Re: "About a new foundational crisis in mathematics"

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To whom it may concern

The recent developments urge me to address my colleagues from mathematics. All what follows is up for discussion. Drawing from science studies, my observation will be that mathematics has developed a subfield that may be called *big math*, and that its existence demands a rethinking of scientific collaboration.

As we all know, a bit more than hundred years ago, Hermann Weyl gave his diagnosis of a *new foundational crisis* of mathematics, motivated by a pressing concern about the justification of mathematical proof methods.[1] Explicit construction seemed to vouch for certainty, closest to an apparently self-evident intuitive clarity. Such constructions became the basis of automated proof-checking, which today allows automated mass proof production which exceeds the ability of human reading, not to speak of understanding.

But let me briefly explain the concept of 'big math'. In science and technology studies, one describes 'big science' as that part of science that requires large organisations, typically employing complicated machinery and long term projects. A classic example, studied in detail by Peter Galison, is high energy physics.[2] Harry Collins studied gravitational wave detection over his career.[3] Of course, also the mil-

[1] Weyl (1921).
[2] Galison (1997).
[3] Collins (2010).

itary complex is part of big science, as discussed in the famous book *Arguments that Count* by Rebecca Slayton.[4]

According to Collins, the transition from small to large projects lies not so much in the number of cooperating scientists, but in the financial and technical resources, which substantially change the way science is conducted and how its goals are decided.[5] With the introduction of automated proof production and checking, a part of mathematics currently undergoes such a change of aggregate state: large corporations, large groups, and large machinery become necessary to make scientific progress in these fields. I would like to mention two effects of this transition.

Firstly, while it has always been necessary for physicists and applied mathematicians to make up their minds if they share ethical standards with their employer, this issue has now entered into focus for research mathematicians, to a degree that has a potential of turning into a crisis dividing the community.

Also, it foregrounds a second aspect. For this, let's consider for a moment the role of machinery in large scale physics. Here, experiments are conducted at a high level of standardisation, in which existing knowledge is physically implemented in the machinery. The machinery is expected to stay within controlled bounds. Given some broadly accepted research questions, the experiment creates the conditions under which structured empirical data may be collected. Not only does the machinery tend to be large, but also the datasets produced by experiments is vast – they demand a long and arduous process of theorisation.

Now to me, the similarity is striking. Today's 'big math' projects physically implement existing knowledge in the form of trained models. When prompted, they generate a specific experimental situation, measured by the proof-checking instruments which produce structured empirical data, currently in the form of Lean programs. And the production of this experimental data is followed by a long and arduous collective process of refactoring, mathematisation and theory building.

[4] Slayton (2013).
[5] Collins (2010), p. 148f..

So far what seems rather uncontroversial. My less grounded prediction at this point is that the production of experimental proof checking data will grow without inner bounds, beyond the capacity to automatically 'informalise' it. Only external bounds such as energy consumption, finances, or corporate and/or military interests will pose limits. As formalised but opaque data grows, the similarity to physics will become even stronger. If it continues, big math will, just like big science, consist of long term projects of debugging, heuristic conjectures, partial theories, large funding bodies, public advertising, etc.

As a final contribution to the 1920s debate on the foundations of mathematics, Hermann Weyl remarked that either mathematics becomes an empty game of symbols ('formalism'), or an intuitively bounded endeavour ('intuitionism'), or indeed, as a third possibility, an experimental science.[6] With experimental science, he must have meant small science of formal experimentation. Given the fact that historically, mathematicians have often been physicists or closely collaborated with natural sciences, this empirical slant is not so far-fetched. But because in general, the applications and alliances are open, the present case of big math makes it necessary for mathematicians to consciously decide with whom to cooperate. If it is best to abstain from production altogether, as did Alexander Grothendieck, or be employed by the biggest castle in town, if it is best to concentrate on human exchange, build a network of community-owned AI infrastructures, or concentrate on other forms of mathematical experiment, every decision of this kind will change what mathematics is about, and should be informed by what we know about the history of small and big science. From my personal experience of teaching elementary higher mathematics and programming in a music academy I am confident that collective and slow thinking about mathematics can be immensely useful in areas that one would not expect it to be. And it seems that we are not alone.

Kind regards, Julian Rohrhuber[7]

[6] Weyl (1924).

[7] Thanks to Lendl Barcelos for valuable improvements, Renate Wieser, Dhruv Raina, and Dennis Scheiba for discussions on the topic.

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