EARLY CAREER

The Early Career Section offers information and suggestions for graduate students, job seekers, early career academics of all types, and those who mentor them. Angela Gibney serves as the editor of this section with assistance from Early Career Intern Katie Storey. Next month's theme will be preparing for the academic job market. All Early Career articles organized by topic are available at `https://www.angelagibney.org/the-ec-by-topic`.

Many of us are thinking of the challenges ahead for early career mathematicians in Ukraine and Russia. We would like to advertise a list of resources, collected by Terry Tao, found at `https://terrytao.wordpress.com/2022/03/02/resources-for-displaced-mathematicians`, as well as this list on the AMS website: `https://www.ams.org/displaced/ukraine-resources`.

Research

How to Make a Portrait of a Bird

Claire Voisin

Si l'oiseau ne chante pas
c'est mauvais signe
signe que le tableau est mauvais
mais s'il chante c'est bon signe signe
que vous pouvez signer

—J. Prévert

When Angela Gibney contacted me about the "Early Career," that she described as "a column in the *AMS Notices*, with advice, written by mathematicians, for graduate students and new PhDs," I first thought that, being close to 60, perhaps the time had come to allow myself to offer some advice to young people. But then the question arose: advice about what? How to be a mathematician? Ridiculous and pretentious! How to make a theorem? Meaningless, absurd and counterproductive. After six months thinking about it, I remembered a charming poem by Jacques Prévert "Pour faire le portrait d'un oiseau" (which I believe is about art) and decided that its title is exactly adapted to the situation. I leave developing the metaphor to the reader, with a starting point that the bird is the theorem.

The reason why I find it very hard to speak about mathematics is that I cannot disconnect my activity as a mathematician from my whole life and personality. I do believe there is something narcissistic in the mathematical practice, because the question is not only: "what is true?" but also "what am I able to prove?" which is a question about myself. If I push this idea a little, I find that, when I am doing mathematics, as for many other human activities, my ultimate goal is to know more about myself, as advised by Socrates.

More concretely, I find that mathematics and life are intertwined because it takes so many steps and involves so many different aspects of our personality before we can claim a new theorem! There is the monastic, introverted period, where we are just contemplating the ocean of our

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ignorance; but then suddenly something happens, a statement, a strategy towards proving a statement, a direction toward which we can throw all of our energy, and then, there is a metamorphosis, the monk becomes busy and excited, in a hurry to look more closely at the details. Sometimes, it was simply nothing, an illusion or worse, a mistake (my computer is full of dead bodies of theorems, but I believe these mistakes are allowed: "on ne fait pas d'omelette sans casser des oeufs"). If the theorem survives the first examination, the next period is the most painful, namely we have to go through all the details, and here, the danger of a mistake is much greater. This time we are now like a mechanic, and although we sometimes need new ideas to finish, these are usually technical ideas of secondary interest. Needless to say, the third period is the most humbling, but, at least, we know who we are and where we are going. One may argue that a great theorem cannot actually depend on obscure and technical details, but this is only true a posteriori, once we are convinced that the result is true. A priori, we need to check tiny details, and sometimes use specific arguments where intuition is of small help, in order to make the proof of a big result work. Who remembers that Serre's GAGA paper[1] has a long appendix where 4 pages are devoted to the proof that the local ring of holomorphic functions is flat over the local ring of algebraic functions?

I find it hard to do mathematical research full time. Hard because new ideas are so rare that we are faced most of the time with our own mediocrity. I also suspect that mathematical ideas like to stay hidden, or to play hide and seek, and so staying at one's desk staring fixedly at the blank sheet is apparently useless. Yet we mathematicians must continue and not stop thinking, otherwise nothing will happen. This is probably why we tend to write so many secondary papers: to stay alive and awake, and to be ready for the few moments where something great happens. (I mention here that the tendency to publish many papers is not a modern plague: Euler's complete writings form 70 volumes, Cauchy's 27 volumes, and Poincaré's 11 volumes.) Many people, including Poincaré, mentioned the importance of the unconscious path in the emergence of new mathematical ideas. I like to think that something like the free-floating attention of the analyst helps us capture deep mathematics, but this can work only if we combine it with concentration, full devotion of our mind to mathematics, and routine work.

It is not clear how to organize one's research, having accepted that one does not have much control over it. Unless I am in the "third period," I usually have a number of problems in mind, some very appealing (but they are often the hardest), others more realistic but also more laborious, which I am happy to abandon if suddenly I get an idea on a different subject. Conjectures are very helpful to organize our work. They provide a sort of network in the mathematical culture that is very beneficial to mathematical communication. There are some tricky conjectures by Hartshorne, Bogomolov, Swinnerton-Dyer, and others that I try from time to time to prove or disprove when I have no other plans. They may be true or not, there is no strong evidence either way, nor do they have strong consequences. They are challenging because we do not see how to attack them, and the method would be presumably more interesting than the result. What about the "big conjectures?" Algebraic geometry has the Hodge conjecture, the generalized Hodge conjecture made by Grothendieck, and the Bloch-Beilinson conjectures on algebraic cycles. These conjectures tend to shape our mind, hence we may lack skepticism and be indoctrinated by the principles underlying the conjectures. For those big conjectures mentioned above, the general principle would be that the Hodge structures associated with an algebraic variety give such rich information that they encapsulate all its motivic data. Having grown up with this principle in mind makes it hard to think that, after all, it would not be such a drama if the Hodge conjecture were false. Personally, I am more and more skeptical about the generalized Hodge conjecture, which has an incredible predictive power on the existence of families of interesting subvarieties of algebraic varieties. I like it better than the Hodge conjecture because there are many concrete families of examples (like low degree hypersurfaces in projective space) where it predicts interesting phenomena; most of them are unsolved. Unfortunately, this conjecture seems even harder to disprove than to prove, as general results in Hodge theory tend to support it, and Hodge structures fit perfectly well with the general formalism of correspondences and motives.

To conclude, let me come back to the birds, that is, the theorems. This reminds me of a talk I gave in a high school, about doing mathematics. At the end of the talk, one student asked me whether a new result in mathematics should be called a mathematical discovery or a mathematical invention, a very clever question to which I arranged not to respond.[2] Whatever name should be used, I do not have a uniform experience of proving theorems. Some of my best results, like the homotopy type of compact Kähler manifolds or the specialization of the decomposition of the diagonal, did not cost me much effort, while some research that I pursued for months led me to nothing. My papers about syzygies were in between, with many efforts, but also new ideas. Unfortunately, as Prévert tells us, the bird does not always sing. I do not like some of my papers which were uninspired, but, in some cases, writing a mediocre paper pushed me to change the direction of my research. My most recent and great experience was a collaborative

[1] Géométrie algébrique et géométrie analytique, *Annales de l'Institut Fourier* ***6*** *(1956), 1–42.*

[2] *I replied by mentioning the book* L'Invention de la Côte d'Azur *by Marc Boyer, which asks similarly whether the Côte d'Azur has been discovered or invented by the English.*

research within an ERC contract, and I have been surprised by the way it has worked. In fact, our collaboration looked like the surrealistic game "Cadavres exquis."[3] Each person added a piece to the game and the final result was somewhat surprising: a simple topological characterization of hyper-Kähler fourfolds of $\mathrm{Hilb}^2(K3)$-type.

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Claire Voisin

Credits

Photo of Claire Voisin is courtesy of the author.

What to Do When You Are Stuck or Confused on a Research Project

Michael Hutchings

Stuck

Being stuck is bad. Time is passing by and it feels like you are not making any progress on your project. What to do about this depends on the reasons why you are stuck. Here are some possibilities to consider.

Break Problems into Simpler Pieces

When possible, work out examples, starting with the simplest nontrivial case you can think of. Just as a picture is worth a thousand words, a good example is worth a thousand theorems!

Use the examples to map out conjectural lemmas that would help, and try to prove them.

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[3]*The name "cadavres exquis" was invented around 1925 by Jacques Prévert, who in his early life participated actively in the Surrealist movement.*

Ask (Concisely) for Help

It can be good to talk to another mathematician, and a great time to do so is when you work in area X and you need to use methods or results from area Y with which you are not so familiar. Instead of struggling in area Y alone, it can be much more efficient to ask an expert in Y for at least a little help.

Try asking your initial question as concisely as possible without much preamble. A very long email may end up in the "I will deal with this later" pile and never get answered, and similarly, a long in-person discussion may leave your conversation partner lost and confused before you get to the point of your question.

An expert may give you a few tips to point you in the right direction; in this case don't forget to thank them in the acknowledgments in your paper. If you find that substantial work needs to be done in area Y, this may lead to a productive collaboration. It sounds too obvious to be worth saying, but you can accomplish so much more if you work with people who can do things that you can't!

Take a Break

You may be able to figure things out subconsciously while doing something else. In addition to healthy non-mathematical activities, this could mean working on different projects in parallel, following your curiosity to read the literature, or going to seminars on new topics.

Don't Be Afraid to Finish Your Paper

There are a lot of reasons for not getting your work out the door.

Some people seem very eager to explore several new projects before finishing up the current paper, perhaps feeling worried about what to do afterwards. I believe that such fears are generally unfounded; each theorem proved raises many new questions.

One has to find a balance: It can become problematic if you are working on so many things that you never finish any of them.

The feeling that you can never find time for your project could be a warning sign that it is not a good fit for you, at least for now (see below).

There may be some annoying technical issues that need to be dealt with before the paper is complete. I try to remember that there is no partial credit for papers that never appear. It can help to make a list of the remaining items to take care of, work through them one at a time, and celebrate when you are done.

Avoid perfectionism in writing; do your best in the time you have. If your paper doesn't accomplish everything you had hoped for, you can always write another paper later. If your manuscript is getting unmanageably long and complicated, see if you can split off a portion which is at least 1 LPU (Least Publishable Unit) and finish that first.