I will talk about an issue that I think is important but mostly avoided for seemingly vague and confusing reasons. Our computers do a huge number of absolutely wonderful things. Yet most of these things seem rather mechanical. Lots of crucial problems do yield to the intuition of our very slow brains are beyond our current computer arts.

Great many of these tasks can be stated in the form of inverting easily computable functions, or reduced to this form. (That is, finding inputs/actions that could produce a given result in a given realistic process.)

We have no idea about intrinsic difficulty of these tasks. And yet, traveling salesmen do get to their destinations, mathematicians do find proofs of their theorems, and physicists do find patterns in transformations of their bosons and fermions! How is this done, and how could computers emulate their success?

Of course, these are collective achievements of many minds engrossed in a huge number of papers. But today’s computers can easily search through all math and physics papers ever written. The limitation is not in physical capacity.

And brains of insects solve problems of such complexity and with such efficiency, as we cannot dream of. Yet, few of us would be flattered by comparison to the brain of an insect. What advantage do we humans have?

One is the ability to solve new problems, those on which evolution did not train zillions of our ancestors. We must have some pretty universal methods, not dependent on the specifics of focused problems.

Of course, it is hard to tell how, say, mathematicians find their proofs. Yet, the diversity and dynamism of math achievements suggest that some pretty universal mechanisms must be at work.

Let me get now more technical, and focus on a specific problem: Consider, for instance, algorithms that 3-color of graphs. Is it true that every such algorithm can be sped-up 10 times on some infinite set of graphs?

Or, there is a “perfect” algorithm, that cannot be sped-up 10 times even on a subset of graphs?

Note that there is a 3-coloring algorithm that cannot be sped-up by more than a constant factor on any subset. The question is, must this constant get really big?

But before further discussion, let me go into some history.

In the 50s, in the Russian math community there was much interest in the works of Claude Shannon. But many of Shannon’s constructions required exhaustive search of all configurations. There was an intense interest in whether these exponential procedures could be eliminated.

And Sergey Yablonsky wrote a paper that he interpreted as showing that no subexponential method could work on a problem that is, in today’s terms, co-NP. It is a problem of finding a boolean function of maximal circuit complexity.

Kolmogorov saw this claim as baseless since the proof considered only a specific type of algorithms. He was unhappy with such a misleading idea being promoted. Kolmogorov advocated the need for efforts to find valid proofs that some commonly believed complexities of popular problems are, in fact, unavoidable.

For that he needed a convincing definition of the running time. But Turing Machines were seen as too limited in speed to use for meaningful lower bounds. Kolmogorov formulated a graph-based model of algorithms that had time complexities as we understand them today. He also organized a seminar where he challenged mathematicians with quadratic complexity of multiplication. And an unexpected answer was soon found by Anatoly Karatsuba, and improved by Andrei Toom: multiplication complexity turned out to be nearly linear. (It is now really fast with subsequent improvements by Cook and others!)

This was an impressive indication that common sense is an unreliable guide for hardness of computational problems, and must be verified by valid proofs.

I, at that time, was extremely excited by some other work of Kolmogorov. He (and independently Ray Solomonoff) have used the Turing’s Universal Algorithm to give an optimal definition of informational complexity, randomness, and some other related concepts.

I noted that similar constructions allow defining an optimal up to a constant factor algorithm for a problem now called Tiling, and therefore for any search problem, as they all have a straightforward reduction to Tiling.

To my shagreen, Kolmogorov was not impressed with the concept of optimality, saw it as too abstract for the issue at hand. But he was much more interested in my remark that Tiling allows reduction to it of all other search problems. He thought I should publish that rather than the optimal search.

I thought it would only be worth publishing if I can reduce it to some popular problems. My obstacle was that combinatorics was not popular in Russia, and my choice of problems that might impress the math community was rather limited. I saw no hope for something like factoring, but spent years in naive attempts on things like graph isomorphism, finding small circuits for boolean tables, etc.

And another interesting angle was added to these issues. In 1969 Michael Dekhtiar, a student of Boris Trakhtenbrot in Novosibirsk, published a proof that under some oracles inverting simple functions has exponential complexity.

In the US, Baker, Gill, and Solovay did this independently. Later I ran into problems with communist authorities. And friends advised me to quickly publish all I have while the access to publishing is not yet closed to me. So I submitted several papers in that 1972, including the one about search (where Kolmogorov agreed to let me include the optimal search).

I guess I must thank the communists for this publication.

But the greatest developments by far were going on in the United States. Cook, Karp, and David Johnson made a really revolutionary discovery. They found that 3-SAT reduces to great many important combinatorics problems. Combinatorics received much attention in the West and these results became a coup!
Kolmogorov asked several questions at that time, still open and interesting. One was:

Are there polynomial time algorithms that have no linear size circuits? We knew that some slow polynomial time algorithms cannot be replaced by faster algorithms. But can linear-sized circuits families replace all of them?

His other interesting comment was a bit more involved. We proved at that time that mutual information between strings is roughly symmetric. The proof involved exponential search for short programs transforming a strings x into y. Kolmogorov wondered if such search for short fast (meant to be robust, tolerating +O(1) slacks in length and log time) programs would not be a better candidate than my Tiling to see if search problems are exponentially hard.

He said that, often, a good candidate to consider is one that is neither too general, nor too narrow. Tiling, being universal, may be too general, lacking focus. Some other problems (say, factoring) – too narrow. And search for fast short programs looked like a good middle bet to him. It still does to me! :-)

Such search is involved in another type of problems that challenge our creativity: extrapolating the observed data to their whole natural domains. It is called by many names, “Inductive Inference”, “passive learning”, and others.

Occam Razor is a famous principle of extrapolation. A version attributed to Einstein suggests: conjectures should be chosen as simple as possible, but no simpler :-).

Ray Solomonoff gave it a more formal expression. He said that the likelihoods of various extrapolations, consistent with known data, decrease exponentially with the length of their shortest descriptions. Those short programs run about as fast as the process that had generated the data.

There have been several technical issues that required further attention. I will stay on a simple side, not going into those details. Most of them have been clarified by now, if we ignore the time needed to find such short fast programs. This may be hard. Yet, this is still an inversion task, bringing us back to the issues of optimal search.

I have a little discussion of such issues in a short paper: Universal Heuristics: How Do Humans Solve Unsolvable Problems? (You see a reference and a link on the slide.)

Now, back to my focus. The concept of optimal algorithm for search problems ignores constant factors completely. So, it is tempting to assume that they must be enormous.

However, this does not seem so to me. Our brains have evolved on jumping in trees, not on writing math articles. And yet, we prove Fermat’s Theorems, design nukes, and even write STOC papers. We must have some quite efficient and quite universal guessing algorithms built-in.

So, I repeat a formal question about these constants:

Can every algorithms for complete search problems be sped-up 10 times on an infinite subset?

Of course, careless definitions of time can allow fake speed-ups. For instance if we ignore the size of alphabet and reduce the number of steps just by making each step larger due to the larger alphabet. Or if we exclude the required end testing of the input/output relation, and choose a relation that itself allows a non-constant speed-up. But it is easy to carefully define time to preclude such cheating.

Let me now go into some little technicalities to see what issues are involved in understanding these constant factors. We look at the optimal search for an inverse w of a fast algorithm f given the output x it must produce on w.

We refine Kolmogorov Complexity with time, making it computable. The time-refined complexity $K_t$ of w given x considers all prefixless programs p by which the universal algorithm U generates w from x in time T. That time also includes running f on w to check that the result is x. $K_t$ is the minimum of the length of p, plus log of this time T.

The Optimal Inverter searches for solutions w in increasing order of this complexity $K_t$ of w given x, not of length of w. For instance, shorter proofs may be much harder to find, having higher complexities. The Inverter generates and checks in time $2^k$ all w up to complexity k.

Btw, the optimal search makes the concept of complexity applicable to individual instances of search tasks, not just to families of instances which we now call “problems” and complexities of which we study. So we can ask how hard is, say, to find a short proof for Fermat’s theorem, not for theorems in general. Would not this notion fit tighter?

The big catch here is that each wasteful bit U requires of p doubles the time. We would need a very “pure” U, frugal with wasting bits. Do our brains have such a one built-in? It seems so to me. We do seem to have little disagreement on what is “neat” and what is cumbersome. There are differences in our tastes, but they are not so huge that we could not understand each other’s aesthetics. But this is just a feeling. The formal question remains:

Is there an algorithm for a complete search problem that cannot be sped-up ten times, even on an infinite subset?

(Of course, this 10 is a bit arbitrary, can be replaced with your favorite reasonable constant.)

I have no time for another issue, related to search problems, that interests me: Climbing algorithms.

Search problems have easy correctness tests for solutions. Climbing algorithms allow also an easy assessment of how close to yielding the correct answer is the configuration at any stage of their run. This offers much flexibility, as it can be instantly assessed how sensible is any deviation from the standard procedures.

An example is the Dual Matrix Algorithm for linear programming. It has little sensitivity to numerical errors and to the number of inequalities. It offers substantial flexibility and, thus, potential for further developments.

I discuss these issues in an article “Climbing Algorithms” in the Proceedings of this STOC.