematics positively, as meaning that mathematical knowledge is an infinite problem-solving process in which, beyond every hypothesis, one looks for deeper hypotheses. Every new, deeper hypothesis makes one see the problem with new eyes. Proust even says that "the only true voyage" of discovery is not "to go towards new landscapes, but to have other eyes" (Proust 1947, 69). Looking for ever-deeper hypotheses leads to an unlimited deepening of the problem.

Analytic Proof and Explanation

What is the role of proof in research? According to Hersh, "in research its role is to convince" (Hersh 2014, 153). For, a proof is "a convincing argument, as judged by qualified judges" (ibid., 147). This means that the role of proof in research is quite different from the role of proof in the classroom, which is not to convince but "to explain" (ibid). Indeed, "in the classroom, convincing is no problem. Students are all too easily convinced" (ibid., 153). Instead, "what a proof should do for the student is provide insight into why the theorem is true" (ibid.). The "educational value of proof is the value of complete explanation" (ibid., 155).

This, however, contrasts with the view of several mathematicians, who think that the role of proof in research is not to convince but to explain. Thus, Gleason says that "proofs really aren't there to convince you that something is true – they're there to show why it is true" (Albers et al. 1990, 86). That is, they are there to explain.

In particular, an analytic proof shows the researcher why the solution to the problem holds. It can do so because the solution is based on hypotheses that are obtained from the problem, so they are strictly connected to it. Hence the hypotheses may reveal aspects of the problem that are essential to the solution. By showing the researcher why the solution to the problem holds, an analytic proof does not merely convince; it explains. This marks an essential difference between analytic proof, on the one hand, and deductive or axiomatic proof, on the other hand (see Cellucci 2008).

Analytic Proof and Diagrams

From its Greek beginnings, mathematics has made essential use of diagrams in proofs. This can even be seen as representative of Greek civilization. Thus

Aristippus, being shipwrecked and cast ashore on an unknown and possibly hostile shore, "started being reassured when he observed, traced on the sand, geometrical figures: for, he thought he must have landed among Greeks, among wise men, not among barbarians" (Galen 1964–1965, I, 8).

Conversely, in the twentieth century, the use of diagrams in proofs has been generally viewed as non-rigorous and alien to the concept of proof. Thus, Hilbert maintains that "the theorem is only proved when the proof is completely independent of the figure" (Hilbert 2004, 75).

However, this negative attitude is contradicted by its very proponents. Indeed, even the very first proof in Hilbert's *Grundlagen der Geometrie* makes an essential use of properties obtained from the figure (see Cellucci 2017, Chap. 19). Therefore, Hersh is quite right in saying that "a proof can be words only, of course. It can be, as in Euclid, words and diagrams. Or it can" be "diagram only" (Hersh 1997, 186).

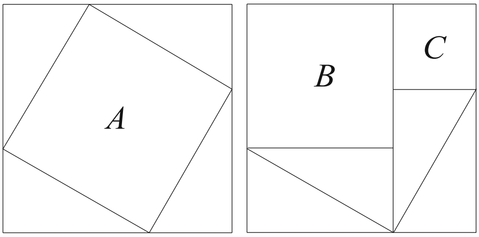
In particular, the use of diagrams is perfectly justified from the point of view of the analytic concept of proof. In terms of it, a proof consists, first, in a non-deductive derivation of a hypothesis from the problem and possibly other data already available, which may include data acquired from a diagram. The hypothesis must be a sufficient condition for a solution to the problem. Moreover, the hypothesis must be plausible.

The proof consists, then, in a non-deductive derivation of a new hypothesis from the previous hypothesis and possibly other data already available, which may include data acquired from a diagram. The new hypothesis must be a sufficient condition for a solution to the problem posed by the previous hypothesis. Moreover, the new hypothesis must be plausible. That the new hypothesis leads to a solution to the problem posed by the previous hypothesis increases the plausibility of the latter.

And so on, *ad infinitum* because, as Poincaré says, there are not solved problems but only problems more or less solved.

The Pythagorean Theorem and Mathematical Diagrams

As an example of use of diagrams in proofs, suppose we want to prove the Pythagorean theorem: The square on the hypotenuse of a right triangle is equal to the sum of the squares on the other two sides. Proving this is a problem to be solved. There is no positive evidence on Pythagoras' solution, but, according to Bretschneider, it could have been based on "the method of dissection of plane figures" (Bretschneider 1870, 82). In keeping with this suggestion, we may suppose a diagram like the following one.



Subtracting the four equal right triangles from each of the two figures, we obtain equal areas; thus $A = B + C$. Now, B and C are the squares on the other two sides of the right triangle. So, to solve the problem, we need only show that A is a square – the square on the hypotenuse of the right triangle. To this aim, by analyzing the conditions under which the problem would be solved, we non-deductively arrive at the following hypothesis:

(I) The sum of the angles of a right triangle is equal to two right angles.

Hypothesis (I) is a sufficient condition for solving the problem. For, by hypothesis (I), the sum of the two non-right angles of the right triangles is a right angle. Therefore each angle of A is a right angle; hence A is a square. This solves the problem.

Hypothesis (I) is plausible. For, it is compatible with the existing knowledge. But (I) is itself a problem that must be solved. To solve it, by analyzing the conditions under which the problem would be solved, we non-deductively arrive at the following hypothesis:

(II) A right triangle is half a rectangle.

Hypothesis (II) is a sufficient condition for solving the problem. For, the sum of the angles of a rectangle is equal to four right angles. Hence, by hypothesis (II), the sum of the angles of a right triangle is equal to two right angles. This solves the problem. The solution to the problem (I) increases the plausibility of hypothesis (I).

Hypothesis (II) is plausible. For, it is compatible with the existing knowledge. But (II) is itself a problem that must be solved. To solve it, it is necessary to formulate another hypothesis. And so on, *ad infinitum*.

An Objection to the Distinction Between Front and Back

On the basis of the concept of analytic proof, we can discuss an objection that has been raised against Hersh's distinction between the front and the back of mathematics.

Greiffenhagen and Sharrock argue that such distinction "downplays the continuity of the two" (Greiffenhagen and Sharrock 2011, 841). The continuity is clear from a comparison between mathematical lectures as one example of mathematics in the "front," and "meetings between a supervisor and his doctoral students" as "one example of mathematics in the 'back'" (ibid., 854). The comparison shows that "the difference between the ‘front’ and the ‘back’” is “not between two kinds of proof” but only “between different stages: of working with an incomplete idea of a possible proof as opposed to presenting a (presumably) complete, thoroughly worked-out proof” (ibid., 858). The “‘finished’ product in the ‘front’ is a later stage and product of the ‘currently unfinished’ work in the ‘back’” (ibid., 841). So, “it should not be expected that increased familiarity with what goes on ‘in the mathematical back’ will lead to any significant revision of understanding of what is on show ‘out front’” (ibid., 861).

This objection, however, is based on the assumption that meetings between a supervisor and his doctoral students are one example of mathematics in the back. The assumption is apparently motivated by Hersh’s assumption that the back of mathematics is mathematics as it appears among working mathematicians, in informal settings, told to another in an office behind closed doors. The assumption is unjustified for the very same reason for which Hersh’s assumption is unjustified. The back of mathematics is not currently unfinished work; the latter is just a preliminary, incomplete version of the finished product. As already said, the back of mathematics is, instead, the creative work of the mathematician, primarily the discovery work.

This entails that between the front and the back, there is really discontinuity. As Grothendieck says, the creative work of the mathematician “is not reflected virtually to any extent in the texts or talks that are intended to present such work,” whether “textbooks and other didactic texts, or articles and original memoirs, or oral courses and seminar presentations, etc.” (Grothendieck 1985, 84). That is, the creative work of the mathematician is not reflected virtually to any extent in the finished product. On the other hand, currently unfinished work is just a preliminary, incomplete version of the finished product. Therefore, the creative work of the mathematician is not reflected virtually to any extent in currently unfinished work.

Contrary to what Greiffenhagen and Sharrock maintain, the difference between the front and the back is not merely a difference between different stages of proof development but is really a difference between two kinds of proof: deductive or axiomatic proof, the front, and analytic proof, the back. Analytic proof is not the working with an incomplete idea of a possible proof as opposed to a complete, thoroughly worked-out, deductive or axiomatic proof. On the contrary, deductive or axiomatic proof is a curtailment of analytic proof that cuts off its crucial part, the upward path from the problem to hypotheses – the process of discovery of hypotheses – only leaving the routine part, the downward path from hypotheses to the problem.

Also, it is unjustified to say that it should not be expected that increased familiarity with what goes on in the back will not lead to any significant revision of understanding of what is on show “out front.” As already stated above, an analytic proof does not merely convince; it explains. So it provides that kind of understanding which can be essential to the growth of mathematics (see Cellucci 2017, Chap. 22).

The Nature of Mathematical Objects

In the twentieth century, many people, noticeably supporters of foundationalism, have maintained that mathematics is about objects that have some sort of existence and whose knowledge ultimately depends on intuition. This view has serious shortcomings (see Cellucci 2017, Chap. 19). However strange it may sound to say so, a suitable alternative is offered by Plato.

Admittedly, Plato says that mathematical objects have a kind of reality that "is always the same, ungenerated and imperishable," it "is not visible nor perceptible by any sense, and only intuition has been granted to contemplate it" (Plato, *Timaeus*, 52 a 1–4). This and other similar Plato's statements are at the origin of "mathematical platonism," the view that mathematics is about a nonphysical reality which exists independently of the human mind.

But Plato also says that mathematicians, as long as they are alive, cannot contemplate this nonphysical reality by intuition. For, "as long as we have the body and our soul is contaminated by such an evil, we will never adequately gain the possession of what we desire, and that, we say, is truth" (Plato, *Phaedo*, 66 b 5–7). Only after death, mathematicians can behold "the happy vision and contemplation" of the reality in question, having "calm and happy visions, in pure light" (Plato, *Phaedrus*, 250 b 8–c 4).

Since mathematicians, as long as they are alive, cannot contemplate the non-physical reality that is the object of mathematics by intuition, to solve mathematical problems, they introduce certain mathematical objects as hypotheses.

Indeed, Plato states that, to solve a mathematical problem, mathematicians must "make use of a hypothesis" (Plato, *Meno*, 86 e 3). Thus, "those who deal with geometry, arithmetic, and the like hypothesize the odd and the even, geometrical figures, the three kinds of angles, and other things akin to these, depending on the subject of their investigation. They take these as known and assume them as hypotheses" (Plato, *Republic*, VI 510 c 2–6). So they arrive at knowing the properties of these objects, not by intuition but by making hypotheses, hence by discursive thought. Thus, it is "discursive thought, not intuition, the state of geometry and the like, namely thought intermediate between opinion and intuition" (ibid., VI 511 d 2–5).

Mathematical objects are hypotheses mathematicians make to solve problems by analytic proof. When such objects are seen to solve the problems for which they have been made, they become objects of study themselves. Being hypotheses mathematicians make, mathematical objects exist only in the minds of the mathematicians who hypothesize them and in the minds of the people who make use of them.

Hersh agrees that mathematical objects "are right here, in our individual minds, shared also with many other individual minds," so their reality "is mental-cultural" (Hersh 2014, 90–91). To this, however, it must be added that mathematical objects are hypotheses mathematicians make to solve problems by analytic proof. (For more on this, see Cellucci 2017, Chap. 19).

Conclusion

My few points of disagreement with Hersh, outlined above alongside several points of agreement, do not reduce my indebtedness to him.

While being originally a mathematical logician, from the very beginning I have been aware of the inadequacy of mainstream philosophy of mathematics. In particular, I have been familiar with Lakatos' views, also from personal contacts, because Lakatos was the external examiner for the undergraduate degree in philosophy and mathematics at the University of Sussex, where I was a lecturer in mathematical logic in 1969–1971.

I first expressed in print my dissatisfaction with mainstream philosophy of mathematics in Cellucci (1982–1983). Soon after, I became aware of Hersh (1979) and Davis and Hersh (1981), which I found very stimulating even, or even especially, when I did not completely agree with them. This greatly helped me to develop my own variety of maverick philosophy of mathematics.

However, by being opposed to mainstream philosophy of mathematics, the fragile young twig of maverick philosophy of mathematics planted by Lakatos has been exposed to storming attacks by mainstream philosophers of mathematics.

Thus, Feferman states that "Lakatos' fireworks" only "briefly illuminate limited portions of mathematics" (Feferman 1998, 93). Contrary to Lakatos' assumption, "the creative and intuitive aspects of mathematical work evade logical encapsulation" (ibid., 178). The "mathematician at work relies on surprisingly vague intuitions and proceeds by fumbling fits and starts with all too frequent reversals. In this picture the actual historical and individual processes of mathematical discovery appear haphazard and illogical" (ibid., 77). Therefore Lakatos' view, that the philosophy of mathematics must be primarily concerned with methodology qua logic of discovery, is unjustified. Only mathematical "logic gives us a coherent picture of mathematics," it "alone throws light on what is distinctive about mathematics, its concepts and methods" (ibid., 93). One can even use its formal "systems to model growth and change" (ibid., 92).

But this overlooks that mathematical logic is incapable of accounting for the real mathematical process (see Cellucci 2013a).

The fragile young twig of maverick philosophy of mathematics has also been exposed to premature declarations of failure by philosophers of mathematical practice.

Thus, Mancosu states that "the 'maverick tradition' has not managed to substantially redirect the course of philosophy of mathematics. If anything, the predominance" of mainstream "approaches to the philosophy of mathematics in the last twenty years proves that the maverick camp did not manage to bring about a major reorientation of the field" (Mancosu 2008, 5). For, mainstream philosophers of mathematics "felt that the 'mavericks' were throwing away the baby with the bathwater" (ibid., 6).

But this overlooks that old ideas are hard to die, even when they have been proved to be inadequate. Somewhat cynically, Planck says that a new theory "does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Planck 1950, 33–34).

Nevertheless, the fragile young twig of maverick philosophy of mathematics could thrive and bear fruit only if grafted carefully upon the old stem of philosophical tradition. Hersh has decisively contributed to this process by showing that the way opened by Lakatos could be further pursued, reconnecting the philosophy of mathematics to the general question of human knowledge. So "at last, in the 21st century, the 'maverick'" tradition "arrived as a legitimate theme of philosophical investigation" (Hersh 2014, 73).

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Does Reason Evolve? (Does the Reasoning in Mathematics Evolve?)

Jody Azzouni

1 What Is Mathematics, Really? Two (Families of) Positions

Hersh (1997) in a book aptly named *What Is Mathematics Really?* stresses the great distance he detects between the reality of professional mathematical practice—contemporary and historical—and the reasoning in formal languages that philosophers (since Frege) have largely characterized mathematical proof in terms of. Hersh criticizes the reasoning-in-formal-languages view of mathematical practice and mathematical proof as "isolated," "timeless," "ahistorical," and indeed, even "inhuman." Hersh (1997, xi) contrasts this derivation-centered view of mathematics (and mathematical proof) with an alternative view that takes mathematics to be a human activity and a social phenomenon, one which historically evolves and is intelligible only in a social context. His alternative view pointedly roots mathematical practice in the *actual* proofs that mathematicians create—actual proofs that Hersh claims philosophers of mathematics often ignore.

I've just indicated two extreme positions (among a family of more moderate ones that are possible). Extreme positions offer clarity—so I'll start by exploring the first one. Call it "the derivationist account." The derivationist account characterizes strict derivations (in one or another artificial language, such as a first-order predicate one) as the only genuine proofs. Genuine proofs, that is, are sequences of formulas of an artificial language, each of which is either an axiom (drawn from a set of axioms characterizing a particular mathematical subject matter) or a formula that recognizably follows from ones earlier in the sequence by the application of one or more logical rules. If the nonlogical axioms and the rules of the background logic are recursive, then derivations are mechanically recognizable. The derivationist account takes the mechanical recognizability of derivations to explain one widely

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noticed peculiar quality of mathematical proof: professionals convince one another of the validity of their proofs (subject, of course, to the time limits of successful surveyability that the lengths of such items pose).[1]

This means, notice, that the derivationist account takes formal derivations to play an epistemic role in mathematical practice. The existence of formal derivations *explains* an otherwise puzzling aspect of that practice: why mathematicians are so agreeable to one another—comparatively speaking. In order to explain this agreeableness of mathematicians, notice further that derivations can't merely exist isolated somewhere or other in Platonic heaven. Mathematicians must actually *have access to* the pertinent properties of derivations in ways that are reflected in mathematical practice. In particular, that access must be evidentially relevant to their recognition of (for example) the validity of theorems in order for this explanation of mathematical agreement to fly.

Three important observations about the flexibility of the derivationist account. First, having "access to the pertinent properties of such derivations" doesn't require those derivations to be Platonic objects or, indeed, to be realities of any sort. Derivations can be virtual objects—nonexistent *projections* of the (shared) cognitive faculties of mathematicians. According to this view of the ontology of derivations, we explain the mathematician's grasp of validity in terms of psychological changes or sequences of thoughts (conscious or unconscious) that we *code* in terms of "manipulations" of those projections. So the phrase, "the existence of formal derivations *explains* ...," used above shouldn't be understood to be ontologically committing. The derivationist account is compatible—pretty much—with any ontological position about mathematical objects: nominalistic denials of the existence of abstracta altogether, views (like Hersh's) that take such objects to be historically evolving social entities of some sort or claims of the traditional Platonic sort. Ontology is largely irrelevant to the issues of this paper, so I'm going to sideline the topic here.[2]

Second, no assumption is needed that the mathematician has psychological access to the needed derivations in the sense that she can actually *carry out* transformations of informal rigorous proofs into their formal analogues or that she has an awareness of these transformations. The derivationist account can accommodate the fact that professional mathematicians are often unaware or ignorant of formal derivations, provided the practice of informal rigorous mathematical proof that

[1]Contrast, for example, discussions of which theorems have (and haven't) been established in various branches of mathematics—standard and nonstandard—with similar discussions of what implications have (and haven't) been established about one or another political policy (or moral theory). Another area where disagreements are notably largely absent (compared to other areas of discourse) is rule-governed games. It's relatively straightforward to tell when the rules of a game have or haven't been followed correctly. This, as I indicate in the course of this paper, is no accident.

[2]See Azzouni (2013a) for extensive discussion of natural languages (and by extension, of artificial languages) from a strictly nominalist perspective—that is, from a perspective that treats such languages as collective psychological projections.

mathematicians do engage in contains the tools for transforming such informal proofs into corresponding formal derivations. The idea is to treat the distance between formal derivations and real-world professional proofs as similar to the distance between the actual utterances of a natural language and the grammatical sentences posited in linguistics. Although in practice, and almost without exception, we speak in broken ungrammatical phrases (that are understood only because speakers and listeners share contexts of utterances), transformations of "what we've said" into impeccable grammatically correct sentences are possible, although they aren't within reach of all speakers, and perhaps they're only within reach of appropriately trained speakers.

On this version of the derivationist account, local context-specific criteria for validity all turn out to be various rules of thumb and other abbreviatory shortcuts (included is the common knowledge among specialists that specific kinds of proof procedures are widely used) that together indicate how informal rigorous proofs can be transformed into derivations. The derivationist account—so construed—allows that the same thing is true of grammatically impeccable sentences and formal derivations. The various local context-specific criteria that license ungrammatical (but acceptable) utterance fragments and the various local context-specific criteria that license informal rigorous mathematical proofs are both linked (respectively) to grammatical-impeccable sentences and to formal derivations in ways practitioners are barely conscious of.[3]

Third, the derivationist account can also handle the open-endedness of standard mathematical topics due to Gödel's theorem—that, for example, the set of truths of the standard model of the natural numbers outstrips any (recursive) axiomatization of number theory. The derivationist account treats "number theory"—when studied by professional mathematicians over time—as individuated by an (open-ended) family of axiom systems which are conservative extensions of one another.[4]

Assuming that the appropriate access of mathematicians to derivations can be established (but acknowledging that this is an important promissory note—I discuss it further in sections 8 and 9), the widely recognized "objectivity" of mathematics can be explained by the nature of formal derivations. Given a set of axioms and a presumed background logic, the consequences of these axioms are matters purely of that logic. *Apart from mistakes*, there is no space for disagreement. Finally, formal

[3]I was tempted by a position like this—in large part because of this analogy—until in 2005, when I realized that all the ways I was offering to bridge the gap between formal derivations in proprietary vocabulary (e.g., ZFC) and informal rigorous mathematical proofs weren't likely to make formal derivations accessible to mathematicians in the ways needed for those derivations to explain the aspects of mathematical practice that the derivationist account needs to explain (see my discussion of this in Azzouni (2009a), especially section 16). I revisit this issue in section 9.

[4]By "open-ended," I mean that the axioms from this family of formal systems, collectively speaking, aren't recursively enumerable; if they were (and because the resulting systems are conservative extensions of one another), the axioms from these families of axiom systems could be coded into a single axiom system. Rather, they are found, when they are found, not by "a uniform process," but by "essentially new methods." Here I'm borrowing Turing's language (Turing (1936, 139)). See the interesting discussion of this in Copeland and Shagrir (2013).

derivations (and the mathematician's access to them) are used to explain the long pedigree of many mathematical results: that they have *eternal* shelf lives. This isn't merely a matter of robustness; rather, apart from mistakes, a mathematical result, once established, is a keeper.

I'll describe how the derivationist account explains aspects of mathematical practice in terms that I'll borrow from Tanswell (2015, 297-298): the derivationist account is taken to explain—at least in principle—what he calls, *Rigor*, *Correctness*, *Agreement*, *Content*, and *Techniques*. That is, the following properties are to be explained (at least in part) by the access of mathematicians to formal derivations: first, the standards of rigor of informal rigorous mathematical proofs; second, the correctness (and incorrectness) of such proofs; and third, that mathematicians largely agree upon perusing candidate informal rigorous informal proofs about whether they're suitably rigorous and whether they're correct. Next, the account must explain how the content of an informal proof determines which formal proof(s) it indicates, and (finally) that account must explain the role of informal techniques of proof that aren't obvious transcriptions of formally licensed inference steps.

I wrote, "at least in part," because the derivationist account can help itself to many of the properties that have been noticed about informal rigorous proofs by opponents of that account. For example, the tacit shared knowledge in a particular mathematical context (a particular branch of topology, say) might be crucial to seeing that an informal proof procedure is correct and to supplying a standard about what can be explicitly stated and what can be left tacit in proofs. And, what's taken to be tacit shared knowledge (and therefore not worth bothering to articulate) can change over time for purely sociological reasons. But when the proof is appropriately filled out to explicitly include this context-dependent tacit material, it can be seen how the content of the (filled-out) proof determines which formal derivation(s) it indicates. The role of the informal techniques of proof in this specific area can also be explained as being abbreviations of certain sorts, or content-specific inference patterns which, again, when spelled out explicitly, make clear which formal derivation(s) are indicated.

2 Inference as Action; Objects of Inferential Actions

It's been suggested more than once (e.g., in conversation with me—but in print as well) that the data of mathematical practice isn't as I've described it in the last section. And so, *Agreement* (for example) *isn't* an aspect of mathematical practice that needs explaining by any account, let alone the derivationist account. Not only have the standards of mathematical proof mutated over the ages (so it's claimed), not only are proofs refined and developed over time as standards change, but there have even been heated disputes (by academic standards anyway) over appropriate proof methods. Take as an illustration the loud row between British and Continental mathematicians in the eighteenth century over the notation for the calculus, or for another example, the more recent debate between proponents of classical mathematics and their intuitionistic opponents.[5] Furthermore, once it's recognized how large a role diagrammatic reasoning plays in informal mathematical proof, it can look like over time *methods of reasoning evolve in mathematics*.

The italicized phrase of the last paragraph doesn't explicitly occur in Larvor (2012), but the evolving-methods-of-reasoning-position itself does (and it's something that's believed by a number of philosophers of mathematics). Larvor argues that mathematical reasoning is best treated as "inference as action"; he argues further that such a position has significant consequences for our understanding of (informal) mathematical proof. He writes (723),

> The benefit of viewing inference as action is that we can see how the subject-matter of informal arguments shapes and contributes to inferences. Indeed, instead of two highly abstract categories, the form of an argument and its content, we now have an indicative list of many and various concrete objects of inferential action (diagrams, models, expressions in special notations, experimental set-ups and so forth). This goes some way towards answering (or at least, making more precise) our question about which activities to count as mathematical practice. The cost is that we have to abandon the hope of establishing a general test for validity.

De Toffoli and Giardino (2014, 333), commenting on this passage in light of their previous discussion of Rolfsen's picture proof (with words) of the equivalence of surgery codes and Heegaard diagrams of the Poincaré homology sphere, write:

> In Rolfsen's proof, we saw that among the permissible actions on the pictures are continuous transformations. These are part of the background material in the sense that any topologist knows immediately that these transformations can be interpreted in terms of homeomorphisms. The validity is thus based on the "practice": it is the practice itself that integrates a way of controlling the actions on the representations used, which results in the establishment of local criteria for validity. The responsibility is shared among experts: since in low-dimensional topology different forms of reasoning are employed, some of which are specific to it, purely external criteria of validity cannot exhaust all the criteria actually adopted. As Brown suggests, we should acknowledge the existence of non-formal reasoning in mathematics: "first-order logic may be well understood, but what passes for acceptable proof in mathematics includes much more than that" (Brown 1999, p. 164). If this is true, then, as Larvor has exhaustively discussed, "the cost is that we have to abandon the hope of establishing a general test for validity." (Larvor 2012, p. 723)

So (modulo certain differences among the thinkers here), a second position has emerged, one that starts from the assumption that inferences are actions; on this view, mathematics—mathematical proof, specifically—evolves. In contrast to the—once and for all—establishing of (a particular) logic as the backbone of all reasoning, or at least, of all reasoning in mathematics, the counterposition is that reason itself evolves. (At least it does in mathematics; but if it does in mathematics, then surely it does so everywhere else.) *Reasoning itself*, borrowing language from Hersh, is a social phenomenon which historically changes with the passage of time;

[5]See, e.g., Gilles (2013, 28) who presses these historical examples and others against my discussion of this in Azzouni (2006, chapter 6). Philip Kitcher has raised the same kinds of examples to me in conversation (November 21, 2002); and indeed, these illustrations often arise in the literature as support for the claim that mathematical proof and practices mutate over time.

and some of these changes can only be explained by sociological factors. This is part of what motivates Larvor's twice quoted sentence, "the cost is that we have to abandon the hope of establishing a general test for validity."

As it turns out, not only can the derivationist account handle a mathematical practice that involves localized context-dependent inference patterns, but it also isn't incompatible with a position that takes mathematical practice to be one of "inferential actions" on "concrete objects of some sort," as we've just seen Larvor put it.[6] At least, it isn't incompatible if we set aside ontological concerns about formal derivations. For called for, any proponent of the derivationist account will claim (one who's inclined, anyway, to take seriously the actual facts of mathematical practice) is a distinction between the mathematician *acting on* "concrete objects of some sort" and the concrete objects themselves. According to proponents of the derivationist account, the mathematician *acts* by imaginatively moving from one step in a formal derivation to the next; similarly, in reasoning, on the views of opponents of the derivationist account, the mathematician also *acts* by imaginatively moving (say) from one stage of a diagram to a later one. (For example, a topologist may stretch the visual representation of one knot smoothly into the visual representation of another—see De Toffoli and Giardino (2014) for discussion of this.) Furthermore, even the phrase "concrete objects of some sort" can be understood to make no distinction between how proponents of the derivationist account view mathematical practice and how their opponents view it (at least in this respect). Even the most concrete of diagrams must be grouped into *classes* of semantically related items (e.g., token diagrams that belong to groups of token diagrams, all of which establish the same mathematical result in exactly the same way). Humans always engage with *specific* items, even if their thinking is characterized in terms of the grasping of formal derivations. That is, the specific psychological actions of mathematicians, when engaged in mathematical reasoning, although always directed toward particular concrete objects, are grouped together into relevant collections according to both views.[7]

[6]De Toffoli and Giardino (2015, 316) instead speak of the mathematician's "manipulative imagination" which is brought to bear on "visual representations." The particulars of the suggestion seem similar to Larvor's. For that matter, see Azzouni (2005), where I discuss "inference packages."

[7]Hilbert seems to have had a view much like this. See Posy (2013, 120-121), where he discusses Hilbert's notion of "intuition." In particular, Posy quotes Hilbert (1926):

> [A]s a condition for the use of logical inference and the performance of logical operations, something must already be given to our faculty of representation, certain extralogical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with the object, as something that can neither be reduced to anything else nor requires reduction. This is the basic philosophical position that I consider requisite for mathematics, and in general for all scientific thinking, understanding and communication.

Let's turn, therefore, to the other aspect of informal proof that Larvor points to. He writes, recall, that, "instead of two highly abstract categories, the form of an argument and its content, we now have an indicative list of many and various concrete objects of inferential action (diagrams, models, expressions in special notations, experimental set-ups and so forth)."

This contrast between informal rigorous mathematics and formal derivations is stressed often, although philosophers and logicians who draw attention to it often disagree on its consequences.[8] Rav (2007, 315), in particular, summarizing his discussion of several examples of informal rigorous mathematical proofs, writes, "I hold that mathematicians' manner of reasoning and inferences are based on *meanings* and an informal notion of *truth* that a formal deduction calculus cannot capture" (italics his).

This too fails to track a genuine difference between informal rigorous mathematical proofs and formal derivations (and, specifically, the last clause quoted from Rav can be challenged). It's true that informal rigorous proofs—diagrammatic proofs in particular—are composed of parts or involve actions (in Larvor's sense) that are understood to represent mathematical objects or operations; it's true that they are understood to have semantic contents. Dots on a page, the lines that are drawn, pencilled triangles (in Euclidean diagrams)—these represent points (without dimension), lines (without breadth), and the perfectly bounded triangles of Euclidean geometry. Similarly, continuous smooth deformations of diagrammatic shapes (drawings of doughnuts into drawings of cups, say) represent homeomorphisms among different geometrical shapes that are depicted in stages in topological diagrams (or continuously in computer graphics). In other diagrammatic proofs, contiguous squares represent sums of areas, boxed arrays of numbers represent operators on vector spaces, and so on.

But formal derivations are the same in this respect, at least as far as their "meanings" are concerned.[9] The formulas of a formal language are often given an intended semantics—an "intended model," as it's put. There is, for example, the well-known intended model of Dedekind-Peano arithmetic (the familiar counting numbers), and more generally, there are the various axiomatizations of any mathematical field that come with intended models—in particular, the mathematical subject areas that are axiomatized. Just as with informal rigorous mathematics (diagrams in particular), the well-formed formulas—"wffs" as they're sometimes called—of artificial formal languages have syntactic "parts" that are understood to have semantic contents because of their relationships to the intended model (or, for that matter, because of their relationships to models). The quantifiers, standardly, are interpreted as "ranging over" the domain of a model, the constants in the language

[8] I too have stressed this aspect of ordinary mathematical proof. See Azzouni (2005, 19), as well as other work of mine.

[9] It's not insignificant that Frege and then Russell and Whitehead (and the early Wittgenstein) understood the formal languages they were studying as intrinsically interpreted—as *meaningful*. The practice of treating formal languages either as meaningless, or as entities to which a semantics can be attached (as it were) as an afterthought, comes much *later*.

are taken refer to items in that domain, and the n-place predicates are taken to hold of subsets of the n-product of that domain. Equally importantly, rule-governed inferential movements from one formula to another—syntactically characterized (and therefore mechanically recognizable)—correspond to semantic relations of implication (defined in terms of the models of such formal languages, and not syntactically at all, although provably equivalent to syntactic characterizations in the case of some formal languages).

Although philosophers of mathematics are aware of these elementary points about formal languages, and the semantics such languages are standardly given, some of those philosophers still seem to think that there are important differences between formal and informal mathematical proofs, with respect to the phenomenon of meaning. Meaning is irrelevant to formal proofs, it's often suggested. The fact that such proofs are "machine-checkable" means that validity can be recognized even if the device (or person) checking the proofs has no idea what the propositions of these proofs mean. Furthermore, at least in the first-order case, there are (the well-known) nonstandard models. Even with logics (say, higher-order logics) that have models that are isomorphic to one another (unlike the first-order case), the objects in the differing models of formal languages can be swapped for one another easily. (More generally, anything, e.g., apples, can be swapped for objects—say, some of the numbers—in any model of a higher-order logic.) It's thought, therefore, that there is no intrinsic relationship in the case of formal languages between what the formal propositions are taken to mean and how they are shown to follow from one another; the meanings of these propositions (if any) are completely irrelevant to the process of derivation.

Informal rigorous mathematical proofs are seen as differing in just this respect. Rav (1999, 11) stresses this supposed distinction, even building it definitionally into his nomenclature:

> Let us fix our terminology to understand by *proof* a conceptual proof of customary mathematical discourse, having an irreducible semantic content, and distinguish it from *derivation*, which is a syntactic object of some formal system. ... [F]urthermore, given a finite sequence of formulas in a formal system, there is a purely mechanical way for ascertaining whether the given sequence satisfies the conditions of being a derivation in the system [italics his].

Rav adds (1999, p. 12), speaking of transformations of informal proofs into formal derivations:

> Once we have crossed the *Hibert Bridge* into the land of meaningless symbols, we find ourselves on the shuffleboard of symbol manipulations, and as these symbols do not encode meanings, we cannot return *via* the Hilbert Bridge and *restore* meanings on the basis of a sequence of symbols representing formal derivations. After all, it is the very purpose of formalisation to squeeze out the sap of meanings in order not to blur focusing only on the logico-structural properties of proofs. Meanings are now shifted to the metalanguage, as is well known [italics his].

The same view of the relationship of informal proofs and meanings is indicated by the earlier quotations I gave from Larvor and from De Toffoli and Giardino: the meanings—the interpretations—of the terms and sentences of informal rigorous mathematical proofs are essential to their functioning. It's this, on such views, that makes context so important to mathematical proof. (De Toffoli and Giardino (2014, 333) write, recall from above, "[continuous transformations on pictures] are part of the background material in the sense that any topologist knows immediately that these transformations can be interpreted in terms of homeomorphisms.") Professionals, that is, know what's being talked about in an informal mathematical proof because of their antecedent training in such mathematical contexts. This is seen as true, in particular, of diagrammatic proofs.

In the next few sections, I'll endeavor to undercut this supposed difference between informal professional mathematical proofs and formal derivations.

3 Rule-Governed Games

I'll start by engaging in what might initially appear to be a section-long digression about the characteristics of a specific kind of rule-governed game that connect those games to notions of computability. But in the sections that follow, I'll return to the topic at hand by sketching a characterization of diagrammatic proof systems—specifically, the venerable system of the Book I of *The Elements*—directly in terms of the notions of computability that are applied to these games. Finally, I'll return to the debate between proponents of the derivationist account and their opponents and show that this material illuminates that debate by providing a successor view to the derivationist account that shows in what sense *Agreement* is a datum of mathematical practice and shows how the satisfaction of *Agreement* by mathematical practice can be explained in terms of the rule-governed games that are described in sections 3 and 4.

To start, a game has a finite set of kinds of *game pieces*. Game pieces are various physical items stipulated as useable in that specific game. Any of the following can be game pieces in a game: chips; balls; chess pieces; playing cards; humans; mental entities (of one sort or another); written items of various kinds on paper or sand or whatever; diagrammatic entities, such as drawings of specified sorts; computer graphics; and so on. Game pieces are understood to have properties. In particular (and very importantly), they are easily distinguished from one another by participants (and by observers of the game)—this is both in the sense that the tokens of a kind of game piece are distinguishable from one another and also in the sense that different *kinds* of game pieces are distinguishable from one another. The properties game pieces are presumed to have (and that enable these distinctions), however, aren't their pure physical properties; they aren't even their *perceivable* physical properties—the physical properties of these items that are perceivable, say, by participants. Rather, and crucially, their properties are *stipulated*, either explicitly or tacitly. Their "properties," that is, are *conventional* ones that they can be specified to have *because of* their perceivable physical properties. I'll discuss this in more detail in section 6.

Important also is the assumption of a *field* that the game takes place on. This is simply the space—either one, two, or three dimensions, that game pieces are placed in (or on). Any field is exhaustively divided into *cells*—again, cells of either one, two, or three dimensions. These cells have *adjacency* relations: Each cell is stipulated to be immediately adjacent to a fixed set (and finite number) of other cells; adjacency relations are reflexive, symmetric, and not transitive. Each cell can contain one and only one game piece at any time. A Turing-machine tape, for example, extends linearly along one dimension and is divided into squares, each of which is adjacent to its two adjoining squares. A piece of paper on which diagrams are drawn uses a field of two dimensions, and a game played in space (such as baseball) can require three dimensions. (Correspondingly, the game pieces themselves can be items of one, two, or three dimensions.) The field can be (potentially) infinite in any or all of its dimensions, although its *resolution* is always finite. That is, any finite subregion of a field contains only a finite number of cells.[10] (I'll call this the *finite-subregion-finite-cell property* in what follows.) Call a cell *occupied* iff it contains a game piece. A *configuration* of the field is any placement of a finite number of game pieces in its cells. A set of cells is *connected* (in a field) if, for any two cells c_a and c_b, there is a sequence of cells, $c_a, c_1, \ldots, c_n, c_b$, where each cell is adjacent to the cells immediately before and after it in the series. A *subconfiguration* of a configuration is any subset of the cells of that configuration.

Game states are recursively specified configurations of none, some, or all of the game pieces according to the rules of the game. The rules, usually, don't specify all the acceptable configurations directly, but only in terms of transitions from earlier configurations and the null configuration (the empty field). These rules, that is, specify the *admissible game episodes*, which are sequences of game states over time, each one admissible according to the rules on the basis of earlier game states. An admissible game episode, that is, is any physical (or mental) movement of the game pieces into cells or from cells to other cells that's licensed by the rules of the game. I'll sometimes call these "admissible transitions" or "admissible game-state transitions." An *umpire* is someone (human or machine) who watches the game and knows the rules. I include among umpires, in addition, those who *execute* derivations according to the rules of a logic. More generally, if a game has only one player, that player is also an umpire—and, of course, anyone who watches a game (or a record of that game) is an umpire. A game is *mechanically recognizable* if an umpire can immediately recognize at a time (by a mechanical or unintelligent application of the rules the umpire has memorized) the admissible transitions occurring at that time of any episode in that game. In particular, this is only possible if the admissible transitions allowed by the rules are all *locally constrained*: they operate only on

[10] It's, perhaps, not obvious that these latter two cases are as I've just described them—in particular that finite subregions of these fields have finitely many cells. (They may strike people, instead, as continuum-structured.) I make my case for this in section 5.

finite subregions of the field that are within the immediate purview of the umpire(s) at that stage in the game.[11]

Playable games, as I've characterized them, are a very broad class of human activities. I intend to be among them the sports games so popular in our culture, but also role-playing games, board games, various card games, as well as the calculational devices of various sorts that we engage in: arithmetical calculations using pen and paper, more physical ways of manipulating things to execute calculations, beads on strings (for adding, say), and so on. They are also meant to include formal proofs and algorithms, as well as the informal rigorous mathematical proofs that use diagrams. Included among these games are "nondeterministic" ones— say, ones using dice or other devices that introduce an "element of chance" into the game. The construction of a formal derivation, of course, is also "nondeterministic" in the sense that there is (usually) more than one subsequent (interim conclusion) that follows from a sequence of formal propositions at any one time.

The focus here, however, isn't on the *production* of game episodes; it's on the ability of the *umpire(s)* to mechanically *recognize* that every game state in a game is due to an admissible transition from earlier game states. I'm *not* assuming that an *entire* admissible episode is effectively or mechanically recognizable "at a glance"

[11] Copeland (2015, 1) writes, "A method or procedure, M, for achieving some desired result is called 'effective' or 'mechanical' just in case: 1. M is set out in terms of a finite number of exact instructions (each instruction being expressed by means of a finite number of symbols); 2. M will, if carried out without error, produce the desired result in a finite number of steps; 3. M can (in practice or in principle) be carried out by a human being unaided by any machinery save paper and pencil; 4. M demands no insight or ingenuity on the part of the human being carrying it out." (I've altered the typographical format of this quotation.) I mean what I've described above to be in the spirit of what Copeland writes (which in turn is in the spirit of Turing (1936)), although as my discussion will indicate, I disagree with some of the details. Here are several points of clarification about these disagreements: "Symbol" is misleading, in the broader context of games—but also, perhaps, in relation to one of the intended applications, derivations in formal languages—so I've eliminated this word. Only in certain cases are the items being manipulated "symbols," as I'll discuss later; this is connected to the issue of "meaning" already raised in section 2. There is some redundancy in Copeland's (1), (2), and (4); the idea is that something (without intelligence) can carry out the procedure. In any case, I've dropped (2) altogether (as well as the relevant phrase "for some desired result" in what Copeland takes to be defined). This is because there need not be a "desired result"; there is only the question of how to characterize the intuitive idea of a method or procedure being "effective" or "mechanical." Goals needn't come into it. Finally, I've generalized the cases of procedures to ones beyond those using pencil and paper, as Turing originally restricted the discussion. It changes nothing essential to do this (or so I'll argue in this and the next section, following Gandy and Sieg); besides, since certain diagrammatic proofs now occur in the medium of computer graphics, instead of pencil and paper (or chalk and chalkboard), the generalization is needed for the topic of this paper: informal rigorous mathematical proof. Notice, in particular, that certain topological proofs can be animated visuals where, for example, one shape smoothly transforms into another over time. A last point: I'm unsure what role "exact" plays in Copeland's characterization of the intuitive notion of mechanical or effective procedure. Many mechanical or effective procedures have (or can easily be imagined to have) rules that don't dictate, given a game state, a transition. This may be because no rule applies to this game state or because it's vague whether a rule applies or not. (If umpires can disagree on the outcome of a rule or whether it even applies, we stipulate that the rule doesn't apply.) At this point, therefore, the game episode ends ("halts" is a perfectly good term for this). Perhaps what's meant is that the admissible transitions of game state to game state that are determined by the rules should be clear to all participants.

or "immediately." The recognition that a game episode is admissible is usually the result of memory and/or the inspection of a record of the game: one sees in this way that all the game-state transitions in that game episode are admissible.

4 Finite Resources for Judging Games

Turing (1936) originally characterized the intuitive notion of an "effective" or "mechanical" procedure for calculating sequences of numerals in terms of "computers" (human calculators) with unlimited time, paper, and pencils on their hands—as well as a pretty inhuman capacity to concentrate endlessly on mechanical tasks. He further assumed such beings have (in principle) only a finite set of mental states and a finite set of symbols available.[12] Despite these apparent limitations, he was able to indicate good reasons for thinking that the intuitive notion of an "effective" or "mechanical" numerical function is equivalent to what has subsequently come to be called Turing computability. (This is a version of the Turing-Church thesis.)

Sieg (2000, 2008, and elsewhere) has stressed the importance of Turing's discussion of the memory and the sensory limitations of humans engaged in mechanical tasks, both in motivating Turing's characterization of Turing "machines" (human calculators), and as the distinctive element in Turing's approach to computability—as opposed to Gödel, Church, Kleene, and others.[13] Sieg uses this characterization of human limits to axiomatize the intuitive notion of mechanical or effective procedure and to show its equivalence to an axiomatized notion of Turing computability. Thus, instead of linking Turing computability (and the other notions equivalent to it) to the intuitive notion of effective or mechanical procedure by the Turing-Church thesis, he captures the relationship by characterizing *both* notions axiomatically and showing their equivalence.[14]

In any case, these limitations motivate two "boundedness conditions" and two "locality conditions" that I borrow from Sieg (2008, 575) and modify to apply to the games I've just characterized:

[12]See Turing (1936, 135-136).

[13]Although not Post. See Post (1936) and also the nice comparison and analysis of Post and Turing's approaches in Sieg and Byrnes (1996).

[14]Sieg (2013, 190) writes: "The methodological difficulties [of there being no proof of Turing's thesis] can be avoided by taking an alternative approach, namely, to characterize a *Turing computor* [a human executing a mechanical procedure] axiomatically as a discrete dynamical system and to show that any system satisfying the axioms is computationally reducible to a Turing machine. … No appeal to a thesis is needed; rather, that appeal has been replaced by the task of recognizing the correctness of axioms for an intended notion. This [is a] way of extracting from Turing's analysis clear axiomatic conditions and then establishing a representation theorem …." I should add that although Sieg's approach is illuminating; I have doubts that it changes the epistemological situation very much—although it *does* show that the Turing-Church thesis is really just a matter of applied mathematics. The possibility that the Turing-Church thesis is false translates (without residue) into the possibility of apparent examples of computation that fail to be appropriately characterized by the axiomatized notions we were hoping those applications would fall under.

(B.1) There is a fixed finite bound on the number of cells (with or without game pieces in them) that any umpire can immediately recognize (at a time).
(B.2) There is a fixed finite bound on the set of rules of any game.[15]

(L.1) An admissible transition of a configuration to another configuration can only be based upon the movement of game pieces with respect to immediately recognized cells.
(L.2) The class of immediately recognized cells can be changed, but each of the new observed cells must be within a bounded distance L of (any of) the previously immediately observed cells.

There are two points about these constraints. They can be motivated, first, directly as requirements on games, as I indicated earlier, by the ordinary (and indisputable) limitations of human memory and human sensory systems—this is pretty much how Turing (and Sieg) motivates them. I should add that these limitations of memory and sensory capacity are shared by current machines and—particularly—current "machine vision."[16] But, second, and more particularly, B.1–L.2 together motivate the specific constraints I've placed on games—particularly, the constraints that any finite subregion of the field of a game has only a finite number of cells and that admissible transitions affect only finite subregions of fields that are being immediately surveyed by umpire(s).

5 Are Certain Diagrammatic Proofs Analogue or Infinite?

It may well be thought that my description of the field of any game explicitly excludes an application of this game model to most physical games that take place in space and time.[17] In the case of baseball, for example, it may be thought that certain events, such as catching a ball in the outfield, involve continuum-many (or at least, countably many) similar possible events, because any such particular event can vary ever so slightly in its trajectory in space and time (without a lower limit in closeness).

Furthermore, attempting to apply this approach to diagrammatic proofs may seem to result in something even more inadequate. Consider the initial instructions we've given in Book I of *Euclid's Geometry*. We may place a point anywhere; we may draw a circle anywhere.[18] Assuming the background space of the diagram is

[15]This can be relaxed to a recursive set of rules.

[16]I expect these limitations to remain in place for machines (and more generally, for robots) forever, although this is controversial in some circles.

[17]Gandy (1980, 125) explicitly excludes from his approach anything which is *essentially* an analogue machine.

[18]Postulate 1: To draw a straight line from any point to any point. Postulate 3: To describe a circle with any center and distance (Heath 1956, 154).

a real manifold—something that *seems* required by other assumptions of Euclidean geometry (in particular, that intersections of lines always yield points)—any hope that Euclidean *diagrams* aren't analogue seems dashed. Thus, treating a piece of paper as a field with the finite-subregion-finite-cell property seems ruled out by Euclidean diagrammatic practices. Similarly, recall that De Toffoli and Giardino (2014, 33) write about continuous transformations being permissible actions on *pictures*; the phrase "continuous transformations" is clearly meant to apply to diagrams, and the semantic interpretations of the diagrams so *continuously* transformed are homeomorphisms. Even more dramatically, Feferman (2012) is titled: "And so on ...: reasoning with infinite diagrams." If Feferman is to be taken literally (and he makes it clear in the paper that he *is* to be taken literally),[19] this rules out pretty explicitly a characterization of the configurations such diagrams are embodied in being finite.

It's important, however, to keep the properties of the *diagrams* mathematicians peruse (on paper, on computer screens, or in their minds) strictly distinct from what those diagrams are semantically associated with. Let me start with Feferman's claims about diagrams and oppose those claims with a *truism*: **No infinite diagrams appear anywhere in Feferman's paper.** Everything that *does* appear in those papers is finite. Below are two examples from his paper (Feferman 2012, 376, 379; I've retained his numbering: Fig. 3 and Fig. 7):

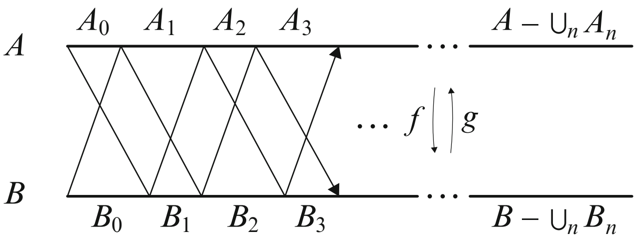


Fig. 3 A diagrammatic proof of the Cantor-Bernstein Theorem

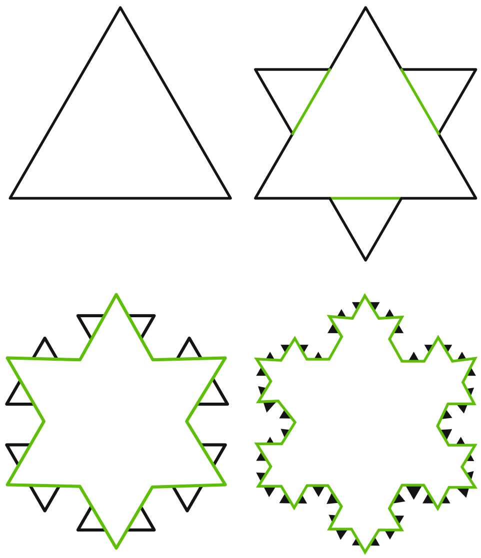
$$\begin{tikzcd}
 & 0 \arrow[d] & 0 \arrow[d] & 0 \arrow[d] & 0 \arrow[d] & 0 \arrow[d] & \\
\cdots \arrow[r] & A_{n+2} \arrow[r] \arrow[d] & A_{n+1} \arrow[r, "d"] \arrow[d] & A_n \arrow[r, "d"] \arrow[d, "j"] & A_{n-1} \arrow[r, "d"] \arrow[d] & A_{n-2} \arrow[r] \arrow[d, "j"] & \cdots \\
\cdots \arrow[r] & B_{n+2} \arrow[r] \arrow[d] & B_{n+1} \arrow[r, "d"] \arrow[d] & B_n \arrow[r] \arrow[d, "\pi"'] & B_{n-1} \arrow[r] \arrow[d] & B_{n-2} \arrow[r] \arrow[d] & \cdots \\
\cdots \arrow[r] & C_{n+2} \arrow[r, "d"] \arrow[d] & C_{n+1} \arrow[r, "d"] \arrow[d] & C_n \arrow[r] \arrow[d] \arrow[uur, dashed] & C_{n-1} \arrow[r] \arrow[d] & C_{n-2} \arrow[r] \arrow[d] & \cdots \\
 & 0 & 0 & 0 & 0 & 0 &
\end{tikzcd}$$

Fig. 7 Connecting homomorphism for exact sequence of homology

[19]Feferman (2012, 376) writes, preceding his discussion of a proof of the Cantor-Bernstein Theorem, "Let us now turn to infinite diagrams which can be visualized in full, in contrast to those of the preceding section, though they may also involve the iteration of certain constructions."

Notice that various conventionalized symbols *do* appear in the above. In particular, in the second diagrammatic proof, there are conventionalized pairings of *arrows* with *ellipses* that indicate that there is more of the same in the various directions the arrows are pointing to (or from, when ellipses precede the arrows). In Feferman's Fig. 3, the conventionalized indications of "more of the same" are more intricate, involving a conventionalized description of how a pair of functions are supposed to operate.

What does *not* appear at all (anywhere) are the *more* of the diagrams that are *indicated* by arrows and ellipses. What *does* appear, to repeat, are just arrows and dots. It might be responded that I'm being unfair to Feferman by demanding that if an infinite diagram is involved, as he claims, then that infinite diagram must appear *in his paper*. Instead (recall from the quotation in footnote 19), what's required is only that the diagram in question be "visualized in full." This response isn't going to do. Feferman, in his paper, is making a distinction between two kinds of cases where a finite depiction conventionally indicates how we are to go on should we desire to sketch more of the diagram on paper (or think of further parts of that diagram in our minds). In the cases above, what's involved is a further sketching of the diagram that looks the same as what's already present to the mathematician's eye on paper. But Feferman understands other cases differently. Consider the diagrammatic proof of the existence of a bounded continuous closed curve with no finite length and no tangent at any point (the "Koch snowflake"). I provide the diagrammatic proof, immediately below, that Feferman (2012, 374) gives (but which I have borrowed from somewhere or other on the web and have modified to include the needed ellipses):



...

This, as Feferman (2012, 374) puts it:

> is the limiting curve of a sequence of polygons beginning with an equilateral triangle of side 1. The sequence is described inductively: at each stage, one simultaneously divides each side of the polygon before us into three equal segments, then builds an equilateral triangle on the middle segment, and finally deletes the base of the new triangle except for its endpoints. Since the length of the circumference of this figure at each stage is multiplied by 4/3, and since $(4/3)^n$ approaches infinity, the limiting curve has no finite length.

The limiting curve cannot be visualized because it has infinite length (and no tangent at any point). This is true, however, of none of the diagrams in the infinite sequence that converges (as it were) to the limiting curve: all of them have finite lengths (which can be explicitly calculated as Feferman indicates) and each lacks tangents at only finite many points. But what's important to realize is that beyond a certain *finite* number of these diagrams (say 140, to be *very* conservative), none of the remaining (infinitely many) members of the sequence of *diagrams* can be visualized either—this is in the sense that the viewer's eye becomes incapable of distinguishing differences between any of them. Notice this important point: limitations in what viewers can see are also (pretty much) limitations in what the viewers can *see* in their "mind's eye."

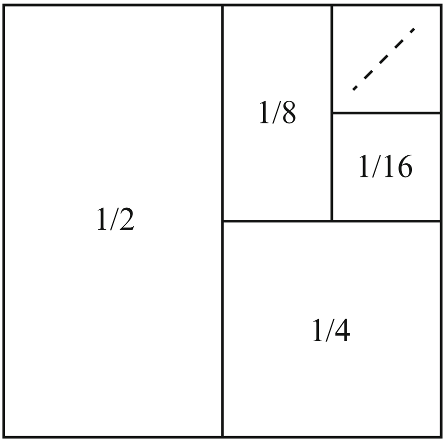
For the same reasons, one cannot visualize *in full*—in one's mind—the infinite diagrams indicated by Feferman's figures 3 and 7. Visualization can only be something that involves a mental replication—at least, in the relevant respects—of what we do when we use our eyes to see anything. But we have no examples of seeing (using our eyes) a fully infinite pattern of any sort. So, unless, mental visualizing can sprint beyond the capacities of our senses with respect to the infinite, we cannot be said to *visualize* completed infinities. There is a difference, of course, between the Koch snowflake and the other diagrams Feferman mentions. This is that the subsequent diagrams in the series (e.g., extending Feferman's Figure 3 or Figure 7) are just "more of the same." But that we *understand that* there is more of the same to a diagram doesn't mean we can (visually) complete that diagram in our mind's eye by presenting to ourselves (mentally) *all* of that "more of the same."[20]

Because Feferman describes these diagrams as infinite, therefore, I have to accuse him of confounding two very different phenomena. (I really don't want to accuse him of this, but I think I have no choice.) On the one hand, there are the actual physically real (although conventionalized) diagrams that we instantiate physically in space and time on computer screens or with paper and pencil, or on blackboard with chalk, etc. (or that we imagine in time), and there are other sorts of completions of such items that we can conceive of but that don't actually occur *instantiated anywhere in* our diagrammatic practices—either on or in visual media of various sorts or in our minds.

[20]I must stress, however, that I'm *not* arguing that we can't *conceive of* completed infinities—this is a claim that certain constructivists and intuitionists make, but not me. I *am* saying that however we do conceive of such things, we don't do so it by directly visualizing them. I'm urging us, therefore, at least when discussing diagrammatic proofs, to take Descartes' old distinction between imagining and understanding seriously. (See Descartes 1979, specifically his sixth meditation.)

Consider the simple diagrammatic proof below that the series ½ + ¼ + … sums to 1:

Proof:



This diagram can be viewed in any of three ways. The first is as I urge, the diagram itself, on my approach, contains nothing infinite. Instead, there are three dashes which conventionally stand for something like "a diagram-like object that continues (forever) the pattern you actually see on the paper." The second interpretation is one that Feferman attributes to the diagrammatic proofs of the Koch snowflake. We're given a couple of diagrams which visually indicate how a manipulation of the diagrams to produce further ones in the series can be continued infinitely and where (up to a finite point!) each of the resulting diagrams can be visualized, but where the limit of the series isn't visualizable because it doesn't "go on in the same way" as any of the diagrams in the series do. The third interpretation is the one that Feferman attributes to his figures 3 and 7: actual infinite diagrams—items that he takes us as able to "fully visualize."

What tempts Feferman to his latter two interpretations of diagrams (I claim) is that the diagrams that actually appear in his paper (and in mine) involve both intended (and explicit) semantic relations to mathematical objects that they are to show results about (series of numbers, curves of various sorts, set-theoretic objects) as well as semantic relations to what should be called "diagrammatic objects"—items, however, that don't appear in these papers but are conventionally indicated by the use of dots, arrows, or lines). Results about these latter items are used, in turn, to show results about the mathematical objects these items also have semantic relations to.

However, to treat the semantically indicated (infinite) diagrammatic objects as *diagrams* that are part of the informal rigorous diagrammatic proofs that mathematicians provide to one another is a mistake. Instead, "diagrammatic objects" in this second sense of "diagrammatic" are (as I've noted) *themselves* mathematical objects that are the referential targets of the diagrams that actually appear on paper (or on computer screens, or whatever) just as numbers, curves, and set-theoretic objects are. *Anything* can be treated as an object of mathematical study—that includes "diagrams" understood as certain sorts of idealized objects only certain parts of which can appear concretely. We should not, therefore, confuse the *actual* diagrams that Feferman reproduces in his paper (or the ones that we can constitute in our thinking) with the ones only referred to by this diagrammatic practice—referred to along with other mathematical objects that are also referred to. These (finite!) diagrammatic proofs, in practice, *refer to* certain sorts of infinite processes (completed or continuously engaged in) applied to what are ultimately *mathematical* diagrammatic objects. These infinite processes (which are also mathematical—not part of the proof but part of what the proofs are about) in turn yield infinite (or limits of continuous series of) diagrammatic objects (which are also called "diagrams"). Results about these, finally, are understood to imply the desired number theoretic or set-theoretic theorems.[21]

The attribution of properties to diagrams that, strictly speaking, apply not to them but (at best) only to the objects to which these diagrams have semantic relations is hardly restricted to Feferman. It occurs widely. When, for example, De Toffoli and Giardino (2014, 33) write about "continuous transformations" of *diagrams*, this is actually a matter of *discrete* diagrams that represent continuous transformations, not anything that's actually being continuously applied to diagrams on paper (or electronically—I include diagrams that move on the computer screen).

Part of the problem is how mathematicians themselves talk. In writing proofs that involve writing about diagrams, or notation more generally (which almost always happens), mathematicians studiously avoid the niceties of "use and mention"—in Quine's sense. Mathematicians, for example, effortlessly shift between talk of "functions" (understood as mathematical items) and "functions" understood as the notation standing for functions—in the latter case, they will talk about the indices on certain functions and shift in the same sentence to describing the properties of those functions as mathematical objects.[22] More elementary examples of the

[21]These diagrammatic proofs, because they refer to both infinite "diagrams" and certain more traditional mathematical objects, have been revealed to have an interesting intricate (semantic) structure that's pretty much never made explicit in their exposition. If it were, there would have to be an entire sequence of theorems about "diagrams" preceding these proofs.

[22]I discuss this in some detail—especially with respect to how it enables mathematicians to abbreviate and shorten informal rigorous proofs—in Azzouni (2006, chapter 7), especially p. 149-150. One nice topic area where distinctions between use and mention are regularly ignored by mathematicians is linear algebra. (Proofs in this subject area would become insanely longer if they were rewritten to avoid use/mention errors, say, between the talk of operators on vector spaces and their properties, as opposed to the properties of the *notation*—matrices—that represent those operators.) One common motivation for use/mention errors in mathematics is that computations often rely on properties of the notation that need to be described in the course of a proof. So, mathematicians regularly (but informally) engage in semantic ascent and descent in the course of many ordinary informal rigorous mathematical proofs. I should add that this kind of "slippage" in mathematical discourse between radically different sorts of "objects" that are nevertheless co-referred to by the same noun phrases is typical of natural language generally. Consider the use of noun phrases, such as "London" or "person" in sentences like the following: "London is so

same practice are the use of the words "triangle" and "line," in the Euclidean diagram tradition, to describe both the diagrammatic items themselves *and* what those diagrammatic items represent. (I do this sort of thing in this very paper, of course.)

But there is another important factor that arises more specifically with respect to diagrams and not with notation, generally. Mathematicians certainly speak (as De Toffoli and Giardino do) of "continuous transformations" of diagrams (in thought, but also on the computer screen). This way of speaking (and of thinking about diagrams) is derived, of course, from how we experience movements in space. We experience movements of objects through space (and of actions in space, such as drawing curves on paper) as continuous—more accurately, as *space filling*. But simultaneously, our experience of *anything* in space always involves finite resolution. This gives rise to subtleties in diagrammatic conventions that I'll discuss in the next section.

6 The Role of Conventions in Diagrams

One thing that makes our experience of space intuitively puzzling (and this is also something that can easily mislead us when we theorize about diagrammatic proofs) is that although anything we do in space (by way of playing games, drawing diagrams, etc.) obeys the finite-subregion-finite-cell property, we don't *directly see this*—in particular, we don't see the *cells* the fields of these games are required to be composed of. The curves that we draw on paper, or the movements we make in space, as I mentioned earlier, look space filling: it's this that makes the postulation of space as composed of continuum-many points seems intuitively plausible; conversely, it's why any suggestion that the space we interact with is finite-region-finite-cell-structured seems, well, *crazy*.

But that any game we play in space (any diagram we draw on a page) must be on a field that obeys the finite-subregion-finite-cell property is easily established by the fact (which is also pretty obvious) that we cannot distinguish two spots, if they are within a certain closeness, or see a spot at all, if it's too small.[23] We similarly recognize that we can only distinguish finitely many curves in any finite region of

unhappy, ugly, and polluted that it should be destroyed and rebuilt 100 miles away" (from Chomsky (2000, 37)). Also see Pietroski (2005) on this matter.

[23]For us to discern nearby dots (pixels) as separate, apparently, they have to be at smaller angular distances from one another than our eye's angular resolution. And the average of the latter, apparently, is around 1 minute of arc. That's why we can discern more distinctions if we move our eyes closer to a computer screen or to a piece of paper a diagram appears on. (Of course, there are serious limits to this method of improving resolution—posed by, among other things, the nose). But a fun trick this allows is pixelating a computer-screen presentation of (well, nearly anything) by moving our eyes closer to the screen. (As we age, our ability to do this diminishes—which is sad.)

space (that we can immediately survey). These two facts motivate, pretty directly, a metaphysical picture of space and what's in it as things that go infinitely beyond our visual senses: we can always (in principle) focus in more closely—either by eye or with magnifying instrumentation—and doing so will reveal further distinctions we can't see otherwise. This is, intuitively, an infinitely iterable process of divisibility that's limited only by practical considerations.[24]

It should be clear, however, that any diagrammatic proof procedure must honor the finite-subregion-finite-cell property because of the simple requirement that whatever visual items that appear in that diagram must be, first, distinguishable and, second, recognizable as what they are supposed to be, *by eye*. This is why the mere visual qualities of diagrams are (and must be) supplemented by explicit and tacit conventions that able practitioners of diagrammatic proofs utilize automatically.[25] In saying this, I'm only pointing out the truism—in the particular case of Euclidean diagrams—that, for example, visual dots (that are perceived by us to have two dimensions) nevertheless stand for dimensionless points, diagrammatic lines (widthful things that are usually perceived by us to be both crooked and irregular in their widths) are to represent straight, one-dimensional items and so on.

The conventions I've invoked play two roles. I'll discuss the first in this and the next paragraph and then turn to the second. There is, first, the classification of various physical items as *game pieces*. It's not merely, for example, that a class of physical items that "look alike" are induced by that alone to be the same kind of game pieces. In practice, a game piece that in fact looks like a different game piece may, because of its role in a series of configurations, be recognized as the game piece it's stipulated to be—despite its appearance. (So, e.g., during a game of chess, something that physically looks like a pawn may in fact be recognized by everyone watching the game to be a king.) Similarly, conventions require us to be able to distinguish distinct game pieces—in different spatial locations—as, indeed, located "in different places." Notice that the actual metaphysics of the field—whatever it *really* is (e.g., quantum foam)—is completely irrelevant to the stipulated properties of the field; we perceive the drawing of a line (of whatever length) as completely space filled. But, instead, if such a line is finitely extended, this is by the placing of conventionally stipulated parallel line symbols (ones stipulated to be parallel to the line symbols already present) in a finite number of cells immediately adjacent to one another. This is a given of the Euclidean diagrammatic practice because we can't distinguish more than finitely many distinctions among the possible lengths and angles of the lines we draw or could have drawn.

[24] Contemporary microphysics is no longer friendly to this metaphysical picture.

[25] For further details about the relevant notion of convention in play here, see Azzouni (2014). I should also note that what we recognize to be conventional properties of elements in our visual experience (e.g., that certain shapes are words with meanings) can nevertheless be experienced involuntarily and automatically. See Azzouni (2013a) for further discussion of this.

In the case of Euclidean diagrams, the game pieces are (one-cell) portions of lines, circles, and so on. In the case of formal derivations, the game pieces are the primitive alphabetic items, individual variable symbols, the quantifier symbols, and so on. Notice further that individual game pieces are manipulated in admissible transitions of formal derivations—although, in practice, more than one piece is manipulated at a time. In the case of Euclidean diagrams, as I've been describing them, finite sets of game pieces (in connected cells) are always manipulated all at once. One never extends a line, for example, by occupying only a single adjacent cell with a line symbol. *Many such* must be occupied simultaneously (although only finitely many *many such*, of course). This is simply because the cells are below visual threshold in Euclidean diagrams—although they must exist because of B1–L2.[26]

The second role that the conventions I've invoked above play is to stipulate semantic relationships between the game pieces (and configurations of game pieces) and mathematical objects. It's by the (usually tacit) stipulation of these semantic relationships that game pieces become *symbols.* These semantic conventions often shift historically because of the background evolution of mathematical concepts. For example, the notion of a function underwent a progressive evolution,[27] from an early notion of *function* to a spread of later notions (*arbitrary function*, *everywhere differentiable function*, *continuous function*). What class of mathematical objects (what set of functions) a drawn curve on a piece of paper *represents* is *not* given "by eye"; it's given by conventional stipulation of the semantic relations of the game pieces (even if the conventions in question are tacit ones).

Although I won't dwell on this in *this* paper, conventional choices of semantic relations can be better or worse for the value of the resulting diagrammatic proof procedure. The soundness and validity of the standard diagrammatic proof of the intermediate value theorem (see fig. 3.2 immediately below this paragraph, from Brown (1999, 27)), for example, turns completely on what class of functions are stipulated as indicated by curves on pieces of paper. If smoothly drawn curves are to represent only continuous functions, then the proof shows what it's supposed to show. Otherwise it may not.[28] The mechanical recognizability of a proof—of any

[26]The actual geometry of the cells is empirically determinable (in particular how many cells each cell is adjacent to)—although that geometry is relative to certain factors. Given a fixed distance of the eye to the page, the geometry is determined by when distinctions can't be made among points and lines and so on. This geometry differs from person to person and changes if the position of eye to page shifts. In practice, we always manipulate diagrams by shifting game pieces in multiple cells at, in effect, the same time (e.g., even when extending a line on the page by ever so little).

[27]See Azzouni (1994, 50-52) and the citations given there.

[28]For example, if the functions depicted include ones that are discontinuous (e.g., with rational values all at zero but with irrational values where the curve appears in the diagram), I should add that the nearly total conventional nature of the diagrams in diagrammatic proof is widely either overlooked or underestimated. In particular, it's often assumed without argument (and without the realization that the claim is implausible) that facts about which mathematical objects the items in a diagram *look like* play a major role in some diagrammatic traditions (e.g., the Euclidean one) in determining what mathematical objects (abstracta) they refer to. See, for example, Giaquinto

sort—all by itself proves *nothing*. That proof must have an *interpretation*: this is equally true of formal derivations and informal rigorous mathematical proofs. And, there is nothing intrinsic to diagrams that forces one interpretation on them and not another. To assume this is to simply overlook that a visual appearance of a diagram may make a certain interpretation *natural*; but naturalness isn't a requirement on interpretation.

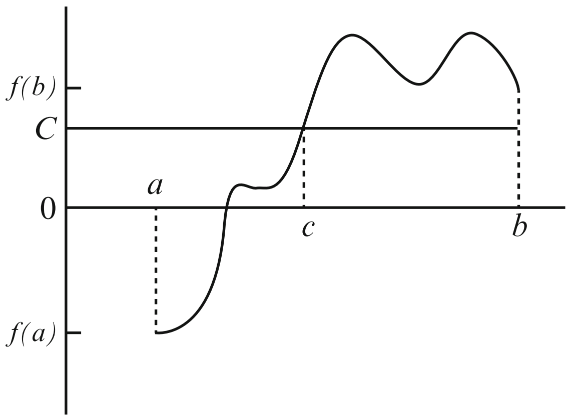


Figure 3.2 The intermediate value theorem

An important point should be brought to attention (although the details about it are developed further in other work cited in a footnote at the end of this paragraph). The effective procedures that can be applied to game pieces and the semantic conventions that determine the mathematical subject matter these game pieces (and their configurations) are about must coordinate appropriately if a diagrammatic proof procedure is to operate successfully. So, for example, allowing the visually measured lengths of lines in Euclidean diagrams to be semantically significant (to correspond to actual lengths of the depicted lines) will yield an unsuccessful diagrammatic practice because effective recognition of the conventionalized properties

(2016), especially his discussion of Azzouni (2013b). Also see Giaquinto (2007), especially chapter 12, which, in part, focuses on a distinction between the differences in extent to which a diagram depends on resemblance vs. conventions of representation. The role of convention, however (here and elsewhere), is underdescribed because it's so widely taken for granted that dots on paper, for example, visually resemble (mathematical) points and drawn lines visually resemble (mathematical) lines. They *don't*, since the mathematical items are not visualizable *at all* for pretty much the same reasons that a Koch snowflake isn't visualizable: something with no dimensions (or only one dimension) can't be seen. How visual capacities are exploited by diagrammatic traditions in a way that makes diagrams so much easier to understand than language-based proofs is—at present—not well understood (although see Azzouni (2005) on inference packages for suggestions). In any case, talk of "resemblance" is a metaphor that doesn't help. (Also see Avigad (2009) for discussion of the relevance of contemporary vision science to the analysis of diagrammatic proof procedures.)

of the resulting diagrams is absent. (We can't tell exactly how long these things are by eye: an appropriate semantic convention using visually perceived lengths to stand for the lengths of the corresponding mathematical objects isn't easily established.)[29]

That is, the properties that game pieces are stipulated to have, in cases where configurations are interpreted, must be ones that yield effective recognition of the properties of configurations while at the same time being *sound* with respect to the mathematical interpretation of these configurations. These are not exactly straightforward requirements to satisfy. In particular, this explains why diagrammatic proof techniques are hard to invent—why serious mathematical talent is needed to do this.

My use of the word, "stipulation," to describe these two roles of conventions can be misleading (so let me try to fix that). In practice, especially with diagrammatic proofs, the experience of the semantic relationships between configurations of game pieces, such as triangles drawn on a piece of paper, and what those triangles are supposed to depict, is so automatic and seamless that it may strike some as an intrinsic semantic property of diagrammatic items—in the sense that these diagrammatic items *must* depict certain mathematical objects and not others. This, however, is surely not true. Nevertheless, our automatic and pretty involuntary experience of certain semantic relationships between diagrammatic items (e.g., drawn lines and mathematical lines) explains the impression some have that the role of meaning and truth is intrinsic to informal rigorous proofs and that this aspect of such proofs can't be captured by formalized imitation. A similar experience with words on a page can give the same impression about the meanings we associate with those words: The impression that certain words *intrinsically* refer to what they refer to. Visual experience, in general, always contains conventionalized elements that are an involuntary part of that experience. In neither case should we think either that the conventionalized elements are required to be there or that they are intrinsic in some way to what's given to us visually by our senses nor should we think that such conventionalized elements are simply a matter of what "we see," whether those are words or diagrams that appear on a page.

Conventions being an involuntary part of visual experience misleads in a second way, especially with respect to diagrammatic proofs. Many philosophers find it tempting to see us (and mathematicians, generally) as "abstracting" or "intuiting" pure mathematical objects from what we see in the world. Although I shouldn't say anything further about this now (solely for reasons of space), it should be clear that I regard this as a completely mistaken characterization of how perception (and the mind) works with respect to our "grasp" of abstracta.[30]

[29]See Manders (1995, 2008) and Azzouni (2004). For a discussion of how fatuous concerns about the rigor of diagrammatic proofs arise from tacit shifts in the mathematical interpretation of the configurations of a diagrammatic practice or from a failure to recognize that the mathematical interpretation of a diagrammatic practice isn't a matter of what the diagrams look like (but is conventionally stipulated) (see Azzouni (2013b)).

[30]"Mathematical intuition" was, and continues to be, a big deal in philosophy of mathematics. See, e.g., Posy (2013, 127) for discussion, specifically about Kant's seminal (and influential) views of it.

With respect to the rest of this paper, the upshot of the foregoing analysis is this. Because of human (and machine) limitations, the (implicit) conventions of diagrammatic proofs force the field that such diagrams appear on to obey the finite-subregion-finite-cell property. In particular, each cell will have a finite number of immediately accessible cells (this is empirically established by our practice of requiring diagrams to be comfortably perusable by professionals). The same point holds of playable games in space and time, as well as mental playable games. This is because the games we play in our minds are limited in the same ways by our capacities for memory (and imagination) that the games that we play physically are.

7 The Turing Computability of Diagrammatic Proof Methods and, More Generally, of Playable Games

Everything is pretty much in place to establish that diagrammatic proof methods, ones that we recognize (intuitively) to be effective or mechanically recognizable, have Turing computable recognition procedures. There are (offhand) four strategies for establishing this that I'll sketch. The first is to bypass the work of sections 3, 4, and 6, by instead invoking a strong form of the Turing-Church thesis (where the intuitive notion to be identified with Turing computability is one of effective or mechanical calculation *of any sort*—not merely that of effectively calculable numeral functions). The second and third strategies go this way. The first (shared) step is to establish, applying finitary-capacity considerations for resolution (both physical and sensory) of humans and machines, that any game humans and machines play is a playable game in the sense of section 3. Then (this is the second strategy) one uses something like Gödel coding to transform any particular admissible game episode into a sequence of single-step transformations that can be directly seen to be Turing computable. Or (this is the third way), one follows the first shared step with a second step of instead establishing that Gandy machines can recognize the admissible moves of any playable game and then relying on the fact that anything computable by Gandy machines is computable by Turing machines. (See Gandy (1980).) The fourth way, finally, is to utilize the generalization of Turing computability to k-graphs, following Sieg and Byrnes (1996), by first establishing that Euclidean diagrammatic proofs can be characterized as the manipulation of k-graphs.

8 Algorithmic Reasoning Is on the Surface of Mathematical Practice

Let's return now to the issue this paper opened with. Proponents of the derivationist account, recall, hope to capture certain aspects of mathematical practice by attributing to mathematicians a grasp of the formal derivations that informal rigorous mathematical proofs correspond to. I'm continuing to leave aside the objection I mentioned that mathematical practice doesn't exhibit the kind of agreement among mathematicians that it purports to detect (*Agreement*). I'll take this up in section 9. For now, assume mathematical practice is as *Agreement* describes it to be (call the phenomenon, "public mathematical surety") and let's worry instead about the possibility that formal derivations are psychologically unavailable to mathematicians—even in the generalized sense described in the third observation of section 1—and so *they* cannot provide the needed public mathematical surety.

Broadly speaking, there are two strategies for establishing the psychological unavailability of formal derivations. I'll be brief about the first strategy because I've discussed it in other work (and others have too). It essentially turns on the fact that the formal derivations that must correspond to informal rigorous mathematical proofs are (1) too long to plausibly be what mathematicians are using (consciously or otherwise) to convince themselves that their informal rigorous proofs are valid. Furthermore, (2) these derivations don't have the appropriate epistemic qualities to provide surety to mathematicians. (Call (1) and (2) the "too-long objection.")

On (1): Once a formal system is specified—in particular, once the proprietary language of the nonlogical axioms is specified—the resulting mechanical step-by-step derivation is (in general) *extremely* long.[31] This all by itself makes it implausible that such derivations are grasped by mathematicians in any real sense because recognizing the validity of these derivations turns solely on their *fine structure* (the effective step-by-step recognition that each line follows by the rules from ones earlier in the derivation). This means that the line-by-line fine structure of extremely lengthy derivations (and *not* some global property that they syntactically possess) is what's supposed to be used by the mathematician to recognize the validity of their informal rigorous proofs and to explain *Agreement*. If so, the mere lengthiness of the derivations all by itself is a strike against them because there is no distilling—even syntactically—some more abstract global property that the mathematician can be seen to be grasping when he or she recognizes the validity of an informal rigorous mathematical proof on the basis of its corresponding derivation.

On (2): But this fine-structure fact about how the validity of derivations has to be recognized makes the derivations corresponding to informal rigorous mathematical proofs epistemically opaque in a significant way. Anyone who reads such a formal derivation can tell (after finishing it—if that's *possible*, I mean) that every step follows from the one before it. That is, anyone will grasp the validity of the derivation in (and only in) the fine-structure sense of the following: this step follows from that step, this next step follows from these steps, *and so on*. But, no global (aha!) experience of understanding how the proof "comes together" that explains why the initial assumptions result in the theorem they result in arises (or can arise)

[31] Pelc (2009) makes a big deal of this, with respect to finding computer-checkable derivations.

in this way. So this can't explain the common experience of understanding that ordinary informal rigorous mathematical proofs often provide.[32]

It's important to realize that early formalization programs (e.g., Russell and Whitehead's *Principia*), and many contemporary ones as well, require a foundational proprietary ideology (a set-theoretic one, category theory, one or another modal logic, etc.) that all derivations are required to be in terms of. This is also true of (current) projects of formalizing mathematical results in computer-checkable form. The proprietary language of the MIZAR system, for example, is formal logic plus set theory. But adhering to a requirement of a particular vocabulary (rather than allowing the relevant derivations to belong to any "nearby" algorithmic system) lengthens the needed derivations all by itself. The derivationist approach doesn't obviously require—without specific argument—a proprietary ideology. This means that the derivations it needs to explain mathematical practice can be much much shorter. I press this point after presenting the second strategy against the availability of formal derivations to explain mathematical practice.

The second strategy is to argue for an overgeneration problem that there are often multiple candidate derivations (in rather different mathematical ideologies) that correspond to an informal rigorous mathematical proof; but there is no principled way to decide which of these (if any) is being utilized by mathematicians to induce public surety. Furthermore, these candidate derivations are too different to provide the needed public mathematical surety as a group.[33]

The mutilated chessboard[34] is an illuminating example that Tanswell gives. Here is a description of it that's due to Black (1946, 157)—but I'm borrowing the quote from Tanswell (2015, 305):

> An ordinary chess board has had two squares—one at each end of a diagonal—removed. There is on hand a supply of 31 dominos, each of which is large enough to cover exactly

[32] I'm going along with this claim about the experience of ordinary informal rigorous mathematical proof for the sake of the objection; but the point needs serious nuancing before it can do the real work that's needed here. That ordinary informal rigorous proofs routinely provide an "Oh, I see!" phenomenology is exaggerated. (See Feferman (2012) on this and Azzouni (2013c).) Perusing ordinary informal proofs is a strikingly heterogeneous *experience* and so is the corresponding cognitive phenomenology accompanying that experience. Some steps may be facilitated by algebraic maneuvering that one knows only by virtue of certain memorized rules and not by anything like a feel for an implication relation between the statements, or anything conceptual. Other steps (quite often) are simply taken on authority ("Oh, that's probably right"). Even a whole, *Oh, I see how this goes*, may actually be fairly piecemeal in the real understanding it provides. It's a perennial (and horribly damaging) philosophical *myth* that mathematical proofs involve that many conceptual or implicational connections that are even *candidates* for *a priori* connections. The same point holds of the phenomenology of logical inference itself. See Azzouni (2005) and Azzouni (2008) on this. I take the point up about the heterogeneity of mathematical proof, and develop its implications, in section 9.

[33] As I've mentioned, the first objection originally drove me from the "derivation-indicator view" of mathematical proof," at least as a view that the role of the indicated derivations aren't purely normative, but additionally are supposed to help explain *Rigor*, *Correctness*, *Agreement*, and so on. See Azzouni (2005, 2009a). The second objection is recent and due to Tanswell (2015).

[34] See, e.g., Robinson (1991), Black (1946), or Gardner (1988).

> two adjacent squares of the board. Is it possible to lay the dominos on the mutilated chess board in such a manner as to cover it completely?

And this is the solution, quoted from Gardner (1988)—but I'm again taking the quote from Tanswell (2015, 305):

> It is impossible ... and the proof is easy. The two diagonally opposite corners are the same color. Therefore their removal leaves a board with two more squares of one color than of the other. Each domino covers two squares of opposite color, since only opposite colors are adjacent. After you have covered 60 squares with 30 dominos, you are left with two uncovered squares of the same color. These two cannot be adjacent, therefore they cannot be covered by the last domino.

Tanswell (2015, 306) points out that this proof "has been formalised a number of times in different systems as a good example of informal reasoning that is tricky to capture formally." In particular, one approach is by reconstructing it set-theoretically, by representing the board as sets of coordinates, defining an adjacency relation on the sets of coordinate, and a tiling that uses the relation. A different approach uses inductive definitions for the set of dominoes and the tiling (and this way proves crucial properties by rule induction). A third approach, finally, uses an ideology of states of the chessboard and actions of placing dominoes on the board, and it approaches the problem using finite-state machines.[35]

The point of the objection, as I mentioned, is that if the derivationist account is supposed to explain the public surety of informal rigorous mathematical proofs in terms of the mathematician (in some sense) grasping derivations corresponding to those proofs, it faces an objection if there are too many *sorts* of derivations that are quite different in their ideological resources. The point is to explain *Agreement*, after all—but if the underlying derivations are *quite* different, we face a problem of explaining why mathematicians would think they're looking at the same informal rigorous mathematical proof, since it isn't the *surface appearances* of proofs that's supposed to be explaining *Agreement*.[36]

Both challenges to the derivationist account (and, correspondingly, the force of the mutilated chessboard example), however, turn on implicitly saddling that approach with a *language-based* requirement on derivations: the relevant derivations must occur in formal systems which are *purely* language based.[37] But this restriction seems badly motivated because the crucial value of mechanical recognizability isn't

[35] Bancerek (1995), Rudnicki (1995), and Subramanian (1994)

[36] We could attempt an error theory here: mathematicians are laboring under the false illusion that their informal proofs can be "filled out" the same way. But this "save" faces tension because the formal derivation is supposed to be *why* they think the informal proof is valid. I should add that Tanswell (2015) presses the overdetermination objection against derivationists in a different way than I do here—he offers a dilemma that turns on whether the derivationist account is agent dependent or independent. Regardless, my response to the objection (however formulated) is the same.

[37] When coupled with one or another foundational program, a *specific* language-based vocabulary is required. This, *in some cases*, may yield derivations (unique up to size) that avoid Tanswell's objection, but nevertheless will allow a stronger version of the too-long objection.

restricted to language-based algorithmic systems. In point of fact, one basic way of directly characterizing effective computability is in terms of Turing machines, which aren't language based.

Any translation of an algorithmic proof procedure that isn't purely language-based to one that is will always require a serious lengthening of the proof, as well as a multiplication of alternative choices in ideology (nonlogical vocabulary). This is because of the need to axiomatize the fields of diagrammatic algorithmic procedures, and their properties, as well as the conventionalized properties of the game pieces and the admissible operations on configurations of the fields. Notice that this is the case whether, in original informal diagrammatic proofs, the spatial relations play semantic roles (as they do in the Euclidean diagrammatic tradition) or whether they function in a purely calculational way (as they do, say, with two-dimensional matrix diagrams in linear algebra).

In diagrammatic proofs (as with Turing machines), on the other hand, the spatial relations are instantiated only in the proof procedures themselves: they aren't part of what's demonstrated but instead are among the items *by which* the demonstration occurs. That is, they play the same role that distinctions among vocabulary items play in the characterization of the syntax in the metalanguage of an axiomatic system.[38] In transcribing any such diagrammatic proof to a language-based form, therefore, proof-theoretic content that's strictly speaking extraneous to the informal rigorous proof *must be* introduced into the derivation itself. That is, content must be moved from the proof-theoretic mechanisms of the diagrammatic proof procedure and be made part of what *is* proved in the course of the proof. To repeat, that there are, in general, *many* ways to do this is hardly surprising.

As a result, debates over the force of the too-long objection and the underdetermination objection have been revealed to be largely nomenclatural. Suppose the "derivations" relevant to the derivationist account are mechanically recognizable sequences of inference steps in a particular formal alphabetic language. Then the derivationist account faces Tanswell's challenge because diagrammatic proofs (in particular) require axiomatic elimination of their proof-theoretical properties that rely on spatial relations. More accurately (since formal proofs rely on the spatial relationship of concatenation and, relatedly, the visual capacity of umpires to distinguish kinds and tokens of the alphabet of any formal language), they require the transformation of proof-theoretical properties that rely on space in ways other than sheer concatenation (and a conventionalized distinction between vocabulary items) into proof-theoretical properties that rely on space only with respect to concatenation (and conventionalized "resemblance"). Similarly, the derivationist account faces the too-long objection because diagrammatic proofs can't be taken largely as is (merely refining the algorithmic procedures mathematicians are aware of and employing) but requires a translation to one or another language-based derivational system. If diagrammatic proofs are characterized directly as effectively

[38]See the discussion of "framework facts" with respect to the Euclidean diagrammatic tradition in Azzouni (2004, 125, and what follows).

recognizable, then what's required to make them "fully rigorous" is, at best, filling in—the relevant derivations can truly be seen as ones "indicated" by the informal rigorous proof mathematicians actually give. In particular, the ideology of the rigorous informal mathematical proof will need little or no supplementation.

So if what's crucial to mathematical proof is only that such proofs be mechanically recognizable, regardless of *whether* this is managed in a pure language-based way or diagrammatically, then the "derivations" needed to explain public mathematical surety can be understood as including diagrammatic proofs. In this case both the too-long objection and Tanswell's challenge vanish because, as I indicated in the last paragraph, "informal" rigorous mathematical proofs can themselves (at least to a very large extent), and on the basis of proof algorithms used in informal mathematical proofs, explain properties like *Rigor*, *Correctness*, and *Agreement*.

What about the other conditions described in section 1 that the derivationist account is supposed to handle? *Content*, to a very large extent, lapses as a requirement on the derivationist account (because the need to translate informal rigorous mathematical proofs to derivations with substantially different ideology is gone). *Techniques* remain a substantial matter for the derivationist account to discuss, and the explanation of it is, indeed, intricate (although I've already raised the relevant points earlier in the paper—in section 6). One shows that particular "informal" proof procedures are effective, and one shows, given the mathematical interpretation they have, that they are sound. There is more to say about this—I'll do this in section 9.

9 The Heterogeneity of Informal Rigorous Mathematical Proof

To some extent, the preceding discussion has focused on recognizing nuances in the phrase "informal rigorous" that occur in the label, "informal rigorous mathematical proof." One claim I've tried to establish is that "informal" contrasts with "formal," apart from abbreviatory shortcuts, only in that the adjective "informal" allows algorithmic systems that aren't purely language based, whereas the latter doesn't. This provides content to the phrase "rigorous" in "informal rigorous mathematical proof." An informal *rigorous* mathematical proof is "rigorous" in the sense that mathematicians are convinced that an effectively recognizable derivation (that's in the ideological neighborhood of the proof they've inspected) has been shown to exist by that proof. The effectively recognizable derivation, that is, isn't very far from what's already present on paper (or computer screen, or whatever). This view becomes much more plausible, once the surface algorithms of ordinary mathematical proofs are seen to possess the epistemically valuable property of being mechanically recognizable and once the local context-dependent background material alluded to by De Toffoli and Giardino and by Larvor is seen to only amount to "filling in" aspects of the already visible algorithms used in ordinary proofs.

If the foregoing were the only relevant considerations, we'd be done. Mathematical practice would be fully explained in terms of algorithmic systems, where such systems go far beyond the language-based ones traditionally taught in logic classes everywhere. The derivationist account would have successful responses to the too-long objection and the overgeneration objection. In particular, that computer-checking proofs involve such a long process of putting a standard textbook proof into the appropriate form (that a computer can check) would also be explained by the fact that current systems (like MIZAR) are also language based and use a proprietary vocabulary which greatly lengthens proofs. Some day in the far (or, possibly, near) future, one imagines that Gandy *robots* with appropriate machine vision will be checking our diagrammatic proofs directly—and in pretty much the ways professional mathematicians do already.

As I said, we'd be done—except for one large wrinkle. This is the point I mentioned in footnote 32. A large diet of mathematical proof, especially in functional analysis and the like, gives the impression of enormous heterogeneity rather than that of all the proofs (in a subject area of mathematics) belonging straightforwardly to one algorithmic system, or even a set of such systems that are tightly constrained to one another in their nonlogical vocabulary.[39] Rather, especially if one realizes how much topology (for example) shows up in analysis, proofs *of all sorts* are being used in all subject areas (although, sometimes, only theorems—without their proofs—explicitly appear). Sheer algebraic computations, diagrammatic proofs (of various sorts, using various conventions and resources), conceptual connections, facts about notation, and any and all of this can occur in an informal rigorous mathematical proof. Rav (1999, 12) *largely* puts the point correctly. He writes:

> Proofs employ deductive reasoning; so do judicial rulings. In both cases logical inferences cement sequences of topic-specific claims and considerations.

This isn't quite right as it stands because what's doing the "cementing" (most of the time) aren't logical inferences but instead the notions of truth and meaning[40]—in the various kinds of proofs that are taken to be about a certain subject area (and other subject areas). That is, the cementing is managed by the inter-algorithmic use of common terms that are understood to mean the same things and, more importantly, to *refer to the same things* (to *be true of* the same things). For example, the definite integrals that appear in matrices refer to the same old functions and, via those functions, to numbers that are studied, say, in any *Advanced Calculus* course,

[39]This is even true of a subject area as apparently restricted as number theory. Number theory, after all, isn't just Peano arithmetic. *Everything* in mathematics comes into play—as the recent proof of Fermat's last theorem illustrates rather dramatically.

[40]Although Rav (2007, 315), recall, summarizing his discussion of several examples of informal rigorous mathematical proofs, writes "I hold that mathematicians' manner of reasoning and inferences are based on *meanings* and an informal notion of *truth* that a formal deduction calculus cannot capture" (italics his).

and those numbers, of course, are the same old items we've (for millennia) been *counting* with.[41]

What's required to justify *this aspect* of mathematical practice is a collection of soundness proofs. Suppose a kind of proof procedure is imported into a mathematical subject area (one that applies to semantically significant items of one sort of another—*notation*, to use a general term that's to indifferently cover diagrammatic configurations and alphabetic terms). And suppose items of that notation are identified with items of notation already in the subject area (algebraic formulas identified with certain diagrammatic items, say, numbers characterized in one way with numbers characterized in a quite different way). Then what has to be shown is that the one way of mathematically characterizing items (proving results about them in one algorithmic system) is compatible with the other ways of doing so. That is, the indiscriminate *borrowing* of mathematical results (and informal rigorous mathematical techniques) that occurs in standard mathematical practice has to be shown to be coherent.

We thus have found our way back to the suggestion made by Larvor (2012, 723) that was quoted several times at the beginning of this paper: "the cost is that we have to abandon the hope of establishing a general test for validity," although we've gotten here by means of very different considerations.

And just because the considerations that have given rise to this earlier concern are *so different* from the ones Larvor (and others) have raised, there is a response: There are two ways to establish the coherence of this *holistic* mathematical practice. The first is to transliterate *the entire practice* into one in—one or another—formal system. Doing so allows a straightforward interpretation of all the apparently disparate algorithmic systems in a single domain—and that straightforwardly shows (via a soundness proof relative to that interpretation) the coherence of the entire mathematical practice. Notice that the facts that there is more than one way to do this (Tanswell's objection) or that the result involves the replacement of relatively short effectively recognizable proofs with extremely long (practically unsurveyable) proofs are irrelevant. The mechanical effectiveness of informal rigorous proofs and, relatedly, the public surety of ordinary mathematical practice are *not* being established by this transliteration.

There is a qualification, of course. A lot of mathematical practice involves seriously alternative mathematics: intuitionistic set theory, for example, or more

[41]Rav (1999, 16) notes that matrix theory isn't axiomatized. That's right—how could it be? Just about *anything* can occur within a matrix diagram (integrals, series of integrals of functions, etc.), and just about anything can be "done to" matrix diagrams or to sequences or series of matrix diagrams (powers of matrices, infinite sums of matrices, and so on). See, e.g., Gantmacher (1960, 1977) for details. (Also recall the discussion in section 5 of Feferman's examples—the same kinds of phenomena are coming up in this subject area.) Notice how the heterogeneity of mathematical proofs follows from the case of matrices alone: all sorts of mechanical-recognizable proof procedures are imported into matrix theory by the mere importation of the notation for these things into matrix theory. (One can imagine, e.g., actual geometric curves, representing functions—or other diagrammatic objects—occurring *in* the boxes of a matrix.)

generally, mathematics based on systems of logic other than classical ones. But mathematical practice acknowledges this: mathematical results aren't indiscriminately applied across alternative logical frameworks or across disparate subject areas (e.g., alternative set theories). And so the phrase "the entire practice" used above doesn't include these nonstandard aspects of mathematical practice. There are numerous alternative branches of mathematics that largely don't intersect.

The second way to establish the coherence of the holistic practice of borrowing results and techniques from alternative proof traditions (that nevertheless are treated as about the same subject areas) is to provide piecemeal soundness results: this is a matter of leaving the algorithmic practices as is, but (in the metalanguage, as it were) providing soundness proofs relative to a shared interpretation.

Where does this leave Larvor's remarks about "giving up on a general test of validity"? It tames its significance. There certainly *is* a general test of validity (i.e., a general recognition procedure—which is how I interpret Larvor's use of the phrase "general test of validity"). This directly follows from the coherence results, provided we're in a first-order classical setting, where completeness proofs for validity are available. Nevertheless, in practice one works with specific ways of establishing validity—the ones generated by the specific algorithmic systems being used.

One last bit to round out this section. Recall the concern (that I've set aside for so long) that the *datum* of mathematical practice that this paper has been dedicated to explaining isn't a real one. Mathematical practice *does* exhibit significant "sociological drift," the objection goes, new approaches to mathematical proof are invented all the time, and they are often controversial.

A comparison with rule-governed games is helpful. Here too, new games are invented all the time, and these games are bewilderingly variable. Nevertheless, their game episodes are all mechanically recognizable. In *this* sense, new games are "the same old thing" all over again.[42] I can put it this way: In one sense, games are always evolving; lots of new games are always emerging with new kinds of objects (game pieces) and new rules to enable opponents to compete in new ways. In another sense, it's just the same old thing all over again: here are the rules, and here is what you do to play a game.

It might seem that this analogy isn't particularly helpful. Games, after all, are generally content-free—unlike mathematics. That they are all governed by rules that allow game episodes to be mechanically recognizable hardly introduces much of a constraint. In the case of mathematics, the corresponding point looks like it's only a Pickwickian victory. Describing "reasoning" as algorithmic, and noting that all mathematics is "proof as usual"—intuitionistic, quantum-logical, various extensions of classical logic, and so on—is to leave out exactly what's significantly different about all this mathematics: how different these branches of mathematics are in content.

[42]Of course there are games that don't involve algorithmic recognizability. "Game" is a notoriously broad word. I'm simply leaving those "games" out of consideration: the remaining class of games is still bewilderingly variable.

I agree with the Pickwickian charge, but the reasons for agreement are subtle enough to deserve further discussion. To see the implications of any algorithmic system (and, in the case of a mathematical system, to understand seeing the implications as a matter of recognizing the theorems of a set of premises—relative to a logic) is to recognize what follows in an "if ... then" (antecedent/consequent) sense. Given such-and-such (where such-and-such includes both the (sometimes implicit) logic of a proof procedure and the nonlogical characterizations of a subject area), then so-and-so follows. As a general characterization of mathematical reasoning, this is content-free because anything, nearly enough, can be incorporated into the antecedent.[43]

This yields, however, an ecumenical characterization of mathematical practice, but at the cost of leaving out the significance of *applied* mathematics. Given that the application of mathematical discourse is to a subject area that's characterized, nearly enough, by a grammatically *indicative* discourse, and given that the applied mathematics is to facilitate the inference of empirical consequences of that discourse, it follows that we don't want those consequences to occur trapped in conditionals. We need to use the mathematics to draw results from empirical discourse of the form C, that we can apply, and not consequences of the form, $A \rightarrow C$, where A is the antecedent mathematics presupposed in the deduction of C from the empirical discourse.

What this requires, in turn, is that applied mathematics (as a whole) needs to presuppose the same logical framework as the empirical sciences it's applied to. That forces a certain amount of shared content across the branches of mathematics that are applied; in particular, it requires a sharing of the background inferential framework (e.g., first-order logic).[44]

10 Conclusion and Summary (and Some Further Thoughts)

The original version of the derivation-indicator view ("the derivationist account"—as Tanswell labels it) was intended to explain *Agreement* and other aspects of mathematical practice in a largely non-sociological fashion. The mechanical recognizability of formal derivations was supposed to be graspable by mathematicians

[43]Anything? Yes, the antecedent can, for example, yield a trivial set of consequences: everything or nothing.

[44]There is a bit packed into this and the last paragraph that I really can't get into here. The relevant "talking points" are what I've called "the external discourse demand"—that required are shared notions of logic and truth across the mathematics and sciences that are brought to bear on one another (Azzouni (2010), 4.7–4.9)—and also, what might be called "the assertional model" of scientific discourse. That is, using scientific discourse to represent (even when idealizing) aspects of the world, and to draw consequences about one's representations of the world, requires "detaching the antecedent." One can't do this by "conditionalizing" one's results relative to assumptions (Azzouni (2009b), sections 1 and 2).

in a sense that would be sufficient to explain *Agreement*. Restricting the relevant "derivations" to those occurring in purely language-based formal systems (with proprietary vocabulary), however, faces two serious objections because both the logical and nonlogical vocabularies of informal rigorous mathematical proofs are so different from that in language-based formal systems: the too-long objection and the overgeneration problem. The solution I've argued for in this paper is to understand the relevant derivations to belong to algorithmic systems that needn't be pure language-based ones. This allows the indicated derivations to be near or at the surface of mathematical practice, in the sense that the vocabulary (and proof procedures) actually used by mathematicians is already close to what the needed algorithmic systems look like. The key to responding to the too-long objection and the overgeneration problem is to recognize that what's essential to explaining *Agreement* is recognizing the mathematician's use of effectively recognizable proofs, and not by employing mappings of these ordinary proofs to derivations in a formal language with a proprietary vocabulary (e.g., that of set theory).

In the foregoing, I've been stressing diagrammatic aspects of informal rigorous proofs as those aspects of these proofs that especially give rise to the too-long and overgeneration objections—but it's important to realize that what's important to avoiding these objections is that the surface algorithms of informal rigorous mathematical proof (whether diagrammatic or not) not be replaced (as they are when such proofs are reduced to derivations in standard formal languages). So, for example, one does not replace the computational *decision procedures* we have with the counting numbers—addition, multiplication, etc.—with Peano arithmetic. Nor does one replace the rules of thumb (the *recipes*) for evaluating integrals with anything else. These (various) recipes are tied together either by background inferential facts (about how these recipes have been derived) or by the background interpretation of the notation. But in this way the surface algorithms that mathematicians are using (and that are convincing them collectively of the results on the basis of these algorithms) are preserved.

There has long been a picture of informal rigorous mathematical proof according to which any such proof is completed solely by "filling in" missing steps. (This model appears at least as early as Descartes.) Because this model is a semantic one—necessarily, because we had no real syntactic model for reasoning until the late nineteenth century—it pushes anyone in its grip to look for the appropriate derivations to occur in a mathematical framework that's "conceptually complete." This in turn motivates foundational programs in which all the appropriate concepts are given (e.g., set theory). Turning one's attention, instead, to effectively recognizable derivations (of *any* form) allows the needed derivations indicated by informal rigorous proofs to be conceptually heterogeneous. This does raise the "unification issue" that I take up in the next paragraph and summarize the results of this paper on.

If informal rigorous mathematical proof is allowed to be proof-theoretically heterogeneous, then a coherence issue looms. One wants to know that the identification of mathematical "objects" across different proof-theoretical practices (various functions, operators, numbers, etc.) is the "same" in the sense that theorem results won't conflict. Transcription of informal rigorous mathematical proofs into a (single) formal language with an intended interpretation can help with that issue. Doing so for *this* purpose doesn't face the too-long objection or the overgeneration problem since this transcription isn't meant to explain *Agreement*, *Rigor*, and so on. (Only *Techniques* is being partially explained in this way.)

I'll close with some explicit remarks about the opening concerns of this paper. That alternative forms of reason—*pace* Frege—in principle exist is an indisputable result of the twentieth-century research in logic. One way to establish that first-order logic (or something in its neighborhood) is the appropriate logic for reasoning is to provide a neutral (a logic-independent) notion of "implicational content" and use it to argue that first-order implication is an inferential operation that genuinely adds no "content": Given A $\rightarrow$ B, where "$\rightarrow$" stands for first-order implication, B has no (logical) content beyond that contained in A. I won't give further details here.[45] But with that assumption in place, an argument can be mounted that applying mathematics places a powerful constraint on the tool of inference for the whole package: applied mathematics and the empirical science it's applied to must have the same background logic, a first-order one.

Regardless of the implications of the remarks of the last two paragraphs, the considerations raised in this paper undercut the claim that the evolution of new informal rigorous mathematical proof techniques all by itself reveals that different notions of "validity" are being developed in ordinary mathematical practice.[46]

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[45]One way to do this, however, is to introduce logical connectives corresponding to implication operators that are minimal with respect to the propositions they imply. See Azzouni (2005, 34–39) for discussion.

[46]My thanks to Silvia De Toffoli for drawing my attention to De Toffoli and Giardino (2014, 2015), as well as for some useful conversations on these topics. My thanks to Otávio Bueno, as well, for spirited conversation on this topic as well as for some important disagreements.

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Mathematical Theories as Models

Michèle Friend

1 Introduction

The thoughts in this paper follow a suggestion by Reuben Hersh that pluralists think of mathematical theories as models of other parts of mathematics. *Through this lens*, the totality of 'mathematics' is then a game[1] of interpretation of one theory by another. As a discipline, mathematics is *sui generis*.

This disturbs more traditional notions in the philosophy of mathematics, where mathematics is grounded in necessity, the empirical world of observation and experience, truth or ontology, where mathematics forms a unity that is consistent, and where mathematical explanation ends with undeniable elementary truths.

As a pluralist, I explore the suggestion of thinking of mathematical theories as models. The disturbance will be looked at in detail, as well as ways of restoring a sense of balance.

[1]The word 'game' suggests a formalist conception of mathematics, and this conception is not one I wish to explore in this paper. To distance the formalist view from the view here: formalism is the view that all there is to mathematics is formal play, within certain rules. For Hilbert the rules included the finisitic constraint on the signs and manipulations and the consistency of each theory. In contrast, the view examined here is that meaning and interpretation in mathematics is had *partly* by one theory being translated into the language of another and then finding out the extent to which the theorems, concepts, or ideas of one theory can be recovered by the second. The emphasis is different, there is no requirement concerning formal representation, and there are no rules imposed from the outside.

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2 Mathematical Theories as Models

There are several mathematical theories. 3.1 **How we count them depends on how we individuate theories**. Some pairs of theories are equivalent to each other. 3.2 **Which theories are equivalent to which other theories depends on what we look for, and what we ignore, in our equivalence relation**. Moreover, there are translations from one theory to another. 3.3 **What is counted as a successful translation depends on the expressive power of our language and whether we more intuitively think that concepts have been preserved in the translation** (**call this 'loyalty of translation'**). While some pairs of theories are equivalent, others contradict each other. 3.4 **Which theories contradict which other theories depends on how we characterise contradictions**.

In the above paragraph, the caveats in bold and their numbers correspond to subsections of the next section of this paper. In the third section, we can then follow the suggestion by Hersh[2] and examine the thesis that, in the practice of mathematics, mathematical theories are used to model, or interpret, each other. I give a description of mathematics that follows this suggestion. But we should remember that models and interpretations are also used to contrast with each other. These two exercises, modelling and contrasting, done as they are with precision *up to the standards of rigour, definition, expressive power of the language and contextual understanding of the time*, are an important *part* of what constitute objectivity in mathematics.

The risk in writing sections two and three is that they leave us with a sense of the *relativity*, or *shifting truth*, of mathematics. This is particularly the case for those among us who are more monistically (should I write 'monotheistically'?) or platonistically minded, those of us who pine for *certainty* and who anchor our certainty in a *fixed*, *consistent*, and *unified* ontology and truth, or who anchor our certainty in knowledge based on *immediate primitive* and *absolutely obvious* starting ideas, or who are convinced that explanations must come to an *end*. The contrast in philosophical expectations is explored in section four.

To alleviate the unease, in section five, I shall discuss why this conception of mathematical theories each modelling the other, nevertheless, supports a sense of objectivity, and in what sense it does so, and all of this in the absence of any traditional foundation, unified, consistent ontology or unified and consistent set of realist (independent) truths of mathematics. The advantage of such an account of objectivity in mathematics is fidelity to the practice of mathematics and the *prima facie* excusing of the practice of borrowing results from one theory in mathematics to help with another (possibly contradictory) theory in mathematics.

[2]The suggestion was made on the occasion of a conference on mathematical pluralism in Kolkata, in December 2015. The conference was organized by Mihir Chakraborty. Hersh's paper had the title: Pluralism as Modelling and as Confusion.

Such borrowing can be found in any scientific discipline, but it is often not faced directly but instead is clothed in vagueness, ambiguity, *ceteris paribus* clauses, methodological or measurement shortcomings, calculated error margins, and contextual imprecision. Thus, the considerations of this paper apply to the sciences as well. The considerations of this paper bring a sense of objectivity to mathematics that is loyal to the practice, but it also brings a normative pressure to mathematicians and philosophers of mathematics to be more precise about the parts in bold above. The account also spills over to upset a more conventional conception of explanation—one where explanations end or, better, one feels justified (not guilty) in terminating an explanation. On this account of mathematics and science, explanations grow. They grow in different directions and they sometimes grow in unexpected directions. The growth of knowledge, understanding, and explanation is developed in the concluding sixth section.

3 Caveats

3.1 Individuating Mathematical Theories

In the footsteps of Hilbert, one of the favorite ways of many modern philosophers to individuate theories in mathematics is by a set of axioms together with rules of inference. While this is possible for some mathematical theories, it is not suitable for all.

Model theory, for example, has no axioms. Nor does the calculus. Model theory is better individuated as a general theory about the notion of model in mathematics and how to compare models to each other. So, for example, we might distinguish a model with an infinite domain from a model with a finite one. In the former, it is possible to find infinite proper subsets of the domain that can be placed into one-to-one correspondence with the original domain. In the latter, there is no such proper subset of the domain. The calculus is individuated by what we are able to do with it: calculate accurately the space under curves or calculate the optimum curvature to cover a space while bearing a load. Unless needed for the argument of this paper, and unless specified, I shall default to the idea that mathematical theories are individuated by their axioms and rules of inference.

3.2 Equivalence of Theories

There are different ways of determining the equivalence of theories. An equivalence relation is, after all, a relation that says that two things (theories in our case) are the same in some respects.

3.2.1 Two theories are extensionally equivalent iff they are about the same objects. Objects are naïvely thought of here as a domain or set over which first-order variables vary.

The equivalence relation between objects is reflexive, symmetrical, and transitive. In formal notation:

R is reflexive iff $\forall x\ [Rxx]$.

That is, for any object that you choose from the domain or for any object that is the member of a set, the relation bears back on that entity.

R is symmetrical iff $\forall x\ \forall y\ [Rxy \rightarrow Ryx]$.

That is, for any two (not necessarily distinguished, that is, you might pick out the same object twice) objects in a domain or members of a set, if the relation bears in one direction then it bears in the other as well.

R is transitive iff $\forall x\ \forall y\ \forall z\ [(Rxy\ \&\ Ryz) \rightarrow Rxz]$.

That is, for any triple of objects (not necessarily distinguished from each other) if the relation bears from the first to the second *and* from the second to the third, then it bears from the first to the third.

Such an equivalence relation will determine an equivalence class, that is, a class of sets of objects that are equivalent to each other.

Two *theories* are extensionally equivalent iff they have the same extension, that is, they pick out the same objects in the sense that the set of objects of one theory are equivalent to, interchangeable with, inter-substitutable with, the objects of the second theory. They might, of course, say very different things about those objects. That is, the theorems of the theory might not be the same.

3.2.2 Two theories can be syntactically equivalent. Two theories are syntactically equivalent iff they have the same inference rules. They will then generate the same theorems under some suitable translation between the languages.

3.2.3 Two theories can be semantically equivalent just in case, under a suitable translation, they preserve the same semantics. This might be in the form of truth-values or models, where 'models' is understood in the strict sense of 'model' in model theory.

3.2.4 A stronger notion than that of syntactical or semantic equivalence, one that combines both, is that of definitional equivalence. For this, following Lefever (2017, 6), we start with the notion of a sort and logical connective-preserving translation from one theory to another. We can broaden this to include preserving whatever it is that concerns us, so we could also include operations, quantifiers, valuations, and so on depending on the sophistication of our language. We then think of an interpretation of one theory by another as preserving the same *derived formulas* or *truths* or *models* satisfying sets of formulas, under the translation. Again, 'model' here is 'model' in the sense of model theory, *not* in the broader sense of this paper where a theory in mathematics models another theory. If we have two theories, with a translation between them that preserves whatever it is we are concerned about in the language, and we generate the same formulas, truths, or models (in the strict model theory sense), then the two theories are definitionally equivalent.

As we can see, there are many respects in which two theories can be equivalent, and a lot depends on finding or generating the right sort of translation.

3.3 Loyalty of Translation Between Theories

Translations can be inadequate, perverse or highly artificial. Here is a perverse one: the language of my new theory consists in one symbol 'TPA,' intuitively standing for "true proposition of arithmetic" in the sense of a true well-formed formula in the language of Peano arithmetic. I can translate all of Peano arithmetic into my perverse TPA language. Every derivable formula of Peano arithmetic is translated as 'TPA'. This is a one-way translation and is inadequate, to the point of being perverse.

A translation can be simply inadequate. It is possible to 'translate' every formula of first-order logic into the language of propositional logic, and *vice versa* trivially, if the language of first-order logic still includes proposition letters. However, this is inadequate because there are subtleties captured in first-order logic, missing in propositional logic. This matters when deriving conclusions. There are some arguments that are valid in first-order logic, that are invalid under their translation into propositional logic. In this sense, the language of propositional logic is inadequate for translating formulas of first-order logic.

Translations can also be highly artificial. 'Artificial' is more of a term of art, since what might appear as artificial to some might seem more natural to others. Witness the names given to some entities in mathematics: irrational numbers, imaginary numbers, nonstandard arithmetic, and so on. Louis Kaufmann once showed me how to translate knot theory into set theory. It is possible. But the problem with the translation is that the truths of knot theory are not true (after translation) in set theory and *vice versa*. The set theoretic notion of membership does not translate well. However, this is also revealing. It shows us an *edge* between knot theory and set theory. We could add special 'knot' axioms to set theory and modify some of the normal, standard, natural, set theoretic axioms to get a definitional equivalence, and this could even turn out to be an interesting exercise.

Sometimes it is quite useful to translate from one language to another even if there is some disloyalty. This is because some things show up in one language that are ignored in another. We see this a lot now in mathematics. A problem that cannot be 'solved' in one theory is more easily solved after translation and using the apparatus of the second theory. This is because by their very nature, mathematical languages draw out some ideas or concepts and obscure others. By changing the language, we change what we see clearly and what we can no longer see.

3.4 Contradicting Theories

Theories can contradict each other. This is important for comparative purposes. They can contradict each other in the following senses: 1. one theory can have an axiom, where another has the negation of that axiom, or an alternative axiom that brings with it other possibilities. For example, think of the many geometries and their different parallel postulates, or ‘axioms.’ 2. A theorem might be derivable in one theory and its negation is derivable in the other. Here we have a syntactic contradiction. 3. The models (in the model theory sense) of one theory, or satisfying sets of formulas in one theory, might be different from the models of another. Here we have a model theory semantic contradiction. There are others. For more detailed discussion, see Friend (2017).

The contradiction-comparison is important in the following way. It shows us that we cannot do as we please in mathematics. There are restrictions and blockages. This correction or impossibility gives us a sense of objectivity in mathematics. We shall return to these two sections hence. Comparisons of equivalence or contrast can only be done *up to the standards of rigour, definition, expressive power of the language*, *and contextual understanding of the time*.

3.5 Standards of Rigor and Contextual Understanding

Standards of rigour change. Usually they increase, at least in theory, for mathematics in general but might decrease for reasons of expediency,[3] pragmatism, practicality, and so on. They increase under the pressure of suspected problems. An extreme problem, in this sense, is trivialism[4] or paradox, but there are more subtle ones, such as when we decide that a concept or definition is, in fact, ambiguous. The ambiguity only surfaces in some circumstances. Or, we might think that a new formulation of an idea or the translation of an idea no longer quite corresponds to the original idea or proto-concept. For example, someone not educated in the ways of set theory might contend that the representation of finite numbers in ZF set theory is too distorting. Under such pressure, we are encouraged to be as precise as possible.

In discussing these caveats, I am not trying to make a catalogue or an exhaustive list. I think that this would be tedious and counterproductive here. Rather, I am trying to make readers sensitive to the myriad of ambiguities and issues that underlie the exercise of interpreting and contrasting one theory with another.

[3]For example, in some applications of mathematical theories to physical reality, we literally use an inconsistent mathematical theory but are cautious never to ask the questions that draw out the inconsistency. For example, ‘renormalisation’ is a procedure in quantum electrodynamics where we simply eliminate the infinite quantities that occur during certain sorts of calculation. We do this because in the physical world we are measuring there are supposed to be no infinite quantities, and mathematically, this means that the normal arithmetic operations such as addition or division give results that, again, are not supposed to occur in the physical measured world.

[4]In the sense meant here, a trivial theory is one where every formula in the language is true or derivable. In a classical or intuitionist theory, if we generate or discover a contradiction that is derivable or true in the theory, then we can derive any formula and its negation. This is also called ‘explosion’ of the theory. Trivialism is what ensues after explosion.

4 Mathematical Theories as Models of Other Parts of Mathematics

Mathematical theories, individuated as per Sect. 3.1, can be used to model, or interpret, other theories. What does this mean? What does the discipline of mathematics look like if we describe mathematics in this way?

We are familiar with reducing theories such as the set theories, the category theory, and the type theories. These are reducing theories in the sense that much of mathematics can be reduced to them. That is, they can be interpreted in them. We can give the set theoretic version of a theory of relations, for example. Or we can give the category theory version of Euclidean geometry. When we reduce a 'small' theory to a 'large', i.e., reducing theory of mathematics, we translate the small theory into the language of the reducing theory and then use the apparatus (set of concepts) and manipulations available to us in the reducing theory to mimic, or derive, the same results as we had in the original theory; or more interesting, we might lose or distort them.

Already this can be interesting because in the very act of translation and in the very loss of some apparatus and manipulations, or in making available new apparatus and new manipulations, we might well find some of the mimicking or recovering of results more difficult or easier. We might 'see' things differently. Language, apparatus, and available manipulations guide us to notice or ignore. They can even change what it is that we consider to be normal or natural and what we take as a legitimate extension of a concept.

Such modelling and interpretation does not only have to be in the form of reducing one theory to another. Different reducing theories can be used to model and interpret each other. For example, within category theory, there is a category called 'set'. Its objects are sets and its arrows are subset, union, intersection, powerset, and so on. We can then use category theory to *interpret* a set theory. We can do the reverse and interpret category theory in set theory. To do this, we form a set of categories within set theory and so use set theory to *interpret* category theory. So, the big foundational theories can play this interpretation game with each other.

Furthermore, we can even use a 'small' theory to interpret a foundational one or just another reducing theory. For example, we might want to hone in on the algebra *of* set theory or the algebra of knot theory. We thereby draw out certain features, the algebraic features, of set theory or knot theory and ignore the rest. This allows us to notice algebraic features that were obscured by language or the apparatus or manipulations of the interpreted theory.

The advantage of interpreting one theory in another is exactly the drawing out and obscuring. The exercise begins with the translation. But it includes bringing to bear the resources made available to us in the theory used for the interpretation. When we translate and interpret one theory in another and 'see' it differently, we are able to solve problems that we could not solve otherwise. One famous example is when Lobachevsky solved the problem of finding exact solutions to the indefinite integrals of Euclidean geometry using his imaginary geometry.[5] This example dates back to the eighteen thirties, but this shifting between theories, through a translation, is done quite frequently in current mathematics. It is almost a necessary exercise for solving problems since there is a sense in the community of mathematicians that there are not many problems left that can be solved simply *within* a theory, that is, using only the resources of the theory itself. This is a sense that current mathematicians sometimes have, but it might not track ‘reality’. But even as a sense, it is enough to entrench the practice. And it is very successful, in the institutional sense that many PhDs and published results depend on this practice.

This practice is very useful and fruitful. It has several aspects. On the one hand, we make new results. By definition, the new result applies to the theory in which we asked the question in the first place, but it might also, very soon, be interesting for the theory in which the result was found, or it might be interesting for finding a result in another theory that can be translated or interpreted in one of the two theories. Such breadth of applicability or interest is unpredictable and depends on mathematicians noticing and taking an interest in a result. One of the other sides of this practice is that to work in, or even read, cutting edge mathematics requires experience and expertise in many areas of mathematics. Regardless, seen this way—where theories in mathematics interpret other theories in mathematics, and so confer meaning on them—we develop an impression that the discipline of mathematics is just floating, that it is not grounded, that it is *sui generis* and self-confirming and fulfilling. Is it then just a highly elaborate abstract exercise?

4.1 The Role of the Physical World

Especially people not so highly trained in mathematics might then ask me about the grounding of mathematics in ‘reality’. By ‘reality’ they usually mean physical reality, or they mean simple things like adding their expenditures (which is actually highly abstract if we think about it). The sorts of mathematical theory I am discussing are largely unapplied to ‘reality’ so understood. It is possible to do so, but it is a bit like using a recipe for Tournedos Rossini to work out how to fry a piece of bread (needed for the canapé of the Tournedos). It can be done, and the slice of bread will be fried (in clarified butter), but a lot of the recipe will stand idle. The sort of mathematical theories I am canvassing here go so far beyond our experience that it impoverishes them too much to hope for them to be grounded in ‘reality’.

So, rather than think empirically that mathematics is grounded in physical reality and our experience of such reality, and is just an abstraction from our experience,

[5]Concurrently, and independently, Bolyai was developing imaginary geometry as well. Both denied the parallel postulate of Euclidean geometry and thus proved the independence of the parallel postulate. The story of the development of non-Euclidean geometry is a very nice illustration of theories of mathematics used to model and contrast other theories of mathematics.

I prefer to think the other way around. We interpret nature and the physical reality around us *through* mathematics. As Kant argued, we need some mathematics just to have experiences in the world at all. What *shape* our experiences take, and how we compare them then, depends on the particular theory, or class of theories, we use. For any phenomenon, event, measurement, evaluation, and comparison of physical objects, we need mathematics, among other things. We also need metaphysics and the ability to make some qualitative attributions. So, we interpret nature through mathematics and other things. So, at best, and in some sense, nature 'confirms' or checks some of our mathematical theories. Not any mathematical theory can be used to interpret any piece of nature (successfully), that is, given a goal, or purpose, and a time constraint.

At best, nature confirms mathematical theories only in a very light sense. Once we have used a bit of metaphysics and qualitative attribution to individuate objects, phenomena, or events, and once we have decided on a goal, we have at our disposal a possibly infinite number of theories. Most of them would not be very useful, or most parts of them would not be used and so on. But we have a class of theories we can draw on. We select one that is familiar to us and that we have been trained to find appropriate. If we are measuring lengths of wood and stacking bricks to make a bookshelf, we could use set theory, category theory, projective geometry with a bit of measurement theory added in, but a bit of easy arithmetic will do, and we forget the other alternatives or that they even existed as soon as we meet with success in constructing our bookshelf. We confirm our basic arithmetic. Now, had we run into a problem, such as we found that all the shelves were tilted, even though we are using the same number of bricks at both ends of the shelf and doing everything one would expect, then we would have reason to reconsider the particular theory. Similarly, when we run into a problem, say, in quantum mechanics, then we start to reconsider our mathematical or logical theory. It is under such pressure, or under imaginative curiosity, that we might develop a new formal logic, or area of mathematics or have recourse to some other theory to patch up the immediate problem. We then very quickly forget what we did, unless we notice that it is interesting in its own right, or has other applications, until we face a new problem. Our experience with 'reality' only serves to foreclose on some theories; it is not enough to select a theory. And it does so imperfectly as when we use another theory as a patch.

5 The Danger in Thinking of Mathematical Theories as Models

It seems as though all we have with this description are theories or models each interpreting each other. Some are foreclosed by our bumping up against reality, but very few, and only very weakly. They are not even foreclosed as *mathematical theories*, only in their *application* to physical problems that we encounter. Thus, some philosophers might argue, there is no objectivity in mathematics, therefore truth in mathematics is only ever relative to perspective. For example, parallel lines meet in some theories of geometry and not in others. In the limit case, theories, of course, interpret themselves. Beyond the limit case, there are plenty of perspectives (theories used to interpret other theories) to choose from. Moreover, it is not that we just have a lot of choices already. Rather, we develop new theories in mathematics. So the number of interpretations already available to us increases over time. Worse still, we could even fabricate a perspective to force a 'perverse' result to look 'true' from the perspective of that, or some other, theory.

Reasoning too quickly, we might think that, therefore, from this 'mathematical theories are models' perspective: anything goes and we have a rampant relativity. Mathematics is therefore quite useless. There is no real truth or objectivity or stability, nothing definite, and no 'correct' or 'incorrect' answers. It all depends....

Digging ourselves deeper into the argument about fabrication: say Basilico insists that there have to be some undeniable truths in mathematics. He takes as an example '$2 + 2 = 4$'. Xara insists that she can choose or fabricate a perspective from which $2 + 2 \neq 4$. She chooses the example of arithmetic mod.3. In arithmetic mod.3, $2 + 2 = 1$, Basilico insists that no one uses arithmetic mod.3. Xara counters that it is perfectly useful if we are concerned only with divisibility by 3, say if we are concerned about dividing an inheritance equally between three inheritors. If we are very insistent on equality, we want to check if everything being inherited can be divided by three: the land, the money, the buildings, the paintings, the cutlery ... as for what cannot be divided by three without remainder, we negotiate the exchange. Basilico concedes that this is an appropriate application of arithmetic mod.3 but returns to the original claim that there are some undeniable truths in mathematics. These might be the logic truths. For example, a & a |- a. Xara contends that there is such a thing as a zero logic, where no inferences can be made. So, in such a 'logic' a & a | -. That is, in a zero logic, we can infer nothing at all. No symbol occupies the right-hand side of a turnstile. "This is perverse" counters Basilico. Xara is perfectly entitled to say that "Nevertheless, such a 'logic' exists. No one said that mathematics or logic cannot be perverse from time to time. Think of historical examples where new developments were regarded with deep suspicion. Already the names we give to suspect developments are telling: irrational numbers, imaginary numbers, non-standard models, forcing techniques and so on. Even the seemingly banal 'truth' $a = a$, could be denied in a formal system of mathematics. We just have to interpret the symbol '=' as '< or >'." "But now I've got you" says Basilico. You deliberately reinterpret a symbol to mean its opposite, and this is surely against common sense or against the rules in mathematics. Zero logic is not a logic, because there are no inferences. No teacher of mathematics is going to allow his, or her, students to answer that $a \neq a$ unless it is part of a *reductio ad absurdum* argument, in which special case it is not asserted as 'true' but as 'absurdly true' if we make the *per impossible* hypothesis in the first place. We can reason similarly in the logic case. There is something deliberately perverse in suggesting such a degenerate formal system be considered to be a 'logic'. "Quite so" answers Xara mysteriously. Has she conceded? No.

5.1 Truth Within a Theory and Truth of a Theory

In mathematics, truth *within* a theory is relatively well defined and quite well understood. Truth *of* a theory is not so well understood.

Let us start with the latter. Whether a theory is deemed to be, or recognised to be, true depends on the meta-theory, or class of meta-theories, in which we interpret or model the theory in question. Once the meta-theory or class of meta-theories is fixed, then it is straightforward, to determine whether the given theory is true, false, both or neither just in case there is a truth-predicate in the language of the meta-theory, and the valuation of truth for the whole theory (not just individual theorems) is well determined. The truth of the meta-theory then depends on a meta-meta-perspective from which to determine the truth, and so on as far as we care to go. Moreover, the conditions for theories to determine *truth* of another theory do not invariably obtain. Not every theory, posing as a meta-theory, has a truth-predicate or a valuation function. Moreover, in many cases, it might be possible to construct an (admittedly perverse) meta-theory where our given theory is now false, or parts of it are false, or what have you. This is a consequence of the fact that attributing truth can be represented as a purely formal exercise. So, within the limits of accepting the formal game of truth conferral as legitimate, it seems that any theory might be true or false.

The above reasoning was too quick. We do not always need a truth-predicate in the meta-theory. We just need a translation. This is enough to interpret a theory in the terms of another. Theories can be used to interpret themselves. This is done through the identity automorphism. We then 'understand' the theory in terms of itself, that is, it is taken as standing alone, as being *sui generis*. This is a type of limit case of 'interpretation' or 'modelling.' It is a bit degenerate and might not be phenomenologically informative and, therefore, in some sense, defeats the purpose of modelling or interpreting. But maybe this is too quick too, since Euclidean geometry was so presented for a very long time in our history. It was the theory of geometry, and there was no mathematically formal way to understand it from outside the theory. So, this gives us a hint about what was wrong with the quick reasoning of the former paragraph. Any further 'meaning' comes from outside and is strictly speaking 'informal', but not, for all that, uninformative. Nevertheless, such interpretation, being as it is, informal, is prone to revision. The revision takes time, and as I mentioned above, it usually takes place only under the pressure of doubt.

As for the former, depending on how we individuate theories, truth within them is supposed to be straightforward. Whatever it is that counts as a theorem of the theory, whatever is calculated by the theory, whatever is correct in the theory, and so on can be said to be true in the theory. But from the outside, from a meta-perspective, even this might be distorted, for we are not meta-logically[6] banned from perverse interpretation.

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[6]By 'meta-logically' I mean logically in the sense of informal logic. That is, were I to say that 'we are not logically banned', this might be interpreted to mean that 'according to a particular formal representation of logical reasoning, we are not banned'. Moreover, we might be able to choose any

Since truth *of* a theory and even truth *within* a theory are not completely solid concepts, the notion of truth is not helpful. This is a bit the result of our having made the notion formal or, more carefully, our formally representing the notion of truth. In particular, we have done this in logic. Once we did this, we were able to play some formal games, adding a new truth-value or several new truth-values, and with these developments, our conceptions of semantics, meaning, and truth became both more precise and also more dispersed and removed from our common intuitions. So, now we are in danger of conflating the various formal representations of truth with truth itself, if we can recover 'truth itself' at all.

5.2 Objectivity of Mathematics: Realist Objectivity

If the notion of truth cannot come to our aide, then maybe the notion of 'objectivity' will help. The traditional accounts of objectivity are either Platonic/realist or constructivist/epistemological.

On the Platonic/realist side, we ground objectivity through an ontology. And, the ontology is supposed to be independent of us and so objective, not subjective, real and not imaginary, discovered and not created, and so on.

While some mathematicians phenomenologically feel that some mathematical statements are objective, we cannot measure, point to, observe, and physically manipulate such objects, so they are not 'objects' in the sense of medium-sized dry objects that we count. However, in mathematical theories, grammatically, we treat parts of our theories as objects. They enjoy properties and relations with other objects, and they can be mathematically manipulated, translated, and even transformed (through a function). So they are not metaphysically so very different from medium-sized dry objects. They are just scientifically different (not physically causal).

The Platonic/realist conception is unstable. We cannot determine the objectivity of such objects in a way that is stable over time and, more important, across mathematical theories. This is because the identity conditions for an object in mathematics depend on the theory. Identity conditions should tell us when two objects are different from each other and when it is that what we thought were two separate objects are in fact the same object. Such determination depends entirely on the theory itself and on the meta-theory we use to interpret the truth of the identity claims. $2 + 2$ is identical to 4 in Peano arithmetic, but it is very different in a computer language where the computational procedure for dealing with '$2 + 2$' is very different from that used to deal with '4'.

formal representation, including invent our own strange one. No. What I mean here is that we do not suddenly find ourselves in the realm of trivialism if we make a perverse interpretation. It might simply be unnatural, ridiculous, strange, uncomfortable, and difficult to make sense of, but not for all that, 'impossible'.

5.3 *Objectivity of Mathematics: Constructivist Objectivity*

If the Platonic/realist conception cannot help with objectivity, let us turn to the constructivist/epistemological account. For the constructivist, the concept that is more basic than truth or objectivity is knowledge. We could say that for the constructivist, truth and objectivity are grounded in knowledge. Objects in mathematics are created through our mathematical knowledge. We say that they are ‘epistemically constrained’. We start with mathematical statements we know. These are true, by definition, since truth is grounded in knowledge. We create new mathematical objects from ones we have already created using a process of creation that *preserves* knowledge.

Ultimately, what grounds this knowledge? There are several accounts we can give: one is Kantian, one is evolutionary theory, and one is intuitive and phenomenological. There may be others, but I shall restrict myself to these at the risk of making a straw man argument.

The Kantian story is that we, as a human species, participate in, or have access to, or cannot escape from our spatio-temporal intuition. It is this which allows us to have experiences in the (physical) world. We use spatio-temporal intuition to navigate and make sense of our (otherwise) raw sense experience. Spatio-temporal intuition gives our experience shape and is a first step to making sense of the world around us. We notice regularities, we individuate physical objects, and so on. Spatio-temporal intuition can be formally represented in mathematics; in fact it is so represented in the form of the many geometrical theories and in the form or arithmetic, set theory, theories of relations and functions, and so on. Of course, after Kant, there have been a lot of developments in formal and informal mathematics, and our understanding of mathematics as whole discipline is more sophisticated, and these developments have sharpened and shaped our intuitions in their turn. So after Kant, we are more precise if we say that there is a three-way interplay between our pre-mathematically informed intuitive senses of space and time, our formal representations of those intuitive senses, and the physical world around us that can only be made sense of through the lenses of those pre-mathematically informed intuitions and their formal representations. The new, adjusted, Kantian account now runs: it is this interplay that grounds the ontology of mathematics.

We then see the instability because the intuition changes with the development of new theories. Is one correct? Maybe, but we cannot tell which one unless we turn to another sort of account.

Let us try the evolutionary account. Under this account, we develop the objects of mathematics as representing our experiences of the physical world around us in order to survive. Logically, or metaphysically, there are ways of trying to do this that would fail and would result in our quite quickly dying off as a species. We have survived, and this shows that we have an evolutionarily sufficiently correct mathematical ontology. It is objective because our survival depends on it, and we do not have to survive. So, we have developed an objectively correct mathematics in the sense of its not being evolutionarily disastrous for our species.

This is probably right as far as it goes. The problem is that it does not go very far. For one thing, it is quite insufficient to pick out one mathematical ontology, since *to survive for an average time of a species of mammal on Earth*, from the perspective of metaphysics or mathematics itself, we only need to get our mathematical ontology very approximately right. In fact, a whole class of mathematical theories is quite compatible with our survival. It is not clear, for example, how much mathematics other species have that manage to survive quite well. Maybe it is written into their DNA, but we are not in a position to recognise this yet. Had we developed projective geometry before Euclidean, we would have been fine. Had we developed an arithmetic where 8 x 68,921 = 3 on every other Tuesday after the year 2017, but was the same as our own otherwise, we would still have survived. Rather, from the perspective of pure (beyond our survival, but within the constraints of what we can imagine or work with) mathematics or pure metaphysics, the account should be reversed. It is through our survival, as a species that we have developed the mathematics that we have. If anything, it is our survival that might have hindered our mathematical imagination. There is nothing metaphysically or mathematically necessary in the selection we made of favorite theories. Individual crazy mathematicians who develop a mathematical theory that leads to the death of our species have not been followed or have not been followed enough for us to have not died out yet. Put this way, we see the *reductio* in the argument forming. The evolutionary account is not more stable than we are as a species.

The intuitive and phenomenological account is the worst, at least from a logical, metaphysical, or non-question begging mathematical point of view. The account goes that 'we' feel that or 'know' that these mathematical statements are true and these others are not. The objectivity is correctly tracked by our intuition, gut feeling, or phenomenology.

This cannot be right if we mean a collective 'we', since mathematicians have disagreed with each other in the past, and there is no guarantee that they shall not continue to do so in the future. If a particular mathematician were to claim that he, or she, has a privileged position, and can (alone) create the 'right' mathematical ontology, then there is the difficulty of choosing between competing such claims. Unless, one shares the phenomenology and agrees with the ontology created by another mathematician, determining objectivity on this basis is entirely mysterious.

If the *traditional* Platonic/realist and the constructive/epistemological accounts fail us for accounting for the objectivity of mathematics, what is left?

6 Recovering Objectivity and Truth When Thinking of Mathematical Theories as Models

The pluralist account is more complex and nuanced than the traditional accounts. The pluralist is willing to think of mathematical theories as models or other mathematical theories. He, or she, is also willing to acknowledge the phenomenology of objectivity and the sense of truth that accompanies the sense of objectivity.

The difference is that the pluralist is not metaphysically constrained to give one unified, consistent mathematical theory in his or her account of the objectivity of mathematics.

The pluralist account relies on some general observations. In the practice of mathematics, there are different ways of individuating theories of mathematics. There are similarities and differences in formal languages used in theories, but there are also translations between them. There are meta-judgments of the form: the objects of this theory are the same as/different from the objects of that (otherwise different) theory. The concepts of this theory are the same as/different from the concepts of that (otherwise different) theory. We can be precise and rigorous up to the standards of the day and up to the *finesse* of our language and concepts. Moreover, we only increase the *finesse* when we encounter and recognise a problem. Changes in mathematical theories and the acceptance of a new mathematical theory, the deployment, and so mastery of a new concept in mathematics take time.

Thus, the pluralist account is one of an interplay between language, concepts, formal representation, dissemination; one of cross-referencing, borrowing between theories, translation between theories, communicating the relevant information to others, the gradual understanding of ideas by individuals, and then by collections of mathematicians. The interplay culminates in the institutional acceptance, reinterpretation, re-enforcement, or confirmation by the development of new theories that are models of old theories or that we can show do not model the old theories. The interplay of modelling and contrasting are what lend stability, longevity, objectivity, and truth to mathematics. There is no *single account* of objectivity or truth. This affects the pluralist account of knowledge, understanding, and explanation.

7 Conclusion: The Pluralist Account of Knowledge, Understanding, and Explanation

7.1 Knowledge

For the purposes here, and as a working hypothesis, I shall distinguish knowledge from understanding. First distinguish between propositional knowledge and other sorts of knowledge. Propositional knowledge is ‘knowing that . . . ’. Propositions, in this case, are bearers of truth in the following sense. Grammatically we can attribute to propositions: truth, falsity, undetermined whether true or false, degrees of truth or falsity, undetermined degrees of truth or falsity, both truth and falsity (paradoxical propositions); approximations to truth, as good as true, acceptability, and their negative and undecided counterparts. This is what we are counting as a proposition here: parts of speech or writing to which it is grammatically reasonable to make such attributions. Truth, falsity, and so on are stable and normatively remain unquestioned up to a recognised error or reasonable doubt. Note that the standards for error and reasonable doubt in mathematics are the highest of any discipline. We are not talking about ‘reasonable doubt’ in the sense of a court case where a lot of common sense helps with the determination of ‘reasonable’. The ‘common sense’ of mathematics is highly refined and fine grained. The standards of precision in the language are highest possible. Mathematics is the field where these standards exceed those of other areas of inquiry, since in other areas they are *more easily* compromised by political, moral, economic, metaphysical, and other factors. It might be impossible to measure whether the standards of precision are more often compromised in one field or another. The *more easily* claim is, therefore, vague, ambiguous, and meta-metaphysically qualitative.

We cannot *know* any particular proposition *tout court*. We can only know a proposition *up to*, or *relative to*, a context, a purpose, a linguistic, or a grammatical constraint. Knowledge of ’one’ fact or object can be increased. It can be increased indefinitely. This is because, for the pluralist, mathematical propositions do not stand alone. They are always accompanied by a context, and our knowledge and understanding of a context change with more information, understanding, and so on.

7.2 Understanding

Let us now distinguish knowledge from understanding. Knowledge, as it is treated above, is propositional. In contrast, understanding is more holistic, phenomenological, and intuitive. In an extreme case, we might even be able to understand, in some sense, without being able to articulate propositions that underlie our understanding. Byers calls this a proto-concept. In such a limit case, our understanding would simply be an intuition or a feeling about something. It is not yet veridical, since not yet articulated in the form of a proposition. The feeling or intuition might be quite weak or quite strong. A mathematician might feel that there is some relation between two ideas and will then seek out an articulation of a hypothesis about what that relation is. A deepening of understanding is had by this seeking. We use language, theories, and articulated ideas to mirror, hone, contrast, or compare our feeling or intuition to articulated propositions. We make whole theories to model our thought. We thereby deepen our understanding further. We look to the limitations or edges of the thought, asking questions about where it breaks down, where it is compromised, and how. This adds further depth to our understanding, and this process can be carried out indefinitely.

Understanding and knowledge increase in tandem in a back-and-forth play. We have a sense that there is something to look into (understanding a proto-concept), we try to articulate it in terms of propositions, and we can then attribute a truth-value or lack of truth-value to the proposition. To do this, we look to the context, and we model the proposition, thereby setting it in a context that will confirm, falsify, and disconfirm, with all of the further nuances and weaker types of ‘truth-value’ attribute. We seek to have as full a confirmation or disconfirmation as possible *up to* our purposes and *up to* the capacities of the language in which we couch the proposition. Modelling, or theory building, is part of this exercise in increasing understanding. The increasing of understanding does not end there because we can then compare theories to each other, and we do this from a meta-perspective which, in turn, receives more precise articulation. Under this pluralist picture, understanding can increase indefinitely. This thought also has repercussions for our conception of 'explanation'.

7.3 Explanation

Explanations are used to deepen understanding. Since understanding can increase indefinitely, so can explanations.

> If some reader wants or needs more details, as for instance concerning modular arithmetic [in a proof to show that $1 + 1 = 0 \pmod 2$)] it can be provided by giving further explanations, as is done in teaching unprepared students. In principle, though, one could go through the whole development of Peano Arithmetic, develop modular arithmetic and what not. How far one has to go back in one's justification of an inference is a pragmatic question; there is no theoretical upper bound on the number of interpolations necessary for an absolute justification (whatever that would mean). (Rav 2007, 313-4)

Explanations can also be subject to extrapolation, so sometimes we are called upon to elaborate on the context or the meta-theory in which we are working. And this, too, of course, can be questioned. From a pluralist perspective, the difference between interpolation and extrapolation is sometimes interchanged.

Moreover, in some sorts of explanation, negative or counterfactual information is helpful: information why we do *not* employ such and such a technique, or at what point a concept no longer applies, or when it is that a translation is disingenuous or perverse. The edges of ideas and their limit thresholds contribute to understanding. These, too, grow. When we end an explanation, this is either a sign of satisfaction, or it is a sign that we have nothing more to add, so a sign of limitation. We rest with the end of an explanation because we are satisfied. But that satisfaction is only temporary. As we learn more, as we look to more intriguing puzzles, as we discover alternative ways of seeing the same phenomenon or question, we might call for a renewed deepening of explanation. The limitation of before is pushed further back. Thus, explanations in mathematics also grow, especially under the pluralist conception of mathematical theories as models that interpret each other.

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Mathematics for Makers and Mathematics for Users

Alexandre V. Borovik

To Reuben Hersh who posed an incisive problem:
What is Mathematics, Really?

Introduction

As I argue in my paper Borovik (2016), the current crisis in school-level mathematics education is a sign that it reaches a bifurcation point and is likely to split into two streams:

- education for a selected minority of children / young people who, in their adult lives, will be filling an increasingly small share of jobs which really require mathematical competence (I call them *mathematical makers*); and
- basic numeracy and mathematics awareness classes for the rest of population, *end users* of technology saturated by mathematics – which, however, will remain invisible to them.

In this paper, I discuss challenges arising in mathematics education for *makers* of mathematics. This is a theme which is rarely discussed in the mathematics education literature. It demands re-thinking of basic assumptions underpinning the mainstream mathematics education.

I invite the reader to discard taboos and start a frank and open discussion of the difficult problem:

> What is Mathematics Education, Really?

In the changing socioeconomic environment of mathematics, it needs to be addressed from the first principles.

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A Thought Experiment: Replicators

I suggest a simple thought experiment. Science fiction books occasionally mention an imaginary device: a *universal replicator*. It consists of two boxes; you put an object in a box, close the lid, and instantly get its undistinguishable fully functional copy in the second box. In particular, a replicator can replicate smaller replicators.

Now imagine an economy based on replicators. It needs two groups of producers: a small group of engineers who build and maintain the biggest replicator and a diverse, but still small, group of artisans, designers, and scientists who produce a single original prototype of each object. Let us enhance the functionality of replicators and assume that they can store originals in memory and share them with other replicators.

Then the world needs only one baker who has produced, once, a flavorsome and precisely textured original of a loaf, baguette, etc. of every kind of bread.

This hypothetical economy also needs a service sector, mostly waste disposal.

Next, try, if you can, to imagine a sustainable, stable, equal, and democratic model of education that supports this lopsided economy.

A wild and irrelevant fantasy? Alas, it is not. This apocalyptic future is already upon us—in the information sector of economy, where computers act as replicators of information. Mathematics, due to its special role in information technology, is the most affected part of human culture. The new patterns of division of labor split *mathematics for makers* of mathematics from *mathematics for end users* of mathematical technology and trigger a crisis of mathematics education. The latter increasingly focuses on mathematics for *users* and undermines itself because a sustainable reproduction of mathematics requires teachers educated as *makers*.

The Ultimate Replicating Machines

I borrowed the title of this section from a chapter in my book Borovik (2010). In the book, I followed Davis and Hersh (1981) who defined mathematics as

> *a study of mental constructs with reproducible properties*,

and I argued that the essence of mathematics was in its precise replicability which imitated the rigid stability of the laws of the physical universe. In the domain of technology,

> *mathematics is the ultimate in the technology transfer.* (Stewart 1990)

A mathematical theorem needs to be proved only once—and then it can be used for centuries. An algorithm needs to be developed only once—and then it can serve, as the Google Ranking Algorithm does, as a kingpin of a global information system.

In previous historic epochs, every use of a mathematical result required the participation of a human, who, depending on the level of his or her tasks, had to be trained in arithmetic (e.g., a bank clerk) or, in addition, had to learn elementary algebra, logarithms, and trigonometry (e.g., an artillery officer.[1]) The development and teaching of mathematics was shaped by a natural requirement: mathematics had to be understood and used by humans. In particular, this technological imperative defined the role of proofs at higher stages of mathematics education: the ability to derive formulae was used as a criterion of understanding.

Nowadays, mathematics works mostly inside of computers, and its applications are developed for being fed directly into computers, with humans excluded from this process and reduced to the role of uninformed *end users*.

Instant replicability of mathematics and the economy of scale, combined together, create a peculiar singularity:

> in mass produced devices such as smartphones, the per unit cost of mathematics encoded and hardwired within the device converges to zero.

For the end user, the mathematical component of technology frequently remains invisible. Even if the user is aware of the presence of a particular mathematical tool in the product used, it is not accessible for maintenance or repairs (try to reprogram a microchip in your credit card!). But this does not actually matter: even if the mathematical core is reached, it remains incomprehensible for anyone with the exception of a very narrow circle of experts.

Indeed, mathematical results and concepts involved in practical applications are much deeper and more abstract and difficult than ever before. And we have to accept that they are beyond the reach of the vast majority of graduates from mathematics departments in our universities.

The cutting edge of mathematical research moves further and further away from the stagnating mathematics education. From the point of view of an aspiring PhD student, mathematics looks like New York in the Čapek Brothers' book *A Long Cat Tale*:

> *And New York – well, houses there are so tall that they can't even finish building them. Before the bricklayers and tilers climb up them on their ladders, it is noon, so they eat their lunches and start climbing down again to be in their beds by bedtime. And so it goes on day after day.* (Čapek and Čapek 1927, reprinted 1996, p. 44).

If you think that these words are too jocular for a discussion of a matter so serious as the fate of mathematics as a cultural system, please take into consideration that Joseph and Karel Čapek were the people who coined the word *robot* for a specific

[1] It is worth to remember that in the first half of the 20th century, school mathematics curricula in many nations were dictated by the Armed Forces' General Staffs—this is why trigonometry was the focal point and apex of school mathematics: in the era of mass conscription armies, it was all about preparation for training, in case of war, of a sufficient number of artillery and Navy officers and aircraft pilots. With this legacy, we still cannot make transition to a more human mathematics.

socioeconomic phenomenon: a device or machine whose purpose is to replace a human worker.[2] Almost a century ago, they were seriously thinking about the social impact of technological and scientific progress.

Investment cycles and research and development cycles in many modern industries are just two years long. On the other hand, proper mathematics education still takes at least 15 years from the age of 5 to the age of 20—or even 20 years if postgraduate studies are needed.

As I argue in Borovik (2016), mathematics education is being torn apart by this tension between the deepening specialization of labor and increasing length of specialized education and training required for jobs at the increasingly thin cutting edge of technology. Public discourse on education policy is in a mess.[3] Key issues are ignored; this is not surprising—when the society does not know the answer, the question does not exist. Nothing said by politicians, by industry, by experts in mathematics education, etc. can be taken at face value.

For example, if banks and insurance companies were interested in having numerate customers—as they occasionally claim—we would witness the golden age of school mathematics: fully funded, enjoying cross-party political support, promoted and popularized by the best advertising companies in all forms of mass and social media. But they are not; banks and insurance companies need a numerate workforce – but even more so they need innumerate customers. 25 years ago in the West, the benchmark of arithmetic competence at a consumer level was the ability to balance a checkbook. Nowadays, bank customers can instantly get full information about the state of their accounts from an app on a mobile phone together with timely advice tailored to individual circumstances on the range of recommended financial products. This kind of service can be characterized in alternative terms: a bank can instantly exploit the customer's vulnerability.

Mathematics for Makers and Mathematics for Users

Makers and Users

The new patterns of division of labor split mathematics for makers from mathematics for end users of mathematical technology. How to describe the two? The replicability of mathematics mirrors the stability of laws of the physical universe, which is captured by an apocryphal formula:

[2]The root of the word "robot" is Slavic and means "work."

[3]Perhaps I would never write this paper and its predecessor (Borovik 2016) if I had not had a chance to observe, at a close range, the recent National Curriculum reform in England.

> *Mathematics is the language of contracts with Nature which Nature accepts as binding.*

Therefore, in my understanding, mathematics for makers is mathematics for those whose duties include writing contracts with nature and inventing, in the process, new mathematics and new ways to apply mathematics—they could be mathematicians, engineers, and scientists. In terms of the "universal replicator" simile from section "A Thought Experiment: Replicators," it is mathematics for those who produce the originals for subsequent replication.

In particular, it is mathematics that cannot be entrusted to computers.

We need to remember that

> mathematicians and physicists are stem cells of a technologically advanced society.

They are re-educable, able to change their role and metamorphose[4]—and, as I argue in Borovik (2016), this is made possible by frequent changes of the mode of their mathematical thinking in the process of their learning of mathematics.

On the other hand, mainstream mathematics education gradually sheds its content and loses its meaning: in its present form, it is not actually needed in the world of end users (Simeonov 2016).

Essence and Phenomenon

At a bit more philosophical level, the issue boils down to the difference between *essence* and *phenomenon*.

I was lucky that my philosophy lecturer at my university was one of the prominent Russian philosophers of that time, Mikhail Rozov. In the oppressive ideological environment of Soviet Russia, he was a quiet nonconformist. In one of the areas of his professional work, he dared to develop Niels Bohr's *complementarity principle* as a general principle of epistemology and then applied it to the humanities—this

[4]While Brexit is still in the news, it is worth to mention that Dominic Cummings, the Director of the *Vote Leave* campaign, explains in his blog http://bit.ly/2ePmyA2 that he hired, instead of professional pollsters and public relations people, some physicists for analysis of voters' intentions. He writes: *Physicists and mathematicians regularly invade other fields* but other fields do not invade theirs *so we can see which fields are hardest for very talented people. It is no surprise that they can successfully invade politics and devise things that rout those who wrongly think they know what they are doing.*

required some courage. In his lectures, he found a clever way to circumvent the official dogma by announcing to me and my peers that all of us, by default, were instinctive dialectic materialists and that he did not hope to advance us any further because of our general ignorance. This allowed him to teach us an honest history of philosophy instead of the official course of dialectic materialism. However, he took care to demystify some sacred dogmas of the official philosophy, in particular the essence/phenomenon double act of the Hegelian dialectics. He taught us:

You can describe a table knife in two ways.

(1) *It is a long narrow flat piece of steel slightly sharpened at one edge, with a handle attached.*
(2) A thing for spreading butter on bread.

(1) *describes how a knife is made; it is its* essence. (2) d*escribes how a knife is used; it is its* phenomenon.

For me, mathematics is all about how mathematics is made; I am a maker, not a user. Steven Strogatz published in *The New Yorker* blog[5] a brilliant popular article about how the number π is *used*; for me, it is more important to understand how the number π is *made* (or discovered). I work with the *essence*.

Pattern Matching

You have perhaps heard an expression popular in the mathematics education community:

"*mathematics is the science of patterns*."

This is mathematics for users. Mathematics for makers can be described as

"the science of structures behind patterns."

Meanwhile, the "mathematics is the science of patterns" approach (I suggest to call it *patternism*) is becoming popular within some parts of the applied/industrial mathematics community. I heard talks with suggestions to abandon the formulation of mathematical models of real-world objects and processes, as well as their subsequent analysis by mathematical means. Instead of that, it was suggested to run pattern-matching algorithms over large data sets.

This technology has every right to exist; it could be quite useful, especially when you are interested in "bulk" solutions which work correctly with a sufficiently high probability, securing, on average, acceptable profit margins. But this is not mathematics as we know it.

[5]S. Strogatz, *Why Pi Matters*, 15 March 2015. http://www.newyorker.com/tech/elements/pi-day-why-pi-matters?intcid=mod-most-popular.

There is an intrinsic danger in the patternalist technology: there is a possibility that very soon it will be monopolized by a few algorithms/systems, the same way as social media are dominated by likes of FACEBOOK and TWITTER. Verification of results could become a problem. There is a danger that the flaws of social media which resulted in the present "fake news" scandal might be reproduced, on a grander scale, in the "big data" technology—with computers spreading "fake data" among themselves.

Besides writing contracts with nature, the professional competences of the new generation of makers should include the ability to *control* computers. Mathematicians must remain prepared to face intellectual challenges so important and critical that they cannot be entrusted to computers.

As a corollary, serious mathematics education has to remerge with computer science.

Interestingly, some trends in mainstream education appear to lead in the exactly opposite direction.

Our new information environment becomes more and more saturated by pattern recognition and pattern matching (predictive typing is a prominent example).[6] There are some first signs that it starts poisoning teaching and learning mathematics. As it is argued in a deep and revealing paper by Yagmur Denizhan, this has already triggered changes in students' approach to learning: they started to imitate generic statistically shaped parse/substitute/append computer algorithms akin to that of pattern-matching Google Translate or predictive typing.

> *What led me to the line of thought underlying this article was a strange situation I encountered sometime in 2007 or 2008. It was a new attitude in my sophomore class that I never observed before during my (by then) 18 years' career. During the lectures whenever I asked some conceptual question in order to check the state of comprehension of the class, many students were returning rather incomprehensible bulks of concepts, not even in the form of a proper sentence; a behaviour one could expect from an inattentive school child who is all of a sudden asked to summarise what the teacher was talking about, but with the important difference that – as I could clearly see – my students were listening to me and I was not even forcing them to answer. After observing several examples of such responses I deciphered the underlying algorithm. Instead of trying to understand the meaning of my question, searching for a proper answer within their newly acquired body of knowledge and then expressing the outcome in a grammatically correct sentence, they were identifying some concepts in my question as keywords, scanning my sentences within the last few minutes for other concepts with high statistical correlation with these keywords, and then throwing the outcome back at me in a rather unordered form: a rather poorly packaged piece of Artificial Intelligence.*
> *It was a strange experience to witness my students as the embodied proof of the hypothesis of cognitive reductionism that "thinking is a form of computation". Stranger, though, was the question why all of a sudden half a century after the prime years of cybernetic reductionism we were seemingly having its central thesis actualised.* (Denizhan 2014)

Alas, I am in agreement with Yagmur Denizhan—I observe this behavior in my own students.

[6] I attempted to write some notes that became a fragment of this paper, on a tablet with predictive typing. A remarkable experience—predictive typing does not help to formulate any new thoughts but speeds up composing routine emails.

I wish to mention, in passing, another cultural phenomenon that I call *cartoon physics menace*[7]: a systematic suppression of laws of mechanics (and physics) in the virtual worlds of CGI movies, cartoons, and computer games, where our (grand)children spend an ever-increasing part of their lives and where everything can happen—pigs may fly. I am afraid it kills the all important "physical" intuition of the real world—the same way as an out-of-tune music toy can damage a child's sense of tone pitch.

What Will Replace the Present System of Mathematics Education?

As I argue in Borovik (2016), the present model of "mathematics education for all" is unsustainable, and, not surprisingly, the first cracks have started to appear. I concluded my paper with a warning that I wish to repeat here.

> Democratic nations, if they are sufficiently wealthy, have three options:
>
> A. Avoid limiting children's future choices of profession, teach rich mathematics to every child—and invest serious money into thorough professional education and development of teachers.
> B. Teach proper mathematics, and from an early age, but only to a selected minority of children. This is a much cheaper option, and it still meets the requirements of industry, defence and security sectors, etc.
> C. Do not teach proper mathematics at all and depend on other countries for the supply of technology and military protection.
>
> Which of these options are realistic in a particular country at a given time, and what the choice should be, is for others to decide.

My own instincts make me to go for option A, but it could happen to be unrealistically expensive—and unlikely to have support of every parent and every teacher.

Meanwhile, there are signs of option B emerging as the preferred one—at least in some countries.

In England, the recent green paper *Building Our Industrial Strategy*[8] sets the aim of

> "*expanding the number of specialist maths schools across the country*" (p. 16),

[7]You can watch on YouTube a useful compilation of relevant episodes from *The Looney Tunes* (the classics of the genre): Zac Snively, *Wile E. Coyote vs. The Road Runner Physics*, https://www.youtube.com/watch?v=EdGxf5sYdsU, 26 March 2015.

[8]Her Majesty's Government, *Building Our Industrial Strategy*. Green Paper, January 2017. http://bit.ly/2kh3roa.

and, which is much more telling, signals a shift of the preferred, from the government's point of view, career destination of

> *today's PhD students [who] are often tomorrow's research leaders, entrepreneurs and industrial researchers* (p. 29)

from academia to the industry.

Option B means separation of mathematics education for makers from education for end users.

But what is mathematics education for makers?

This question has never been seriously discussed. To answer it, Reuben Hersh's famous question (Hersh 1999):

> *What Is Mathematics, Really?*

needs to be recast as

> What Is Mathematics Education, Really?

This question is especially important in the context of education for makers. This is what I focus on in the rest of the paper.

I am not discussing the mathematics education of users—it is where the present model of mathematics education is moving to, in chaotic jerky moves, like a caterpillar pulled by ants in the general direction of their anthill. Some rather extreme suggestions have been made— for example, Emil Simeonov made a case for

> *drastically reducing mathematics teaching in schools to the level of music teaching, and introducing specialized schools* (i) *to prepare future engineers and scientists,* (ii) *to prepare for all other professions who need mathematics and* (iii) *where all those children who are just interested in mathematics can go deeper into the subject.* (Simeonov 2016)

Educating Makers

As I write in my paper Borovik (2017a),[9] advanced specialist mathematics schools such as Kolmogorov School in Moscow, Fazekas,[10] or Lycée Louis-le-Grand[11] accumulated a considerable experience of advanced mathematics education at the secondary school level. It remains mostly undocumented, unpublished, and not

[9] Some of the material in this text is built on observations made in that paper.

[10] A good description of Fazekas can be found in Juhász (2012).

[11] Lemme (2012) contains a fascinating analysis of Lycée Louis-le-Grand. M. Lemme said in a private communication: *It should be borne in mind that the system of* classes préparatoires *never was meant to train mathematicians On the other hand, and however immodest it will sound, the Institute I went to,* Lycée Louis-le-Grand, *always made a specialty of training the best and in particular the few who would become professional mathematicians.*

properly analyzed. However, a blog of the London Mathematical Society contains a collection of papers Borovik (2012) on advanced-level specialist mathematics schools in various countries. Even a brief look shows that these schools are all different. Also it is immediately clear that they all provide *mathematics for the makers*.

They nurture in their students specific mental traits which are almost never discussed in the literature on mathematics education or mentioned in education policy discourse:

- the ability to engage the subconscious when doing mathematics;
- the ability to communicate intuition;
- the ability to learn by absorption;
- the ability to compress mathematical knowledge;
- capacity for abstract thinking;
- being in control of their mathematics.

I'll try to explain these objectives point by point. I will also argue that development of these mental traits is the essence of mathematics education for makers.

I have to make an important disclaimer: I am not proposing to impose the model of mathematics education as practiced in the best (one might wish to say: elite) specialist mathematics schools on the rest of the world: I only wish to discuss lessons that can be learnt from their experiences.

Engaging the Subconscious

This is an aspect of mathematical practice that is mostly unknown outside the professional community of mathematicians.

In humans, the speed of totally controlled mental operations is at most 16 bits per second. In activities related to mathematics, this miserable bit rate is further reduced to 12 bits per second in addition of decimal numbers and to 3 bits in counting individual objects. Standard school mathematics education trains children to work at that speed, controlling and verbalizing each step: "left foot, right foot . . . ." Perhaps they can learn to walk slowly—but not many of them will ever be able to run.[12]

By comparison, the visual processing module in the brain crunches 10,000,000 bits per second (Nørretranders 1998, pp. 138 and 143).

I offer a simple thought experiment to the readers who have some knowledge of school level geometry.

[12]Here I borrow some details from my book Borovik (2010).

> Imagine that you are given a triangle; mentally rotate it about the longest side. What is the resulting solid of revolution? Describe it. [Answer is in footnote.[13]] And then try to reflect: where has the answer come from?

The answer comes from your subconscious. This is the best kept secret of mathematics: it is done by the subconscious. Moreover,

> Mathematics, in one of its many facets, is a language for communication with the subconscious.

If you were able to answer the question about the rotating triangle, then you were able to pass your commands to the visual processing centers of your brain, which then managed to unambiguously interpret them and return you the result in a form ready for verbalization and communicating back to me.

It is like training a dog.

Dogs have many faculties which we, humans, are lacking, for example, a fantastic sense of smell. To exploit these faculties, we have to send our commands to the dog and interpret its reactions. A learner of mathematics is a dog trainer; his subconscious is his/her "inner dog" (or a puppy), a wordless creature with fantastic abilities, for example, for image processing or for parsing of symbolic input. The subconscious has to be trained to react to commands *triangle!*, *side!*, and *rotate!* in a way similar to a dog reacting to *sit!*, *bite!*, and *fetch!*.

We need to look at that in more detail—so a further digression into the subconscious is needed.

Digression into the Subconscious

I share Paul Bloom's conjecture that the human brain contains the equivalent of two quite separate supporting structures for two different causality systems: one for the physical world another for the social world. As he metaphorically put it in Bloom (2004),

> We have two distinct ways of seeing the world: as containing bodies and containing souls (p. xii).

For some years, Bloom was trying to test his conjecture by psychological studies of infants:

> *We suggest that infants possess different systems – or modes of construal – for reasoning and learning about inanimate material objects versus reasoning about people (and possibly, all*

[13]Most people who I asked this question usually answered, after a few seconds of looking inside themselves, something like "*two circular cones glued at the shared base*".

> *intentional entities). This is supported by the present data, as well as by a body of research suggesting that infants interpret inanimate objects in terms of physics and not goals (e.g. Woodward, 1998; see also Kuhlmeier, Wynn, & Bloom, in preparation), and interpret people and other animate entities – but not inanimate objects – in terms of goals (e.g. Meltzoff, 1995; Shimizu & Johnson, in press; Woodward, 1998). We suggest that these systems are not the product of past learning. Instead, they provide the foundation for future learning.* (Kuhlmeier et al 2004b; see also Kuhlmeier et al 2004a.)

If true, Bloom's conjecture could have some interesting consequences for the philosophy of mathematics because it allows us to modify the Davis-Hersh definition of mathematics mentioned in section "The ultimate replicating machines":

> Mathematics is a reproducible and verifiable modelling of the causality systems of the physical world in terms of causality relations of the social world.

Bloom's conjecture also allows us to describe mathematics as a language for communication between the two causality systems, thus creating a conceptual framework for my "inner dog" metaphor. Actually, it would be best to talk about "inner wolves"—all behavioral traits of dogs are present in wolves and only amplified or suppressed in dogs by breeding (Fogle 1990).

The "inner wolf" is the physical causality module of the mind; it lurks below the horizon of consciousness, and the key issue in learning mathematics is learning a language for communication with it.

Wolves (the real ones, not metaphorical) are remarkable for the apparent disconnection between their social and physical causality systems. They have a sophisticated signal system for social interaction. They also patiently observe and then can predict the behavior of their prey. But they do not communicate with each other about the prey!

Wolves can show social aggression toward other wolves, but it is very different from their "true predatory aggression"[14]: wolves do not feel any emotions toward their prey; emotions are reserved for other wolves.

In baboons, the disconnect between the perception of social and physical worlds is even more striking, and the book *Baboon Metaphysics* by Cheney and Seyfarth (2007) is quite revealing because, in evolutionarily terms, baboons are much closer to humans than wolves.

A society of baboons' male troop has a linear transitive hierarchy recalculated every day after each fight between adjacent members. Human boys in less humane places such as various kinds of borstals, reformatories, and juvenile prisons form a similar strict linear order hierarchy recalculated every day as a result of fights.

[14]The true predatory aggression is suppressed by breeding in most breeds of dogs.

But the social order of female baboons—with grandmothers[15] and even grand-grandmothers caring about their descendants—is very different. In the words of *The Baboon Metaphysics*, female baboons live in the *Jane Austen's World*. Female baboons also form a transitive linear hierarchy, stable – they do not fight for a higher place in the order – but which is recalculated by transitivity every time a daughter is born and is inserted into the linear order immediately after her mother and her older sisters. This is the reason why a book about baboons (Cheney and Seyfarth 2007) contains a definition of *transitive relation*—and explanations of its meaning are repeated in the text several times.

In short, baboons are users of transitivity and linear order—but they apply it only to the social world.[16] Their relations with the physical world are much more primitive than their social life.

It appears that the barrier for information exchange between the two causality systems has been broken only in humans—and only partially:

> fMRI reveals reciprocal inhibition between social and physical cognitive domains. (Jack et al 2013)

So I wish to reaffirm my conjecture:

> "the inner dog", as I described it in Section 6.1, is the physical causality module of our mind.

Neurophysiology is still in infancy, but there is at least one example of a state-of-the-art experimental study:

> *Our work addresses the long-standing issue of the relationship between mathematics and language. By scanning professional mathematicians, we show that high-level mathematical reasoning rests on a set of brain areas that do not overlap with the classical left-hemisphere regions involved in language processing or verbal semantics. [...] Our results suggest that* ***high-level mathematical thinking makes minimal use of language areas and instead recruits circuits initially involved in space and number****.* (Emphasis is mine – AVB.) *This result may explain why knowledge of number and space, during early childhood, predicts mathematical achievement.* (Amalric and Dehaene 2016)

The physical causality module has immense raw processing power, but it is mute. The social causality module has access to language but otherwise is very slow. It has to train the physical module, much as people train dogs (i.e., domesticated wolves).

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[15]There are no baboon granddads—males' lives are short and brutal, and they die young, mostly killed by other male baboons. From the day of reaching his position at the top, alpha male rarely stays alive for more than a year. Lower down the hierarchy, fights become less physically harmful and more ritualistic.

[16]I write more about baboons and their mathematics in Borovik (2017c).

I think it is obvious to every working research mathematician that, in their professional community, mathematicians are ranked by the size and strength of their inner dogs. When two mathematicians meet, their inner dogs start to sniff each other.

I dare to suggest that children who grew up to become mathematicians are to some degree aware of the existence of their dog (or puppy) and perhaps even love it and care about it.

I collected hundreds of mathematicians' testimonies about difficulties they experienced in their earliest encounters with mathematics (you may read some of them in Borovik (2017b)). A generic one was being misunderstood by adults. The most frequent specific difficulty was telling the left from the right—for lack of logical justification for the distinction between the two. A child can be told by adults "this is left and this is right," but his inner dog may tell him, using its posture and a skeptical position of its ears as means of communication "sorry, master, but they smell the same to me." For a child, to retain his/her mathematical ability means to retain the ability to listen to his subconscious and not to hurry to accept, as absolute truth, what he is told by adults.

How can a learner of mathematics start engaging his/her subconscious? Perhaps even without noticing it—in sharing his/her intuition with other likely minded young mathematicians. I say more on that in the next section "Sharing Intuition."

Sharing Intuition

There are four conversants in a conversation between two mathematicians: two people and their two "inner dogs."

When mathematicians talk about mathematics face-to-face, they frequently use language:

- which is very fluid and informal;
- is improvised on the spot;
- includes pauses (for a lay observer, very strange and awkwardly timed) for absorption of thought;
- has almost nothing in common with standardized mathematics "in print."

Mathematicians are trying to convey a message from their "inner dogs" directly to their colleagues' "inner dogs."

Alumni of high-level specialist mathematics schools are "birds of a feather" because they have been initiated into this mode of communication at the most susceptible age, as teenagers, at the peak of intensity of their socialization/shaping group identity stream of self-actualization. Learning to speak to a peer's "inner dog" is an efficient way to learn a language for communication with your own "inner dog."

This process is remarkably similar to the way toddlers learn to think by first directing at themselves the speech of their parents directed at them—and then interiorizing it.

Learners of mathematics need to talk to each other to develop this crucial interiorization of their outward-directed speech—and to talk informally—in a language they invent themselves.

In this context, the role of mathematics teachers goes beyond giving to students examples of "proper" mathematical language; teachers have to provide their students with a rich diet of challenging problems which go beyond the application of procedural recipes, stimulate mathematical thinking, and thus require the use of a deeper intuition and sharing of intuition.

In that respect, mathematics is not much different from arts. Part of the skills that children get in higher-level music schools, acting schools, ballet schools, and art schools is the ability to talk about music, acting, ballet, and art with intuitive, subconscious parts of their minds—and with their peers—in a semi-secret language which is not recognized (and perhaps not even registered) by the uninitiated.

Learning by Absorption

For talking to each other, the best option is to meet face-to-face, and specialist mathematics schools provide the best environment for that:

> "*students find their tribe and learn from each other*."[17]

This is an aspect of mathematics/physics education of "mathematically able" children which is almost never mentioned: "mathematically inclined" (my preferred term) children have a high capacity to learn by absorption. This trait remains dormant in the mainstream school environment but will be activated when kids find themselves surrounded by children *like them*. My university has a large and vibrant community of mathematics PhD students, and it is a place where learning by absorption can be observed "in the wild." It is less known that the same could happen with a certain kind of 13–16-year-old kids when they form a small learning community.

Indeed, who will teach them in their professional future? They will have to teach themselves and learn from each other. The key to the success of mathematics education for makers is the creation of a self-learning environment where students learn by absorption.

[17] A. Wolf, quoted in L. McClure, *All students should receive excellent math teaching not just those in specialist maths schools*, http://bit.ly/2k33Kj3, posted 3 February 2017, accessed 18 February 2017.

Compression and Abstraction

The specific modus of communication based on sharing intuition triggers the development of another mental skill specific for mathematics: compression of information. In the words of William Thurston, one of the greatest mathematicians of recent times,

> *Mathematics is amazingly compressible: you may struggle a long time, step by step, to work through some process or idea from several approaches. But once you really understand it and have the mental perspective to see it as a whole, there is often a tremendous mental compression. You can file it away, recall it quickly and completely when you need it, and use it as just one step in some other mental process. The insight that goes with this compression is one of the real joys of mathematics.* (Thurston 1990)

In its turn, compression requires abstraction; I wrote in Borovik (2013) about the strange fate of abstract thinking and the paradoxical situation when computer science requires much higher levels of abstract thinking than is developed in recipients of the mainstream mathematics education in school and university.

We are talking about the next generation of mathematicians who, most likely, will routinely use automated proof checkers and engineers who will be using modelling and analytic software of a similar degree of sophistication. To be efficient and safe in their work, they will need a firm grounding in computer science and a sharpened ability for abstract thinking.

On the other hand, children in their early teens are quite open to absorption of abstract concepts; after all, they are grappling with other important abstract concepts in their lives, for example, “love.”

Being in Control

On this point, I refer the reader to my recent paper Borovik (2017b) where I discuss emotions related to a person’s control (or lack of control) of his/her mathematics:

> sense of danger; sense of security; confidence, feeling of strength; feeling of power;

which eventually lead to the ultimate emotion of mathematics:

> realisation that you know and understand something that no-one else in the world knows or understands – and that you can prove that.

These higher-level emotions are not frequently discussed in the context of mathematics education—but, remarkably, they are known not only to professional research mathematicians but also experienced by many children in their first encounters with mathematics.

I think it is self-evident that mathematics education for makers should nurture independence of their thinking and put them in control of their mathematics.

Conclusions

The social role of mathematics is changing. To save mathematics as a cultural system, we need to take special care of education of the next generation of mathematically competent *makers*, perhaps at the background of collapsing mass mathematics education.

I tried to argue that

mathematical intuition, ability to share intuition, compression, abstraction, and being in control

should be seen as the cornerstones of mathematics education for makers.

These key skills can be nurtured by uniting mathematically inclined students with their tribe, encouraging the communication of mathematics, and providing children with rich mathematics, gentle academic guidance, and a strong value system.

The aim of my paper is to start a discussion.

I understand that I pose more questions than give answers.

For example, I have not said a single word about what should be taught (perhaps I can only suggest that mathematics needs to be re-united with physics and computer science). I completely ignored all organizational, administrative, and political issues.

Instead, I tried to focus on methodological and pedagogical challenges highly relevant for a selective "deep" mathematics education which are ignored in the current model of mass education.

I have to warn that this is a political and ideological minefield. Academically selective education is a hot potato, at least in Britain.

The social/physical duality of causality modules of human mind is an even more difficult theme. Indeed, another obvious area of human activity affected by interaction between the two causality system is religion, myth, and magic—I mention them briefly in Borovik (2017b). The literature on this topic is already saturated by references to "body-soul duality." Neurophysiology is still in its infancy, and identification of the two causality systems appears to be too subtle a problem for direct experimental study at the current level of research technology.[18]

Despite its first successes, experimental neuroscience does not yet provide us any certainty or protection from distracting ideological debates.

Meanwhile, let us abandon taboos and start a frank and open discussion of this difficult problem.

[18]See Yeo et al (2017) for a meta-analysis of some of findings – they are very interesting, but they are not enough.

Disclaimer

The author writes in his personal capacity; his views do not necessarily represent the position of his employer or any other person, corporation, organization, or institution.

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A Case Study in Reuben Hersh's Philosophy: Bézout's Theorem

Elena Anne Corie Marchisotto

I met Reuben Hersh, in person, in 1989. However, I knew of him well before that. I had read *The Mathematical Experience* ([11] 1981), a book he had coauthored with Philip Davis that won the National Book Award in 1983. Written for a general audience, this book sought to promote an understanding of mathematics from historical, philosophical, and psychological perspectives.

I obtained a library copy of *The Mathematical Experience* and immediately recognized that it had potential for use in the classroom. At the annual AMS/MAA meeting in Phoenix, I attempted to purchase the book. I was told that this was the last copy at the publisher's table, and so I would need to wait to have one sent to me. At that very moment of rejection, a booming voice proclaimed: "Give her the book!" It was Reuben. I could barely find my voice, when this big bear of a man then invited me to discuss the book over coffee. We have been friends and collaborators ever since, and my life has been so wonderfully enriched by my association with him.

So it is indeed an honor to have been invited to contribute to this *Festschrift* in celebration of Reuben's 90th birthday. When Bharath Sriraman of the University of Montana sent the invitation, he noted that Reuben agreed to the volume with certain conditions—one of which was his "being able to shape this in order to break convention." Knowing Reuben, this response is hardly surprising. Consider, for example, how he came to be a mathematician.

Reuben earned his PhD in 1962, at the Courant Institute of Mathematical Sciences at New York University, writing his thesis on hyperbolic partial differential equations. The path toward this achievement was anything but conventional. In 1946, Reuben had graduated from Harvard earning a B.A. with honors in English literature. What, then, led him to Courant and to mathematics? Reuben told me that

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he initially worked in journalism, hoping to impact the world in a positive way. That not happening to his satisfaction, he then took a job as a lathe operator, believing that at least he could contribute something "concrete" to the world. An industrial accident cut that career short, and so, as I recall Reuben telling me, he wandered into Courant one summer day and requested admittance as a graduate student.

In an interview for the American Mathematical Society ([21] 2014), Reuben described to his editor, Edward Dunne, what he did after graduation from Courant: "I started out doing my job as a mathematician. What was that? I proved things." Indeed, reflecting on his conventional "mathematical self," he had made this observation: "I find mathematics an infinitely complex and mysterious world; exploring it is an addiction from which I hope never to be cured. In this I am a mathematician like all others" ([11] 1981, p. 2). But, that being said, Reuben would also traverse an unconventional path, emanating from his experiences in teaching a foundations of mathematics course at the University of New Mexico in the 1970s: "I have developed a second half, an Other, who watches this mathematician with amazement, and is even more fascinated that such a strange creature and such a strange activity have come into the world, and persisted for thousands of years" ([11] 1981, p. 2).

Reuben published extensively in applied mathematics. In addition, largely from his unconventional self, there has emerged a plethora of writings and activities promoting the idea that mathematics must, above all, be understood as a human activity, a social phenomenon, historically evolved, and intelligible only in a social context. See, for example, ([25] 1990), ([24] 1997), ([23] 1997). Indeed, according to Reuben, mathematics has existence or reality only as part of human culture. It is not infallible, and it is not unique. It is neither physical nor mental. It is social. It is part of history. It is like all those very real things which are real only as part of collective human consciousness ([24] 1997, p. 1).

Reuben distinguishes between the "front" and "back" of mathematics: the former consisting of polished results and the latter consisting of what mathematicians must do to obtain them.

> Front mathematics is formal, precise, ordered, and abstract. It's broken into definitions, theorems and remarks [. . .]. Mathematics in back is fragmentary, informal, intuitive, tentative. We try this or that. We say "maybe," or "it looks like." ([23] 1997, p. 36).

His humanist philosophy of mathematics focuses on the "back" of mathematics, and he advocates demonstrating this view in the classroom. For Reuben, the humanist philosophy brings mathematics down to earth, makes it accessible psychologically, and increases the likelihood that someone can learn it, because "it's just one of the things that people do" ([24] 1997, p. 4). To that end, the teaching of mathematics should expose students to the difficulties to which our present theorems offer solutions ([25] 1990, p.105).

Such a view of mathematics teaching had been advocated by Alvin White of Harvey Mudd College ([54] 1975), and Reuben became a strong proponent of White's program. See, for example, ([55] 1993), ([22] 2011). Today White's vision continues in the publication of online-only, open-access, peer-reviewed journal *The*

Humanistic Mathematics Journal [26], whose editors, Mark Huber and Gizem Karaali of the Claremont Colleges, describe in [26] this way:

> The term *humanistic mathematics* could include a broad range of topics; for our purposes, it means "the human face of mathematics. Thus, our emphasis is on the aesthetic, cultural, historical, literary, pedagogical, philosophical, psychological, and sociological aspects as we look at mathematics as a "human endeavor."

With Reuben's humanist philosophy of mathematics as a context, I now consider his challenge for the contributors to his *Festschrift.* Reuben had posed a series of questions about the future of mathematics research, mathematics education, and philosophy of mathematics emanating from this provocative statement that Paul Cohen had posed to him (Reuben didn't specify when): "At some unspecified future time, mathematicians would be replaced by computers."

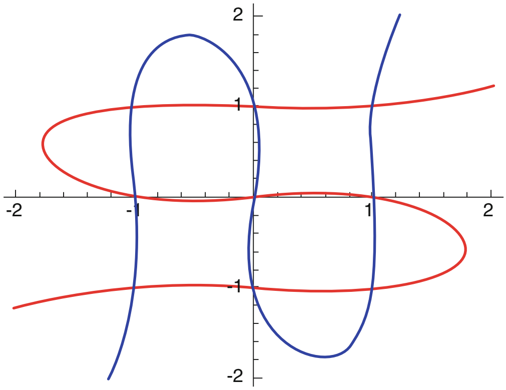
Any reasonable response to Cohen's assertion would need to point out the distinction between the replacement of a mathematician and the replacement of a mathematician's job. A spate of recent books such as the *Rise of Robots: Technology and the Threat of a Jobless Future* suggests Cohen was prophetic with respect to the latter: "A computer doesn't need to replicate the entire spectrum of your intellectual capability in order to displace you from your job; it only needs to do the specific things you are paid to do" ([17] 2016, p. 230).

Still, as early as 1950s, Alan Turing ([48] 1951) had predicted that there would come a time when computers would have intellectual capacities that exceed those of human beings, and when that happens, the machines would take control. The field of artificial intelligence (*AI*) emerged soon after, and in some circles, there is the belief that it can enable action beyond human control. See, for example, ([3] 2014). But, even if *AI* could achieve such superintelligence, would that suggest that mathematicians could be replaced by computers?

Turing offered no thoughts on whether machines taking control would be good or bad. But, with respect to wresting control from mathematicians, Reuben did. During the before-cited 2014 interview, Dunne posed this question: "Do you think a machine is capable of doing mathematics?" Reuben replied: "Why would we want it to? It is impossible and a terrible idea." At the foundation of this response is Reuben's philosophy of humanism. In human beings, intelligence is inseparable from social awareness, among other things. And for those who espouse a humanist philosophy, social interaction is an essential component of what makes mathematics grow.

What better way to support Reuben's view that a machine cannot replace a mathematician than to examine a piece of mathematics through the humanist lens? This is what I have chosen to do for Reuben's *Festschrift.* Essential to my discussion is exploring social interaction in the role of "conversation," as described by Steve Strogatz: "A very central part of any mathematician's life is this sense of connection to other minds, alive today and going back to Pythagoras. We are having this conversation with each other going over the millennia." See ([9] 2015).

For a glimpse into such conversations (albeit selecting a scant few among multitudes) and to provide what I believe is a compelling illustration of mathematics as social, part of culture, and part of history, I plan to discuss a result concerning the precise number of points of intersection of two plane curves.



This result would eventually become known as Bézout's theorem, named for a French algebraist Étienne Bézout who proved and generalized it. Precedents for the theorem can be found in the work of Colin Maclaurin ([33] 1720), Gabriel Cramer ([10] 1750), and Leonhard Euler ([15] 1764), among others. The first modern algebraic—and perhaps the first fully complete treatment of it—wasn't given until the twentieth century in ([32] 1916) by F.S. Macaulay ([19] 1984, p. 152).

It would take many "conversations over centuries" for mathematicians to recognize what conditions are necessary to precisely determine the number of points of intersection of two plane curves. Such conversations would significantly broaden the scope of the result, as well as vary the contexts in which it could be conceived and the strategies by which it could be proved.

The history of Bézout's theorem is richer and more complicated than I would ever be able to convey in this short article. I only hope to provide a glimpse, within a narrow window of time, into a few among the wealth of contributions toward its algebraic solution, as well as a glance at the emergence of a purely geometric approach to the theorem that led to alternative paths toward its proof. I begin with the algebraic story.

In the eighteenth century, Bézout in ([2] 1764), as had Euler in ([16] 1748), sought to prove, using determinants in algebraic elimination, that the number of points of intersection of two plane curves is given by the product of the degrees of the polynomial equations that represent those curves. To make this statement to be precise, the polynomials must have no common factors; the field must be algebraically closed; the points of intersection must be counted with the right multiplicities; and intersections at infinity must be considered. But these conditions would only become part of the collective consciousness regarding intersection multiplicities that would evolve over the course of centuries. The efforts of Bézout's and his contemporaries started the conversation.

Bézout's strategy for finding the number of intersection points was this: given two nonzero polynomials in two variables of degree m and n, respectively, derive a determinant from their coefficients, calling the polynomial equation given by the vanishing of the determinant the "resultant," and use the resultant to determine that the number of common solutions for the two equations is *mn*.

Like his contemporaries, Bézout was indebted to Maclaurin and Cramer for the method of solving simultaneous linear equations using determinants. To prove his theorem, Bézout was led to consider a certain determinant in the coefficients of P and Q viewed as polynomials in y whose coefficients are polynomials in x. How he derived it and what he did with it is why the determinant became known as "the Bézoutian" ([56] 1909, p. 327).

Bézout was able to simplify the calculations in the elimination method used to find the determinant, which is a function of one variable, and, in addition, to interpret it in a new way to find the number of solutions to the given polynomial equations. In the process, Bézout engaged in a "conversation over the centuries" with René Descartes, who, in ([13] 1637), had advocated a method of undetermined coefficients that involved the comparison of coefficients to find unknown quantities.

It was known to Isaac Newton and Gottfried Leibniz (and perhaps even Pierre de Fermat before them) how to apply a process for combining any two equations to eliminate one unknown ([14] 1985, p. 6). Bézout's contemporaries would apply this step-by-step "common factor" process with more equations and more variables. But Bézout had a new idea. He proposed instead to consider all the equations at once. He hypothesized that from $(k + 1)$ equations, k variables could be eliminated at the same time and that the result would be a polynomial linear combination of the given functions. The idea was to use multipliers with degrees sufficiently high so that the resultant would be univariate and would have the smallest degree sufficiently great for this purpose.

Then Bézout did something else that was innovative. Unlike his contemporaries, he did not solve for the roots of the resultant equation. Indeed, it is often difficult to determine the nature of the roots of the resultant. Instead, he showed that the degree of the resultant is the number of common zeros of P and Q.

However, Bézout addressed the concept of intersection multiplicity, on which his theorem hinges, only heuristically. He treated multiple points of curves by implication only, locating them at infinity and showing, for particular cases, how they affect the number of finite intersections ([56] 1909, p. 332). To make the proof of his theorem, rigorous:

1. Multiplicities of intersection points need to be correctly counted. Consider, for example, the case where P and Q are represented, respectively, by the equations $x^2 + y^2 – 1 = 0$ and $x^2 + 4y^2 – 1 = 0$. By Bézout's theorem, the number of intersection points is four. But in fact that number is only two, because each intersection point of the circle and the ellipse has multiplicity two.
2. The exclusion of repeated factors in the equations for the curves is necessary to avoid the possibility of infinite intersections. Consider, for example, the case where P and Q are represented, respectively, by $x^2 - xy = 0$ and $x^2 + xy = 0$. By Bézout's theorem, the number of intersection points is four. But in fact that number is infinite because their intersection contains the y axis $(x = 0)$.

3. The context for the theorem needs to be changed. Descartes' analytic geometry cannot account for all intersections because it fails to be closed in either the algebraic or the geometric sense. The reals are not closed algebraically because they do not admit imaginary points. Consider, for example, the case where P and Q are represented, respectively, by $y = x^2$ and $y = mx + b$. By Bézout's theorem, the number of intersection points is two. However, when m is finite, the intersection points of the parabola and the line can be two real and distinct points (if $m \neq 0$), two complex and distinct points (if $m = 0$ and $b < 0$), or one multiple point (if $m = b = 0$). If, on the other hand, we remove the restriction that m is finite and let m increase without bound, then $y = mx + b$ becomes a vertical line, and the number of real intersection points is one because the Cartesian plane does not admit points at infinity.

And so the conversations continued, with mathematicians introducing new strategies to confront these issues. During the eighteenth century, when analysis dominated, Gaspard Monge attempted to reengage mathematical thinking along geometric lines. In a "conversation over the centuries" with Girard Desargues, Monge promoted synthetic methods, which are characterized by direct consideration of geometric figures, rather than by the translation of the properties of these figures into equations. Monge's pupil, Jean Victor Poncelet, with ([42] 1822), would effect a revival of projective geometry in the nineteenth century and in doing so provided a more satisfactory context for mathematicians to attempt to properly count all intersections—real, complex, and those at infinity—in proving Bézout's theorem.

Poncelet proposed a definition of intersection multiplicity based on the idea that if figures have certain properties, then these properties hold for corresponding figures obtained by "continuous transformations," for example, homologous figures obtained by projection and section ([42] 1822, p. 68). Known as Poncelet's "principle of continuity," or "principle of conservation of number," this idea, as developed by Hieronymus Georg Zeuthen and Hermann Schubert, is "at the foundation of the research in enumerative geometry" ([14] 1985, p. 68), the object of which, classically, was to find the number of geometric figures satisfying given geometric conditions, in terms of invariants of the figures and the conditions ([30] 1976, p. 299).

To define the intersection multiplicity at one point of two figures P and Q, Poncelet would vary one of them, say P, in such a way that for some position P′ of P, all the intersection points with Q should be simple, and so one could count the number of these points which collapse to the given point when P′ tends to P, in such a way that the total number of intersections (counted with multiplicities) would remain constant. For example, a point of tangency of a line with a curve is a limit of intersections of nearby secant lines. Poncelet's principle for curves in a plane, which stipulates their number of intersection points is a continuous invariant, can be interpreted algebraically in this way: a change in the coefficients of a polynomial equation does not affect the number of roots, provided the change does not annihilate the leading term of the polynomial.

Appealing to his principle of continuity, Poncelet, in ([42], 1822), proved Bézout's theorem. He proceeded in this way:

> Given two curves P and Q in the complex projective plane, of degrees m and n respectively. Observe that the curve Q, for example, belongs to the continuous family of all curves of the same degree n, and that in that family there exist a proper subset of curves, call it S, which have a multiple point belonging to P or pass through a multiple point of P or are tangent to P at a common point, simple on both curves. Let R be a curve of degree n not in S. Then P and R have in common only simple points where their tangents are distinct. So R can be a deformation of Q that is union of n distinct lines. Then each line meet P in m distinct points and so P and R meet in mn distinct points. Take the multiplicity of the intersection of P and Q at a point r to be the number of points of intersection of P and R that tend toward r as R tends toward Q. Then the weighted number of intersections of P and Q is mn.

In his proof, Poncelet assumed the number of simple points of P and R (where their tangents are distinct) is constant due to the principle of continuity. There was some controversy over his assumption, but, ultimately, it can be justified in the plane using the continuity of the roots of an equation as a function of the parameters and the fact that the complement of the union of the curves in S is connected ([14] 1985, pp. 67–68). Indeed, Poncelet's principle became part of the collective consciousness in the nineteenth century. See ([28] 1985) and ([60] 1915, Chapter 5).

A few decades after Poncelet introduced his principle, Michel Chasles would propose a principle which provided a way to discuss a correspondence that can be expressed by an algebraic equation, without having to know that equation. Chasles defined an (m, n) correspondence as a relation between two points x, y, varying on the same projective line such that to each point x there exist m points y related (or corresponding) to x and to each point y there exist n points x related to y. His "principle of correspondence" asserts that the number of points x (counted with multiplicity) that are fixed (i.e., each x is related to itself) is $m + n$, unless the number is infinite (and so every x is fixed). The principle holds because the graph of the correspondence (m, n) is defined by a bi-homogeneous equation of degree m in x and n in y, so setting $x = y$ yields an equation of degree $m + n$ or one that is identically zero. Chasles exploited the projective property of duality in a plane to transfer the principle from a correspondence between points on a line to a correspondence between lines through a point. See ([8] 1855), ([7] 1864).

Chasles's principle, like that of Poncelet, became part of a network of shared concepts among projective geometers. Zeuthen, for example, in ([59] 1873), wrote about its "great importance" and provided his own proof of it.

Chasles used the transferred principle, as well as Poncelet's principle, in ([6] 1872), to construct a synthetic proof of Bézout's theorem in the plane. He proceeded in this way:

> Let P and Q be two curves in general position, of degrees m and n, respectively, in a complex projective plane. Compute the intersections of P and Q by first taking two points o, s in general position in the plane of P, Q. Since the degree of P is m, a line will intersect it in m points; likewise, Q by a line in n points. Construct line ox to meet the curve P in m points α. The lines drawn from these points α to s meet the curve Q in mn points of contact α' since by Poncelet's principle, the number of intersection points is not changed by projection. Through these α' draw mn lines oo'. These mn lines correspond to the line ox.

> In the same way, to each line *oo*', which meets the curve Q in n points correspond nm lines *ox* because each *oo*' meets Q in n points α', and the lines in these points intersect the curve P in nm points α, through which pass the nm lines *ox* corresponding to *oo*'. The principle of correspondence (appealing to duality which transfers the correspondence from the family of lines that intersect in a point, to the set of points on a line), says there are $mn + nm$ lines *ox*, each coincident with one corresponding line *oα'*. But nm of these lines coincide with *os* which does not have any points in common with P and Q. And the other mn lines are those that pass through a point α on the curve P which is coincident with one of the points α' on Q. Thus the points of intersection of P and Q are mn in number.

In this nice synthetic proof, there continue to be issues with the assignment of intersection multiplicities. For example, of the $mn + nm$ lines *ox*, each coincident with one corresponding line *oα'*, Chasles claims, without any justification, that nm of them coincide with *os.* Nonetheless, while Chasles's proof would not be considered rigorous by today's standards, he was able to convince other important mathematicians, including Zeuthen ([59] 1873, Note 4), Georges Fouret, and Mario Pieri, of its validity. Fouret, in ([18] 1872-3), and Pieri, in ([40] 1888), generalized Chasles's proof for higher dimensions. Zeuthen would ultimately demonstrate his own proof of the theorem in ([57] 1914) without reference to Chasles's principle.

It would take centuries for the idea of intersection multiplicity for plane curves to be made rigorous. Conversations swirled around static and dynamic approaches. In the eighteenth century, by defining multiplicity in terms of the resultant, Bézout was among those who had used a static approach where the equations are not varied, reducing the question of the intersection multiplicity of two curves to the multiplicity of a root of a polynomial in one variable. In the early nineteenth century, Poncelet adopted a dynamic approach, where the multiplicity of a solution is the number of solutions near the given solution when the equations are varied. Some decades later, Arthur Cayley ([4] 1863) and others would develop a dynamic definition in the following way: to determine the multiplicity of a point of intersection p of two plane curves, add together individual contributions to the multiplicity of p coming from a pair of branches at that point, one from each curve. These individual contributions are obtained through the intersection of the pair of branches by a line parallel to the y axis with abscissa tending toward p and multiplying together the number of points on each branch which collapse toward p. See ([14] 1985, Chapters 4 and 6). By the twentieth century, these ideas would be made rigorous notably by Francesco Severi ([46] 1912), Bartel Leendert van der Waerden ([51] 1927), and André Weil ([53] 1944). Mathematicians would propose definitions such as these for the multiplicity of a curve at a point: Algebraically, the multiplicity of a point p of a plane curve P of degree n at a finite distance is defined as the smallest degree of a term occurring in the expansion of P about p. Geometrically, p has multiplicity d if most lines through p meet P at p in d coincidental points or if they meet P outside p in n-d points.

However, new issues regarding intersection multiplicity emerged as mathematicians sought to generalize Bézout's theorem. Bézout himself constructed an analytic proof that if n hypersurfaces of degrees $d_1, d_2, \ldots d_n$ in n-space, intersect in a finite number of points, then the number of common intersection points is the product of the degrees, $d_1 * d_2 * \ldots * d_n$. He did this by generalizing the resultant to the case of n polynomials in n variables. See ([2] 1764), ([1] 1779). But this methodology does not universally apply in more than two dimensions. Three surfaces may not only have a finite set of points in common but also a curve or a system of curves, in which case their resultant may vanish identically, having an infinite number of roots. For example, three quadric surfaces can intersect in one, two, or three lines, a conic, or a twisted cubic. According to Bézout, these three quadratic surfaces should intersect in eight points. But if they have a common line, four of those intersection points are absorbed. If they have a common conic, six of the intersection points are absorbed, and if they have a common twisted cubic, all eight of the intersection points are absorbed. Except for the case where the common curves are straight lines at infinity, Bézout did not address this problem ([56] 1909, p. 336).

In the 1800s, George Salmon, also appealing to the algebraic theory of elimination, would extend Bézout's theorem to three dimensions. Observing that curves in space are classified according to the number of points in which they are met by a plane, he noted that "three equations of degree *m, n,* and *p,* respectively, denote mnp points." Salmon claimed that "this follows from the fact that if we eliminate y and z between the equations, then we obtain an equation of the degree mnp in x." Thus, he concluded, "this proves also that three surfaces of degree *m, n,* and *p,* intersect in *mnp* points" ([43] 1882, p.15). See also ([44] 1866, pp. 61–63).

On page 299 of ([43] 1888), Salmon referenced the following statement which he called a "principle": a curve of degree r meets a surface of degree p in pr points. He claimed "this is evident" when the curve is the complete intersection of two surfaces whose degrees are m and n because then $r = mn$, and the three surfaces intersect in *mnp* points. He then proposed that this is true also "by definition" when the surface breaks into p planes. Salmon ultimately indicated: "We shall assume that, in virtue of the law of continuity, the principle is generally true." But was his appeal to the "law (principle) of continuity" valid?

Poncelet had successfully applied the principle of continuity for a proof of Bézout theorem in the plane, assuming a plane curve belongs to the continuous family of all curves of the same degree and that, in this family, there exist curves which degenerate into a system of straight lines, each meeting a fixed curve in distinct points. But Poncelet's principle is not easily extended beyond the plane. Mathematicians in the nineteen century (and indeed into the twentieth) were generally not aware of the potential hazards of so extending Poncelet's principle. See ([31] 1976).

In a major report on enumerative methods in algebraic geometry ([60] 1915), Zeuthen and Pieri made the following statement: "To establish that, in general, *mn* is the number of points common to a surface of the m^{th} order and to a curve of the n^{th} order, it is legitimate to replace the surface by a system of m planes, whereas it is not permissible to substitute for a curve a system of n lines" ([60] 1915, pp. 275–276). Since Salmon kept the curve fixed and just moved the surface until it broke into p planes, his reasoning was sound.

George Halphen ([20], 1873–1874) would cite Salmon's result for three dimensions, when he constructed a proof of Bézout's theorem in n dimensions. Halphen began by using fractional power series to determine the intersection multiplicity of two curves at a point, changing coordinates so that the points at infinity are moved to be at finite distance. He proved that the number of intersections of two curves, converging in a point O, is equal to the sum of the orders of the infinitely small segments intercepted by the two curves on a secant whose distance to the point O is infinitely small of the first order and which does not coincide with any tangent to one of the curves in this point. But when he generalized this process for n dimensions, he stumbled.

So again, the conversations continued. In ([36], 1877), Max Nöther addressed the error in Halphen's proof and corrected it. Ultimately, however, in ([50], 1928), van der Waerden showed that length multiplicity would not provide a correct measure for Bézout's theorem. The conversations continued. Van der Waerden ([51] 1927) and Weil ([53] 1944), according to Severi ([46] 1912), would rescue Poncelet's principle. Precise definitions of intersection multiplicity in algebraic intersection theory would endeavor to make proofs of Bézout theorem rigorous. But before that happened, the conversations about the theorem would be expanded, adding synthetic geometric arguments into the mix of analytic algebraic ones.

Mario Pieri was a key contributor to that conversation. In ([40] 1888), Pieri generalized the theorem to n dimensions, but for two varieties of complementary dimension, instead of for n hyperspaces. In conversation with historical and contemporary mathematicians, he would help to shift the focus from algebra to geometry: He expressed the theorem in purely geometric terms, advocating a purely synthetic proof rather than an analytic one, replacing elimination methods with enumerative ones.

Pieri's generalization of Bézout's theorem for complementary varieties in projective n-space P(n) involved a generalization of the notion of degree. Namely, the degree of a k-dimensional variety X in P(n) is the number of points in its intersection with a general (n - k)-dimensional linear subspace of P(n). In his synthetic proof of the generalized theorem, Pieri focused his consideration exclusively on geometric figures, rather than on equations derived from the properties of these figures. To create the required intermediate geometric figures, he made use of the geometric processes of projection and of section. Using enumerative methods to obtain the number of intersection points, he appealed to Poncelet's continuity principle. Pieri also engaged in "conversation" with Chasles, both with respect to generalizing Chasles's correspondence principle as well as Chasles's proof of Bézout's theorem for curves in a plane.

Before he constructed his proof of Bézout's theorem in n-space, Pieri proved Chasles's principle of correspondence for n dimensions ([41], 1887): He enumerated the number of coincidences in any algebraic correspondence between two n-dimensional projective spaces, using the methods of projection and section, and the principle of mathematical induction. For his inductive hypothesis, Pieri was in conversation with Chasles ([7] 1864), Salmon ([43] 1865), and Zeuthen ([58] 1874), who had established such coincidences for dimensions up to $n = 3$. Pieri's generalization may be called rigorous by modern standards. See ([19] 1984, pp. 315–316), ([45] 1912, p. 678).

Pieri then used this result in his proof of Bézout's theorem ([40] 1888). He began by proving the following lemma:

> Let V(p) and W(q) be algebraic varieties of dimension p and q, and degree r and s in an n-dimensional projective space P(n). To prove that when $p + q = n$, the degree of (V $\cap$ W) $= rs$, first prove that in P(n – 1), there are (t)(r)(s) lines that meet P(t - 1) and P(n – t – 1) and two linear varieties V(t - 1) of degree r and W(n - t - 1) of degree s, where t - 1 $\leq$ (n - t - 1).

Pieri's proof of this lemma involved degenerating V and W into two unions of linear spaces and appealing to Poncelet's principle of conservation of number and Pieri's own n-dimensional generalization of Chasles's correspondence principle. Next, Pieri generalized Chasles's synthetic proof of Bézout's theorem. He let the two varieties V(p) of dimension r and W(q) of dimension s remain fixed. Fixing two auxiliary linear spaces P(n - p - 1) and P(p - 1) and generalizing Chasles's auxiliary points, Pieri uses them to set up a correspondence, to which his n-dimensional generalization of Chasles's correspondence principle is applied. He then is able to prove Bézout's theorem using the principle of mathematical induction, assuming it is true in lower dimensions, as had been proved by Chasles in ([6] 1872), for two dimensions, and Fouret in ([18], 1872–3) for three.

Pieri constructed his proof with an awareness of the collective consciousness about the theorem, from both the analytic and the synthetic perspectives. He followed Fouret in generalizing Chasles's synthetic proof. But like Chasles and Fouret, he did not provide his own definition of intersection multiplicity. This is likely because he accepted the analytic one for the n-dimensional case that had been given by Halphen in ([20], 1873–1874). Indeed, Pieri observed that the analytic premises on which his synthetic proof was founded could be reduced to the theorem that an algebraic equation of degree n has n roots (or else an infinity of them).

While Pieri's use of Poncelet's principle can be made entirely rigorous as demonstrated by William Fulton in ([19] 1984, pp. 127–127,180–185,193–194) and by Solomon Lefschetz in topological intersection theory (see [29] 1980, p. 124), validating Pieri's proof of Bézout theorem would not be an easy task. That is perhaps why it, and other synthetic proofs of the era, are not as well know as they should be. In a sense, we have come full circle traversing the path from the algebraic approach of Bézout to Pieri's synthetic one: Modern mathematics would advocate rigorously proving geometric statements, such as Pieri's, by translating them into algebraic equivalents.

That being said, Pieri did much to enrich the conversation at the turn to the twentieth century, continuing to explore issues surrounding Bézout's theorem. See, for example, ([39] 1891), ([37] 1897). He played a role in paving the way for the ultimate resolution of the theorem in modern algebraic intersection theory, begun by Weil ([53] 1944), in conversation with Severi ([46] 1912) and van der Waerden ([50] 1928) and others in the 1930s, culminating in Fulton's work ([19] 1984) and that of Steven L. Kleiman ([27] 1987). Indeed, Fulton ([19] 1984, p.318) noted that Pieri's fixed point formula in ([39] 1891, p.265) stands out as a precursor of modern excess intersection theory. Wolfgang Vogel's assessment of Pieri's contribution was even stronger: "It seems that a starting point of an intersection theory in the non-classical case was discovered by M. Pieri" ([52]1984, p. 11).

The collective consciousness about intersection multiplicities in modern times that reflect the historical evolution of thought, inspired by Bézout and his contemporaries, and enriched by those engaged in conversation over the centuries, is evident in these statements of Bézout's theorem for the plane and in n-dimensional space ([19] 1984, pp.14, 144–152):

1. The sum of the intersection multiplicity for all common points of the two projective plane curves (assumed irreducible and distinct) over an algebraically closed field is the product of the degrees.
2. If n hypersurfaces of degrees $d_{1,}$ d_2, $\ldots d_n$, intersect transversally, or if they intersect in finitely many points counted with their proper multiplicity, then the number of common intersection points is the product of the degrees: $d_1{}^* d_2{}^* \ldots {}^* d_n$. Hypersurfaces intersect transversally at a point P when each one is smooth at P, and P is the only point of intersection of their tangent spaces. The intersection of the n hyperspaces themselves is said to be transverse, if it is transverse at each of its points.

The same can be said regarding proofs of the theorem. Navigating the path toward the ultimate justification, in intersection theory and beyond, of the result and all it has come to represent gives an appreciation of mathematics as a process, during which progress is both impeded and stimulated by the collective consciousness at any given time. In the eighteenth century, no general attempt was made to attach an integer measuring the "multiplicity" of the intersection to each intersection point in such a way that the sum of multiplicities should always be the product of the degrees of the intersecting figures ([52] 1984, p. 2). By the early twentieth century, mathematicians were either struggling to properly define intersection multiplicity or took it for granted. Even when the notion was well defined, it was still not obvious if the intersection multiplicity computed for figures in general position is valid. Nor was it obvious for which cases the use of the principle of conservation of number could be correctly applied. See ([31] 1976).

What I have discussed here, opening only a small window on the evolution of thought about Bézout's theorem, illustrates the social nature of mathematics and the avenues that emerge because of it. Indeed, in presidential address to the American Mathematical Society in 1908, which focused on Bézout's theory of resultants and its influence on geometry, Henry White said:

> The accepted truths of today, even the commonplace truths of any science [...] were the doubtful or the novel theories of yesterday. [...] The first effect of reading in the history of science is a naïve astonishment at the darkness of past centuries, but the ultimate effect is a fervent admiration for the progress achieved by former generations, for the triumphs of persistence and of genius ([56] 1909, p.325).

White further observed: "A life of unremitting labor is not ill spent if it leaves a work so easily intelligible, so full of interesting problems, and in proportion to contemporary science so complete as this *Théorie générale des équations algébriques* of Bézout" ([56] 1909, p. 335). His observation was made more than a century ago!

The narrow glimpse I have provided into the history of Bézout's theorem references only a few of the many mathematicians who, with "persistence" and "genius," impacted the progress toward its solution and generalization. And what an interesting journey!

Today, the name *Bézout'*s *theorem* is part of the collective human consciousness, part of the "front" of mathematics, "attached to a number of theorems in algebraic geometry concerning intersections of arbitrary cycles" (formal sums of irreducible varieties) on projective space "and often to more general situations whenever an intersection ring of a variety is explicitly computed" ([19] 1984, p.152).

A full exploration of the journey to this point would truly reveal what Reuben calls the "back" of mathematics, rich in human activity replete with twists and turns, failure and successes, derivations and innovations, and multiple examples of mathematicians conversing over decades and centuries.

Reuben had asked, in the context of Cohen's premise: "Will the computer change mathematical research, mathematical philosophy, mathematical teaching?" It would be difficult to find any mathematician who would say no. James T. Smith of San Francisco State University put it nicely: "The advent of the computer has provided a huge increase in the subject matter inviting mathematical research, mathematical-philosophical inquiry, and mathematical teaching. New mathematics, new philosophical inquiry, and new approaches to teaching result" ([47] 2017).

That being said, I believe there are limits, despite claims of a super-intelligent AI, on what a computer controls. It is subject to human intervention. It can only be motivated to do what humans prescribe. It is not curious. It cannot follow hunches. It is not innovative. It cannot initiate changes in contexts. It cannot take into account, to the extent mathematicians do, cultural and historical successes and failures. Indeed, notwithstanding the fact that in today's world computer algebra systems are used to solve many problems in algebraic geometry, the evolution of thought about Bézout's theorem suggests that a computer can add to the conversation, but not replace it. It cannot replicate the social interaction that in Reuben's view is essential to the growth of mathematics.

Allow me to close with a personal comment about the impact Reuben has had on my mathematical life. He has mentored me, encouraged me, and guided me to many interesting projects. He orchestrated my introduction to other mathematicians, with whom I have had the opportunity to collaborate—in particular, Phil Davis of Brown University ([12] 2012) and Dick Stanley of UC Berkeley ([49] 2003). Others of my associations that came independently of Reuben circled back to him. Anneli Lax of the Courant Institute of Mathematical Sciences at New York University was my thesis advisor. I developed a deep and enduring friendship with her and her husband, Peter Lax. It was only later that I learned Peter had been Reuben's thesis advisor. So it seemed that almost by destiny our lives and our work became intertwined with Reuben's. See, for example, ([34] 1999), ([55] 1993).

Thank you Reuben for your many conversations with me—both mathematical and non-mathematical. Regarding the latter, I recall, with fondness, your reaction to reading (at my request) one of Anton Chekhov's short stories. You wrote: "I finally read 'The lady with the dog' and Chekhov's vision of human life. Joy is inseparable from tragedy. Reality indistinguishable from the imaginary and the ideal. Maybe even good and evil inseparable, impossible without each other." Well said! Thank you Reuben, for inviting me, so many times, to see the world through your eyes.

Acknowledgments I thank Christopher Diorio, Paul Hoffmann III, Beverly Kleiman, and Michelle Marchisotto for conversations with them that gave me insights into directions to pursue this article. I am deeply grateful to Steven L. Kleiman for the gift of his time and expertise in enriching my mathematical thoughts about this topic and as always to James T. Smith for his insightful comments and suggestions.

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A Gift to Teachers

Nel Noddings

Reuben Hersh's *What Is Mathematics, Really*? is a beautiful gift to math teachers. For many years, despite wave after wave of innovations, math teachers have been bogged down by the insistence that they teach a litany of facts, skills, and concepts that most students will never use. The widespread use of technology seems only to have increased this tendency. Teachers are told to state a carefully specified objective, provide instruction designed to accomplish it, and test, test, test!

The classic mathematical philosophy of Platonism has contributed to the heavy stuffiness of mathematics education. Hersh comments:

> Platonism can justify a student's certainty that it's impossible for her/him to understand mathematics. Platonism can justify the belief that some people can't learn math. Elitism in education and Platonism in philosophy naturally fit together. Humanist philosophy, on the other hand, links mathematics with people, with society, with history. (1997, p.238)

In *Loving and Hating Mathematics* (2011), Hersh and Vera John-Steiner build on the humanist idea to argue strongly that educators should give greater attention and respect to subjects other than mathematics. Perhaps mathematics should not play such a central role in college admission. After all, many significant intellectual activities do not involve mathematics, and students should not be made to feel academically inferior because they are not interested or proficient in mathematics. That said, there are everyday tasks that require some mathematics, and students should be encouraged to develop the skills and understanding necessary for everyday life. Acknowledging this basic need, Hersh and John-Steiner still suggest (and I agree) that we should discontinue "the use of mathematics as an academic filter" (2011, p.230):

> Instead, the goal is to treasure diversity in talent and interest; to provide advanced mathematics teaching/learning to motivated students, while decreasing the number who

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> suffer from math phobia. The challenge is to develop a systematic, society-wide perspective, rather than imposing the same values and approaches on both enthusiastic and reluctant learners. Because we love mathematics, we want to minimize the number of those who hate it. Our purpose in these proposals is to shift the premise of the current debate. It is to create a humanistic role for mathematics and its teaching in our culture, a way of teaching mathematics that focuses on the needs and abilities of students as well as society. (2011, pp. 330–331)

In agreement, I have argued that all educators—perhaps all citizens—should think more deeply about the aims of education. In recent work (Noddings, 2013, 2015), I have suggested that the main aim of education should be to produce better people and to think deeply about what we mean by "better." Surely, we want people to be "better" socially, intellectually, morally, aesthetically, and physically. We want good citizens, good parents, good friends, good workers, and people who are culturally well informed and aesthetically appreciative. And, yes, what we mean by "better" should be forever open to lively and sensitive exploration; we must not allow the term to be defined by ten specified goals or instructional objectives.

Hersh has contributed powerfully to this vision in his humanist description of mathematics as a form of life. As a form of life, it is necessarily connected to forms of life beyond mathematics. In education, this is especially important. We must share these connections and get rid of the long and stubbornly held view of mathematics as a disciplinary specialty that everyone can master if he/she works hard enough. We should put greater emphasis on interdisciplinary studies.

Hersh illustrates what can be done with a wonderful set of short biographical/historical anecdotes. All kids are commanded to learn the Pythagorean theorem. How many get to hear that Pythagoreans were forbidden to eat beans or that they constituted a religious brotherhood? How often are the religious beliefs of Descartes or Spinoza discussed? (Is Spinoza even mentioned in high school math classes?) Do math students hear anything about Bertrand Russell's antiwar efforts? Is his atheism mentioned? Do they hear the name of John Stuart Mill or have an opportunity to discuss his views on the power of open, intelligent, civil dialogue? Hersh summarizes his support for treating mathematics as a form of life connected to other forms:

> Mathematics is like money, war, or religion—not physical, not mental, but social. Dealing with mathematics (or money or religion) is impossible in purely physical terms . . . It can only be done in social-cultural historic terms. This isn't controversial. It's a fact of life. (1997, p.248)

Hersh's views are supported by other interdisciplinary thinkers today. E.O. Wilson, famed biologist, urges biology teachers to reach outside biology: "The ongoing explosive growth of knowledge, especially in the sciences, has resulted in a convergence of disciplines and created the reality, not just the rhetoric, of interdisciplinary studies" (2006, p.135).

A recent biographical account of Alexander von Humboldt reminds us that interdisciplinary thought and dialogue were treasured by Ralph Waldo Emerson, Edgar Allan Poe, Walt Whitman, and Henry David Thoreau. Andrea Wulf (2015) notes that Humboldt rejected the temptation to draw sharp lines between the arts and sciences and between the subjective and objective:

> As scientists are trying to understand and predict the global consequences of climate change, Humboldt's interdisciplinary approach to science and nature is more relevant than ever. His beliefs in the free exchange of information, in uniting scientists and in fostering communication across disciplines, are the pillars of science today. His concept of nature as one of global patterns underpins our thinking. (2015, p.336)

Endorsing the humanist approach to mathematics, Hersh concludes:

> Recognizing that mathematics is a social-cultural-historical entity doesn't automatically solve the big puzzles in the philosophy of mathematics. *It puts those puzzles in the right context, with a new possibility of solving them*. (1997, p.249)

Similarly, expanding the teaching of mathematics into vital interdisciplinary studies will not solve all of our pedagogical problems, but it will put us on a more relevant, far more exciting, path.

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The Philosophy of Reuben Hersh: A Nontechnical Assessment

William Labov

Reuben Hersh and I attended Harvard College in the 1940s. It was during the Second World War, when, as it was said, "they let anyone in." The only training I had in mathematics was a first year Calculus course, and I have been trying to catch up ever since. Connections between philosophy and science may not have been as strong then as they are now. But I remember the reaction of my freshman advisor John Wild, a Thomist philosopher, when he spotted a Chemistry B course on my program. "Where," he asked, "did you get that *idolatry of science*?" I was stunned. "This man is really intelligent," I thought. "How could he tell from that one course that I have an idolatry of science? Because I do."

Our Harvard contacts were not so much in the classroom as in political action groups. Reuben and I had the great pleasure of working for FDR in his third term victory of 1944 and campaigned for a united front before the red-baiting days of the 1950s did so much damage at Harvard. Reuben appeared to me to be an informed Marxist. With a major in English and philosophy, I knew enough from my philosophy course to qualify as a materialist. We were convinced that most of the social problems in our society were the product of the inherent contradictions of capitalism.

On graduation, we both pursued a career remote from any university. Each of us made efforts to resolve some of those contradictions and make that larger world a bit better. As his Wikipedia page reports, Reuben went to work as a machinist and applied himself to the politics of union organization. My efforts were less well-thought out. I had a 3-months' temp as an editorial assistant at Alfred A. Knopf and should not have been surprised when my option was not picked up after I marched with the Knopf contingent on May Day. I lost a job at Drug Trade News when I unthinkingly left a subscription list for the *Daily Worker* open on my desk. Given

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the needs of a growing family of four children, I turned to industry and spent the next ten years in the laboratory as a formulator of printing inks and silkscreen inks.

At the end of that decade, each of us for different reasons returned to the university to pursue a different career. Reuben cut off his thumb with a band saw, which ended his work as a machinist. I discovered that my skills at formulating new and better printing inks would be forever buried in the domain of "trade secrets." Without knowing, we followed parallel paths, both to graduate school in New York City: Reuben at the Courant Institute, I in the Department of Linguistics at Columbia.

I cannot follow the direction of Reuben's thinking in his early graduate years, as we were not in touch. What I know is from *What is Mathematics, Really?* and more recent conversations. It will take me a paragraph or two to show how I was brought to face many of the same problems as are dealt with in that book.

I entered the field of linguistics as an exciting and lively domain in which theorists vigorously debated the merits of their solutions to the problem of mapping the abstract and complex structures that unite form and meaning. The data were primarily the linguist's (or native speaker's) feelings about what could be said. I had the idea that the field could advance on a more solid basis if the data were what people actually did say, and for that purpose, I proposed to use a new invention, the tape recorder. Though it had been invented by the Germans before World War II, it had found little use in linguistics up to that time. When I entered the community and engaged people in lively conversation about important events in their lives, I found that their speech varied in ways that challenged description. Until then, the only numbers in linguistics papers were the page numbers in the upper right corners. I presented my first results by cross tabulations that showed independent dimensions of social and stylistic stratification. Here I wished that I had gone farther than that one Calculus course at Harvard. Many of the dependent variables were binary, and in the late 1960s, logistic regression was not generally available in the standard packages. David Sankoff of the University of Montreal wrote such a program, and the field (now known as the study of linguistic change and variation) accelerated around it. The NWAV conference on "New Ways of Analyzing Variation" is now in its 46th year and accounts for a substantial fraction of linguistic research.

In the course of this work and the recording of some 7,000 individual speakers in various parts of the English-speaking world, I found myself taking a strong position on the role of the individual in linguistic analysis that he or she didn't count for very much. It appeared that the variation in the speech of each individual was the product of an unconscious pattern characteristic of the community pattern. In spite of such widespread variation, the language used by each individual was a product of the speech community, the intersection of all the communities he or she belonged to. My most recent paper, delivered in Taiwan, was entitled "What must be learned: the tyranny of the speech community." I should add that the argument has not been won and many linguists continued to take the individual as the fundamental unit of linguistic analysis. The most commonly used tool for statistical analysis is now mixed level regression analysis, which adds the effect of random independent variables (such as the individual speaker) to the fixed factors that are community property. So far, I have seen little additional profit from the addition of such random variables to our understanding of linguistic principles except the reassurance that all bases were being covered. My Taiwan paper advanced as "the central dogma of sociolinguistics," the position that the community is conceptually and analytically prior to the individual. It began with Durkheim's characterization of language as a *social fact*, defined as

> . . . ways of behaving, thinking and feeling, exterior to the individual, which possess a power of coercion by which they are imposed on him
> – *Règles de la méthode sociologique*, 1937 p. 5

Over the years I had not maintained the close connection with Reuben that I would have liked. After his retirement I visited him at his home in Santa Fe. But one day I was startled to receive an email message to the effect that Reuben Hersh was stranded in England without passport, requesting that I send a substantial sum by international mail to a London postal address. I responded without hesitation or second thought, but it was several days before it occurred to me to call Santa Fe to find that Reuben was happily at home and had never been in England. This event left me astonished at the effectiveness of such phishing expeditions and wondering how the authors could possibly have known that my friendship with Reuben was so deep and so longstanding that I would respond without the least intervention of common sense. It also led me to return to *What is Mathematics, Really? [WMR]* and pursue the question of what we shared in our professional work, in spite of the fact that I was no more than a mediocre user of common statistical programs.[1]

I turned to the index of WMR to see if it included Durkheim's view of the coercive character of social facts, a reference I had used in my own work. There are indeed two references to Durkheim in WMR, and turning to page 14, it is evident that Durkheim's view is central to Hersh's thinking. WMR draws upon the mathematician L. White to present Durkheim's position:

> Collective ways of acting and thinking have a reality outside the individual who, at every moment of time, conform to it. These ways of thinking and acting exist in their own right.

WMR concludes with the social constructivist position in the philosophy of mathematics (as formulated by Paul Ernest):

> objective knowledge of mathematics exists in and through the social world of human action, interaction and rules, supported by individuals' subjective knowledge of mathematics (and language and social life). WMR p. 228-9

It is perhaps not so remarkable that Reuben and I arrived at similar positions on the coercive character of social facts, given our early beginnings from a similar platform. But there is one more coincidence that I would add to the account. In January of 2015, my wife and I visited Reuben and Vera Steiner in their home in

[1]I have long been aware of the limitations of my mathematical background, though I have read more than a few general accounts of relativity theory. One day in the 1970s, I asked my astrophysicist son Simon whether it was possible to understand relativity theory without a control of the equations. He said yes, it was, but I have never believed him.

Santa Fe. Driving through the mountains of New Mexico, I had found that I had trouble breathing. The next morning, an ambulance took me to Santa Fe's Christus St. Vincent Regional Medical Center with a severe case of influenza. Reuben visited me as I was recovering. While we were talking, the nurse assigned to me came into the room. She stared at Reuben with great surprise. "I know you!!" she said. "You were in the picket line!" And indeed, it appeared that Reuben and Vera stood out as older figures on the picket line in the community support of a nurses' strike at St. Vincent's in the summer of 2014. "I wouldn't be here if it wasn't for you," the nurse told him. "I'd have eight patients to look after instead of five."

So it is only natural that I responded with pleasure to the invitation to contribute to this volume in honor of Reuben Hersh. With distinct but parallel trajectories, we have arrived at similar end points. Seventy years' engagement with intellectual and social issues has led us to the conclusion that the answers to these puzzles lie in the better appreciation of our fellow human beings.

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Friends and Former Comrades

Chandler Davis

My life as a Communist pervades and colors my life as a mathematician, in ways normally kept silent. The same is true of many others: Dirk J. Struik, Lee Lorch, Jean-Pierre Kahane, Kenneth O. May, Israel Halperin, Reuben Hersh, Steve Smale, and on and on. Maybe a breaking of the silence will be welcomed.

* * *

If this is a memoir, a meditation on my own life, it is more broadly a meditation on the whole group of us. We were an appreciable portion of the mathematical community, but not a cohesive subcommunity. I'm talking about people born before 1940, mostly in North America or Western Europe. I'm concentrating on those who at least for some time were part of "the Movement," the organized socialist movement that accepted the Soviet government as part of its leadership: the "Third International," we used to call it.

Is this to concentrate too narrowly? It does seem so from a twenty-first-century perspective, in which pro-Soviet politics are rightly placed within a much larger and more various current of socialist and progressive militance. But don't forget that in many countries in the 1930s, anti-capitalism, anti-racism, feminism, and anti-militarism were regularly attacked as Red causes and felt as such. Both friends and enemies of the Left felt for some years that it had more unity than it had, that there was one noble Cause.. "Advance, proletariat, to conquer the world!" (to quote the Comintern's anthem of the 1930s, by Hanns Eisler). "Into the streets

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May First!" (to quote a song of the 1930s by Aaron Copeland). In the postwar years, the Movement came to feel much less mighty and less monolithic, but we were in the wake of an era of its optimism.

One reason for our silence about this is that our memory is vivid of times when speaking freely about it could lead to firing or worse, and not only in Red-hunt America and fascist countries. We formed a habit of discreet reticence, and often we would have done well to be more discreet than we were. In the present retrospective I will be systematically vague about who was in the CPUSA or equivalent in just which years. This is not merely that residual reticence; and it is not a matter of protecting reputations (the damage anyone could suffer from my outing them as a one-time Red is less now than it was); it is realistic.

First, we really didn't keep track back then of who was in the Party. It was safer that way. What one doesn't know one can't let slip. Of course we knew that some people could be trusted, but really, what need did any of us have to know whether Rep. Vito Marcantonio was in the CP[1] or Pete Seeger?[2] Membership in any organization was not always key, anyway (as Dirk Struik remarked to me, when political parties became illegal and dangerous, people might prefer to found new bird-watching clubs). But in addition, the people I'm harking back to were very diverse, and their experiences in the Left were different. Not simultaneous, either. The way our relations evolved is central to my story, and if every name had dates of joining and leaving the Party attached, the story would seem drier than it is.

* * *

A good many mathematicians were raised in the faith: "red-diaper babies," we sometimes say. Felix Browder and his brothers, of course, but also Rebekka Struik, Allen Shields, myself, Peter Rosenthal, and many others. Whether or not a red-diaper baby grew into a political adult animal, the Movement was inevitably a big component.

More of the Reds came into the Movement as young adults. Concerned with the challenge of making the world a better place, young people shop around. One friend of mine joked that in her high school, it was normal to go from Hashomer Hatzair to the CP to the Trotskyists; she was unusual only in going directly to the Trotskyists without a spell in the CP. People shop around among belief systems in their youth as one may date a succession of partners. I don't mean to imply that this was casual. Changing from one love to another is sometimes done lightly, but often it can't be done without deep anguish, and that is the way with belief systems too. Think of Ralph Ellison's *The Invisible Man*.

Now the generation before mine in the Movement was rather impatient with dropouts. They might mutter that so-and-so had "turned sour." This of a good

[1] He never was.

[2] He was, for a while.

friend who had dropped out of the Communist Party because he couldn't accept the Soviet Union's making a non-aggression pact with Nazi Germany in 1939—a pact the Soviet leadership itself regretted thoroughly less than two years later. Turned sour? Wasn't this a matter rather of a different analysis of events imperfectly known and taking place quite far away? But it was at the same time a matter of breaking solidarity and of rejecting Party discipline. It was clearly not betrayal like divulging Movement secrets to the FBI, and it wasn't felt to be. But it was felt to be disloyalty to shared values: almost apostasy.

The joke had it that an ex-Red can't stand another ex-Red who left the Party earlier or later.

My generation had to cope with a great deal of loss of faith. Any Red who hadn't figured out already that horrible injustice was done to its citizens by the Soviet state, had to face the fact in 1956, when the content of Khrushchëv's "secret speech" became known. No longer was it possible to tell ourselves and each other that the Great Terror in the Soviet Union had been a legitimate punishing of traitors. No longer could we dismiss criticism of lack of civil liberties in the Soviet Union as soft-headed. Our responses differed, dramatically.

I recall a conversation I had at the Nice International Congress in 1970, with three fellow mathematicians, firm friends all. Though this conversation was emblematic to me, I found decades later that all three of them had forgotten it, so I will report it without identifying them. The question was raised what the Left should take away from the disappointments of Soviet socialism. One mathematician said, "The Bolshevik line was always basically correct; serious mistakes were made, but the Soviet Union is still on the correct course." A second mathematician said, "The Soviet Union was on the correct course to socialism until Khrushchëv betrayed the revolution and followed the capitalist road." A third mathematician said, "What we have to learn is that it is very hard to build socialism." Even now, forty-odd years later, it would be hard to sort out the rights and wrongs here, but that is not my subject: I am talking about the way the Left in my generation dealt with such divisions. The three positions I cite are so different that surely any of them could be the preface to an accusation of heresy in a movement trying to maintain ideological conformity; and differences akin to those I cite (but less clear) had in fact, not long before 1970, splintered the seemingly ascendant New Left into hostile factions. Yet the four mathematicians in that Nice conversation maintained mutual respect and (usually) harmony through the political vagaries that have followed. Nobody turned sour or was said to have turned sour. I here raise, and only partially answer, the question of how some sort of unity could be preserved.

Some who had been fervent Communists became anti-Left. They might repudiate their own former beliefs, and they vigorously dissociated themselves from their former comrades. This is easy to understand and is sometimes public and memorable, like Yves Montand rejecting Georges Marchais's appeal for solidarity on French national television with the exclamation "Merde!" I am saying that very often we quietly got along with former comrades despite disagreements. It wasn't always easy, and it was generally tacit. Though I have foregrounded the issue of loyalty to

Soviet leadership, other issues too raised hostilities within the Movement, and they likewise were sometimes overridden in ways hardly seen in earlier days.

I will proceed by a few more anecdotes.

* * *

At the meeting of the American Mathematical Society in St. Louis MO in 1977, there was a little extra session in honor of Lee Lorch, organized by three of his friends: Vivienne Mayes, Diane Laison, and myself. We invited the profession to celebrate Lee's unique contribution to encouraging Black women to enter the profession (Vivienne herself among them); to celebrate also the tough struggle by Lee and Black colleagues beginning in 1951 to end the practice of the AMS and MAA of holding sessions in Whites-only halls. The honor was well deserved, and the session was well attended. Among others we had invited to address it was the redoubtable Lipman Bers.

Bers could sincerely praise Lee's extraordinary service to equality in the world of mathematics, and he did. He also said, "Lee and I have had our disagreements over the years, and thinking back on them I must say that I was always right!" Everyone laughed, including Lee. Now it is traditional in some circles that testimonials include a "roast" in which the praise has some put-downs jocularly mixed in; this was not exactly a roast. Bers was joining wholeheartedly in the encomia to Lorch for exemplary devotion to antiracism and free speech at home, but he was going on record that he was not conceding an inch on fronts where they disagreed.

One of those fronts was public criticism of the Soviet Union. Their disagreement was sometimes public. (At a well-attended business meeting of the AMS, a motion by Bers, seconded by me, had protested the imprisonment of a Soviet mathematician for political dissidence, and Lee had been conspicuous as one of the very few abstainers (nobody voted against). I could give other instances.)Many who heard Bers's quip saw the reference.

At that St. Louis session in his honor, Lee rose to thank us. He said modestly and perceptively, "The only thing that's special about me is that I'm very stubborn." Indeed. Stubborn in uncompromising support for desegregation when the personal costs to himself and family were prohibitive; and stubborn in loyalty to a Soviet system which many of us thought had shown itself a tyranny. At a merry celebration of Lee Lorch's 90th birthday in Toronto, 2005, greetings were read from the leading French Fourier analyst J.P. Kahane, a long-time friend, saying among other things that Lee remained loyal to his Third World connections and loyal to his Soviet contacts "as long as that was possible." Seeing the stubbornness as part of the strength we valued in Lee is a part of why I and others stood by him even when we think he was wrong. Only a part.

More needs to be said. If we who look back fondly to days when it was less stressful to be in the Movement tried to coalesce in a new united front, we would fail. Shared nostalgia wouldn't sustain us. Nor can unity be based only on quid pro quo, the "I'll sign your petition for legalizing marijuana if you'll sign mine for more bike lanes" pattern. What we retain from the Left (the broad Left, the

Frente Amplio) has a coherence beyond what we easily discern. When a devoted champion of indigenous rights sees reason to collaborate with devoted feminists, for example, it is not just quid pro quo. There are deeper commonalities across the divisions—not only the divisions between former allies but also between some to whom alliance will be strange and new. This is a time when reaching out must take precedence over settling scores: now when capitalism, self-immolating, threatens to take everything else with it. There is a world to lose, and to save it will require of us new reconciliations.

* * *

On the Nature of Mathematical Entities

Reuben Hersh

In *What is Mathematics, Really?* I argued, following Leslie White, that mathematical entities are real objects and that they are part of culture, i.e., sociocultural entities and intersubjective.

The particular feature of mathematics, or pure mathematics as it is sometimes called, is the vast body of ESTABLISHED MATHEMATICS. There is a methodology called mathematical proof that is the criterion for acceptance of a claim or a theorem into established mathematics. The acceptance of its proof gives that claim or theorem a high degree of plausibility and licenses it to be quoted in other proofs. The statement is sometimes made that accepted theorems are absolutely certain. That notion is naive. Established mathematics undergoes steady revision and correction. It is a human artifact and, like all human artifacts, can never claim final perfection.

I have argued for these theses on the basis of my own experiences and those of others in the published literature, that is, on the basis of empirical observation, sometimes disrespected as "introspection." I also accepted the common sense view that our thoughts are events in our brains, as nowadays detailed and elaborated by neuroscientists led by Stanislas Dehaene.

The two views of mathematical entities, as neural events and as mental events, are not conflicting or competing, but rather two different views or ways of accessing the same thing.

Now I want to point out that the "existences" of the three sides or views of mathematical entities—social, mental, and neural—are logically inescapable and prior to their empirical verification.

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First of all, there is no question that mathematics has a history, that it is institutionalized, that it is published, and that it is taught. In brief, no one doubts that mathematics as a social institution is a reality.

Likewise, no one doubts that people do think about mathematical entities—numbers, triangles, functions, spaces, etc.—and think about them effectively and productively. We work on these mental entities, manipulate them, transform them, discover their properties, and prove statements about them which are often called "theorems." Consequently, it would be logically incoherent to deny that there ARE such things as mental models of mathematical entities. Their existence is logically inescapable from undenied and undeniable facts about the world.

And the same status must be accorded to the common knowledge that thinking takes place "inside" the human being who is thinking—and mainly in her nervous system, especially the cerebrum.

So there ARE mathematical entities, and they have three principal manifestations: social, mental, and neural. But none of these three can stand alone. The social existence of mathematical entities depends on the participation of individual minds and brains; otherwise, there would be no social or intersubjective mathematical entities.

On the other hand, the existence of mental models in individual minds comes about by participation in social activity. Education, interaction with fellow human beings in school or in collaboration, and reading books and papers, all are part of the social existence of mathematics which makes possible the existence of individual mental models.

And the existence of neural processes corresponding to mathematical entities is the result of the individual's thinking and learning. The neural trace of the exponential function is available or present in the brain not because of genetic or biological reasons but because that brain is the brain of a person who has learned about the exponential function and thinks about it.

The exponential function is a single entity or object, with many properties. It is recorded in books and in computers. These recordings are mathematics, not just meaningless marks, because they are comprehensible and meaningful to people. The primary reality of mathematics is not inorganic artifacts, but human consciousness, manifested as individual thought, as intersubjective concepts, and as neural counterparts or bases in brains. None of these three principal manifestations is possible independently of the other two. By including the neural aspect, one allays a fictionalist or nominalist fear that "mathematics can't be real because only the physical, the observable by physical instruments, is really real." The activities of brains are physical things. The brain traces of mathematical entities are the aspects of mathematical thoughts which we know how to observe physically. The empirical evidence I have offered elsewhere, that we have mental models of mathematical entities, now is recognized as a CONFIRMATION, of what is ALREADY undeniable by logic and indisputable common knowledge.

A philosopher can demand more as a definition or characterization of mathematics, beyond the existence of the body of established mathematics, the practice and knowledge of people who do mathematics, and their brain traces. Such is the demand of the structuralist, the formalist, or the Platonist.

The inclusive, interdisciplinary view that I am presenting here has been developed in recent years under several influences: my own papers on pluralism presented in Rome and in Kolkata, books by the neuroscientists Antonio Damasio and Stanislas Dehaene, and the excellent *Introduction to Phenomenology* by Robert Sokolowski.

From the viewpoint of phenomenology, it is fundamental and commonplace that there are in the world objects that have an identity, a persistent identifiable existence, that is manifested in multiple ways. These multiplicities are not logical contradictions. They are the different ways we know things—any kinds of things—including mathematical things, which are manifested as cultural items, as personal experience, and/or as currents in our flesh and blood.

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