# Reminiscences*

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In his early memoirs, Trakhtenbrot told several stories. The greater part is an intellectual history of Soviet research in theoretical computer science from the 1950's to the late 70's. A second story – of academic and political disputes that shaped the course of Soviet research in the area – is briefly indicated. Finally, there is a laconic suggestion of the scientific life of a gifted, prolific mathematician and scholar.

Of course the emphasis in each story is on the Soviet side, but the international context in which research in theoretical computer science has been conducted for several decades is very apparent. The parallel between Soviet and American research is especially visible to me personally because, long before I had the pleasure of meeting and later collaborating with Trakhtenbrot, I was first delighted and then warmly thrilled to watch through the medium of research papers and notes the thinking of a scholarly soul mate. Repeatedly and independently, Trakhtenbrot's and my choices of scientific sub-areas, even particular problems, and in one instance even the solution to a problem, were the same. The similarity of our tastes and techniques was so striking that it seemed at times there was a clairvoyant connection between us.

This personal story offers an alternative, more intimate perspective on the nature of Soviet/Western research interaction in the area of theoretical computer science, as well as some additional biographical information about the author of these memoirs.

Trakhtenbrot probably became most widely known in America because of his tutorial monograph on *Algorithms and Automatic Computing Machines*. I did not become aware of Trakhtenbrot for another half a dozen years, and actually realized only much later that I had studied this book as a graduate student in 1963 when it first became available in translation. I remembered it well as an exceptionally clear and elegant introduction to the basic ideas of computability theory. It was another decade before I learned first hand from the author something of the circumstances under which it was written: the new Ph.D. Trakhtenbrot, who arrived in 1950 in the University at Penza, was certainly not of proletarian background – a Jew who spoke eight languages, whose research was decidedly abstract and "pure", and who, if his present manner may accurately be extrapolated back over fifty years, must have seemed to the casual observer an easy fit to the stereotype of an absent-minded professor. Whispered accusations of bourgeois idealism were heard: in that era of Stalinist paranoia they were gravely threatening. The book was written to demonstrate that this apparently unworldly scholar could produce an object at least of pedagogical value

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to the Socialist state. It succeeded admirably not only in increasing its author's professional visibility, but possibly in keeping him out of prison.

Trakhtenbrot is also fortunate to have found a basic theorem of mathematical logic which is now named after him. Trakhtenbrot's Theorem is a finitary variant of the undecidablity of first-order logic: the problem of whether a first-order formula is valid in all finite models is, like the general validity problem, undecidable, but in a technically different way (co-r.e. as opposed to r.e.). Although this result is accepted as a core result of classical logic, it already reveals Trakhtenbrot's concerns with constructive processes on finite structures. Not bourgeois idealism at all, really.

It was through my own exposure to the pioneering research on computational complexity theory by Hartmanis & Stearns at GE research in Schenectedy and Manuel Blum at MIT in the late 60's that I learned about Trakhtenbrot. There were only a few published papers and no books documenting this exciting new area. Trakhtenbrot had written a set of lecture notes for a course on complexity theory he gave in Novosibirsk and had sent a copy of these notes to Blum (by then at Berkeley). The notes contained a valuable exposition of the results of Blum and Hartmanis-Stearns – based on their published papers in American journals – as well as new results by Trakhtenbrot: his "gap" theorem and his automaton-theoretic analysis using "crossing sequences" of the complexity of transferring information on a linear storage tape. The concern with problems of *perebor* which led Trakhtenbrot and his group to these interests are outlined in his memoir and differed slightly in emphasis from the motivating concerns of the American researchers, but within a couple of years after publication of the basic results in the West, the approaches of Trakhtenbrot's and the American groups had virtually converged. Indeed, a principal result of Borodin's Ph.D under Hartmanis at Cornell in 1969 was his independent version of Trakhtenbrot's "gap theorem". Likewise, the method of crossing sequences was developed independently by the Israeli Rabin (then at Harvard), Hartmanis at GE and Hennie at MIT. The roots of the crossing sequence technique lie directly in the classic papers of 1959 by Rabin and Scott and by Shepherdson on finite automata; these papers were well known to the world research community, so there is no mystery at the independent duplication of the results. Still, I noted at the time that Trakhtenbrot (and Hennie's) development of the "crossing sequence" technique went an extra elegant step beyond Hartmanis'. This was my first hint of the flair and penetrating quality of Trakhtenbrot's style.

Trakhtenbrot's "gap theorem" showed similar quality. Its technical details are immaterial here, but its general nature reveals Trakhtenbrot's refined mathematical aesthetics. There is an easily satisfied side-condition which was needed in the proofs by Hartmanis-Stearns and Blum of their fundamental results establishing the existence of inherently complex problems. I remember myself as a student pointing out after an early lecture by Hartmanis that he had neglected to mention the need for the side-condition in his presentation, and he acknowledged that it was technically necessary but didn't seem worth highlighting. The scientific significance of the results was not impaired by the side-condition. But mathematically, it is intellectually wasteful if not exasperating to use a hypothesis that is not strictly necessary. The "gap theorem" demonstrated that the side-condition was absolutely necessary, and dramatically illustrated a set of pathological time-bounding functions which needed to be avoided in developing complexity theory.

In fact, the gap theorem was the principal stimulus for joint research carried out by my first student, Ed McCreight, and myself. Our "Honesty Theorem" showed how the pathological "dishonest" time-bounding functions of the "gap theorem" could always be replaced by "honest" functions satisfying the side-condition. This method of proof involved one of the most elaborate applications up to that time of the priority method of recursion complexity theory.

It's worth interjecting here an early instance of the astonishing parallels between Soviet and Western research in logic and the theory of computation. The fundamental problem of Post about whether all undecidable axiomatic systems were of the same degree of undecidability was solved in 1956, a dozen years after its formulation. This was achieved, independently, by Mucnik in the Soviet Union and Freidberg, a Harvard undergraduate, within a few months of each other. Their solutions were very similar and involved the invention of the priority method of computability theory.

At the time of this work with McCreight, we had not seen any Soviet writings. Perhaps Hartmanis, a Latvian emigré, was a Russian reader who saw Trakhtenbrot's notes, or perhaps Blum's Romanian emigré student Filloti, who was fluent in Russian, read them. In any case, we learned of the results of Trakhtenbrot's notes by word-of-mouth from Borodin, Hartmanis, and Blum.

Blum passed on a copy of the Trakhtenbrot notes to me around 1970, when I was at MIT, since I knew of a graduate student who was interested in translating them. His work was not very satisfactory, but then Filloti came to MIT to work as a post-doc with me, and did a respectable job. By this time the notes began to seem dated to me (about five years old in 1972) and I decided that they needed to be revised and updated. This youthful misjudgment doomed the project, since I was too impatient and too much of a perfectionist to complete the revision myself, and the final editing of the translation was never completed.

Some very personal issues arise at this point in my story which are nevertheless appropriate to Trakhtenbrot's account. Other neurotic attitudes of mine also undermined the project of publishing Trakhtenbrot's notes. I was at the time enmeshed in a pained marriage with two small children – a marriage which ended in divorce a few years later – and the depression I suffered during those years, which I carefully concealed from my professional colleagues, carried over into doubts about my own research and the significance of the field of complexity theory in general. These doubts, and perhaps other invidious feelings about the productivity of other researchers who seemed more prolific and less depressed than myself, left me inhibited at the final stages of several publishing projects, of which the Trakhtenbrot notes were, I regret to report, just one instance.

As the reader will learn, two of the principal characters on the Soviet side, the prodigies Tseitin and Levin, suffered in one way or another severe inhibitions about communicating their results, and Trakhtenbrot indicates that Tseitin suffered doubts similar to mine about the value of his own outstanding contributions. Whether this is coincidental or a typical reflection of the personality factors that lead young mathematicians into their vocation I remain unsure, but the psychological parallels here are as striking as the intellectual ones.

It amazes me in retrospect how I was actually able to make good scientific use of my doubts to suggest new questions to test the adequacy of our theories. One aspect of complexity theory which had dissatisfied me from the beginning was that in the theory "complexity" was essentially synonymous with "time-consuming to calculate". This definition was inconsistent with certain intuitive ideas about complexity; in particular, it seemed that there ought to be computational problems that were time-consuming, but were nevertheless intuitively not complex in that they merely involved a very simple computation for their solution, though the simple computational procedure might need a very large number of repetitions to produce its result. Thus, there ought to be distinctions among equally time-consuming computational problems reflecting the intuitive idea that two problems might take the same long length of time to solve but for different reasons.

There is no obvious formulation of what a "reason" for complexity in a problem might be, but the famous solution to Post's problem by Friedberg/Mucnik mentioned above offered what seemed to me a straightforward formulation of a precise conjecture which captured the idea. If one could construct two equally time-consuming problems with the property that, even given the ability to compute solutions to instances of one problem in no time at all, the other problem remained as time-consuming to solve as without this instantaneous ability, and vice-versa given the other problem, then it seemed legitimate to say the problems were difficult for different reasons. Once the conjecture was formulated, the construction of such sets became a matter of making suitable modifications of the "classical" priority methods of recursion theory. Working with my expert colleague Fischer, it took only a few weeks to polish the details, leaving one purely æsthetic flaw which seemed amateurish: the construction of the two sets involved defining two of each kind of bell and whistle needed in the construction, one for each set. This seemed wasteful and clogged up the reasoning with subscripts $i = 1, 2$.

Complexity theory was still a new, small field in 1972, and I was hungry for the excitement and reassurance that hearing new results in my area provided. The American Mathematical Society at that time offered a computer search service of journal abstracts to which one could subscribe and specify a rather sophisticated protocol to generate titles, abstracts, and even reprints of articles depending on the degree of match with the subscribers interest profile, and I was an enthusiastic subscriber. The service brought me many of the abstracts published in the *Doklady* by the characters in Trakhtenbrot's memoir.

One day, an abstract from Trakhtenbrot himself turned up of which I needed to read only the title, "On auto-reducibility". Here was the simple repair for the æsthetic flaw: don't construct two sets, neither of which helps the other be computed more quickly; construct one set such that answering membership questions about one part of the set does not help in computing solutions to membership questions about any other parts – a set that is not auto-reducible. Trakhtenbrot had thus come to look for similar results to ours, had obtained them with similar methods, and had added the final elegant touch we had missed. This was the event which confirmed my growing admiration for his ability. It also again reassured me that these abstract, speculative, positively obscure preoccupations of mine were not utterly narcissistic – or at least not *uniquely* narcissistic, because they were shared by an older, experienced researcher whose abilities had been certified by an altogether different establishment. I loved Trakhtenbrot for that reassurance. And given my secret depression and self-doubts, it helped to have a beloved mentor whom I hadn't met and who worked half a world away – little risk of rejection or disappointment that way.

Trakhtenbrot has emphasized that Westerners were too frequently unaware of the contributions of their Soviet counterparts, but as the story above suggests, this was not my own experience at the time. The failure of the AMS abstracting service, which I found so valuable, was an event that surprised and disappointed me at the time, but which helps explain Trakhtenbrot's impression of what he worried was a parochial neglect of Eastern research by Western researchers in theoretical computer science. The service, I was told, failed for lack of subscribers; I was one of the few who actually made use of it. Today, as a senior scientist in the now much larger and better known area of theoretical computer science, the failure of the AMS service seems much more understandable: there are far too many interesting results being discovered and far too many ingenious and significant papers to keep up with. The problem is not to find papers to read but to avoid being overwhelmed by them. I would not be a subscriber today.

The story of the parallels between Trakhtenbrot's and my research areas has too many more chapters to spell out much further. Suffice it to say that we found ourselves happily and fruitfully collaborating firsthand in an entirely different area of theoretical computer science than complexity theory to which we were led by independent decisions reflecting our shared theoretical tastes.

But there is one more personal epiphany which adds some perspective on Trakhtenbrot's story of the academic disputes between the Moscow establishment personified by Yablonskii versus Kolmogorov and Trakhtenbrot's own Novosibirsk group. The doubts that plagued me about the significance of complexity theory – with an emotional force which undoubtedly sprang from intimate aspects of my personal life – also were reinforced by external criticism from logicians and computer scientists alike. The problem was that, however provocative the theorems of complexity sounded, in the final analysis all the early results rested on the same kind of "diagonalization with priorities" which formed the core of classical computability theory. But unlike the classical theory for which natural instances of the kind of undecidability phenomena analyzed in the theory were well-known elsewhere in Logic and Algebra, no instances of provable complexity phenomena were known. For example, I remember presenting my results on sets that were complex-for-different-reasons at a 1971 logicians' meeting.

During the question period after my talk I received one cool question from an eminent logician: “Do your results give any information about the complexity of deciding propositional tautologies?” The lay reader should read this question as “What does what you’re doing have to do with the price of eggs?” I had nothing to say about eggs.

These criticisms were echoed by those of Yablonskii, who scoffed at the emptiness of the diagonal technique. But let there be no misinterpretation of Trakhtenbrot’s softly toned account of the disputes with Yablonskii. The man was clever enough to fasten on a weak point of complexity theory at that stage, but his actions in attacking the proponents of the theory to the extent of destroying careers and denying students their degrees cannot be accounted for out of sincere intellectual doubt; this was a villainous careerism which the Soviet system seems to have fostered. My feelings were hurt that the mainstream community of logicians and computability theorists were initially cool to my interests, but my career was never in jeopardy, and if I could not find support – moral or financial – among them, there were other communities of engineers and computer scientists with positions and grants. With Yablonskii in centralized charge in Moscow of higher degree granting, promotions, and even scheduling of research meetings, working in an area he opposed proved to be a perilous professional choice for my Soviet counterparts.

Shortly after my admission of ignorance about the economic principles of egg pricing, an American, Cook, at Toronto (and independently, though without comparable recognition, Levin in Moscow) discovered them. The precise computational complexity of deciding propositional tautologies remains open, but it is now understood to be the central problem of theoretical computer science, known, through the development of a rich theory, to be equivalent in complexity to hundreds of other apparently unrelated problems.

The excitement of Cook’s discovery and its elaboration a few months later by Karp reawakened my interest in research at a time when I was actually taking some first steps towards leaving a scientific career altogether. In 1972, jointly with a very talented student, named Stockmeyer, I found the first genuinely natural examples of inherently complex computable problems. I knew there were only a small handful of people who understood the field deeply enough to appreciate immediately the significance of our examples, so it was with pride and anticipation that I sent the earliest draft of our results to Trakhtenbrot in Novosibirsk, where they were indeed received with immediate celebration. Sending these results first to Trakhtenbrot was doubly appropriate, because, though I did not know it at the time, Trakhtenbrot was one of the seminal researchers in the area of automata theory and logic from which Stockmeyer’s and my first example came.

We come now to the question of to what degree these individual anecdotes represent a pattern of East/West scientific collaboration in the theoretical computer science. Trakhtenbrot – as noted above – was concerned that Western researchers were too unaware of Eastern research and may therefore have mistakenly underestimated their ability and the potential that would exist, were the stifling climate under Yablonskii's stewardship to abate, for dramatic contributions to be made. But while I agree that the names and contributions of several Soviet scholars, perhaps especially Barzdin, were not as prominent in the West as they may have deserved, nevertheless when I review the major scientific discoveries in the area in the 70s and 80s, I find major Soviet contributions widely and quickly recognized in the West. Thus, the ingenious tree manipulation algorithm of Adelson-Velsky was widely taught in undergraduate computer science courses throughout the world, the theoretically efficient algorithm for linear programming of V'jugin, Nemirovsky and Khachian was the focus of more than one entire scientific colloquium in the West after it was noticed (after, I should add, an uncertain delay) in the Soviet literature in 1979, and a major advance in the study of combinational complexity by a student of Kolmogorov named Razborov captured the imagination of the American research community.

So, I do not find great underestimation or neglect of Soviet activity, though the collegial connections with the West are rarely very close, as one might expect given the obstacles to travel and communication imposed. So, the names of Eastern researchers are not especially audible in informal conversation in Western scientific circles.

As Trakhtenbrot once said, "There can be no question but that in terms of sheer magnitude of pioneering efforts, the work of Western computer scientists exceeds that of their Soviet colleagues." The history of parallels confirms the abilities and contributions of the Soviet research community, but, on the other hand, can be read as showing that the West did not particularly need the Eastern contributions, since they would undoubtedly have been forthcoming anyway from corresponding Western work.

Nevertheless, Trakhtenbrot has called attention to potentially outstanding results such as those of Barzdins and Tseitin, which may have been unduly neglected in the West. It is in the last analysis impressive – and a testament to the vitality of theoretical ideas – that valuable contributions were continually made by Soviet researchers within an academic bureaucracy that would have overwhelmed a Western researcher.