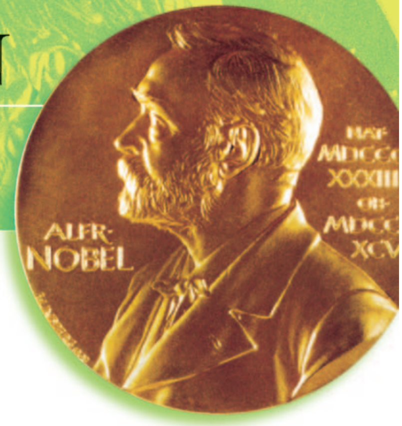
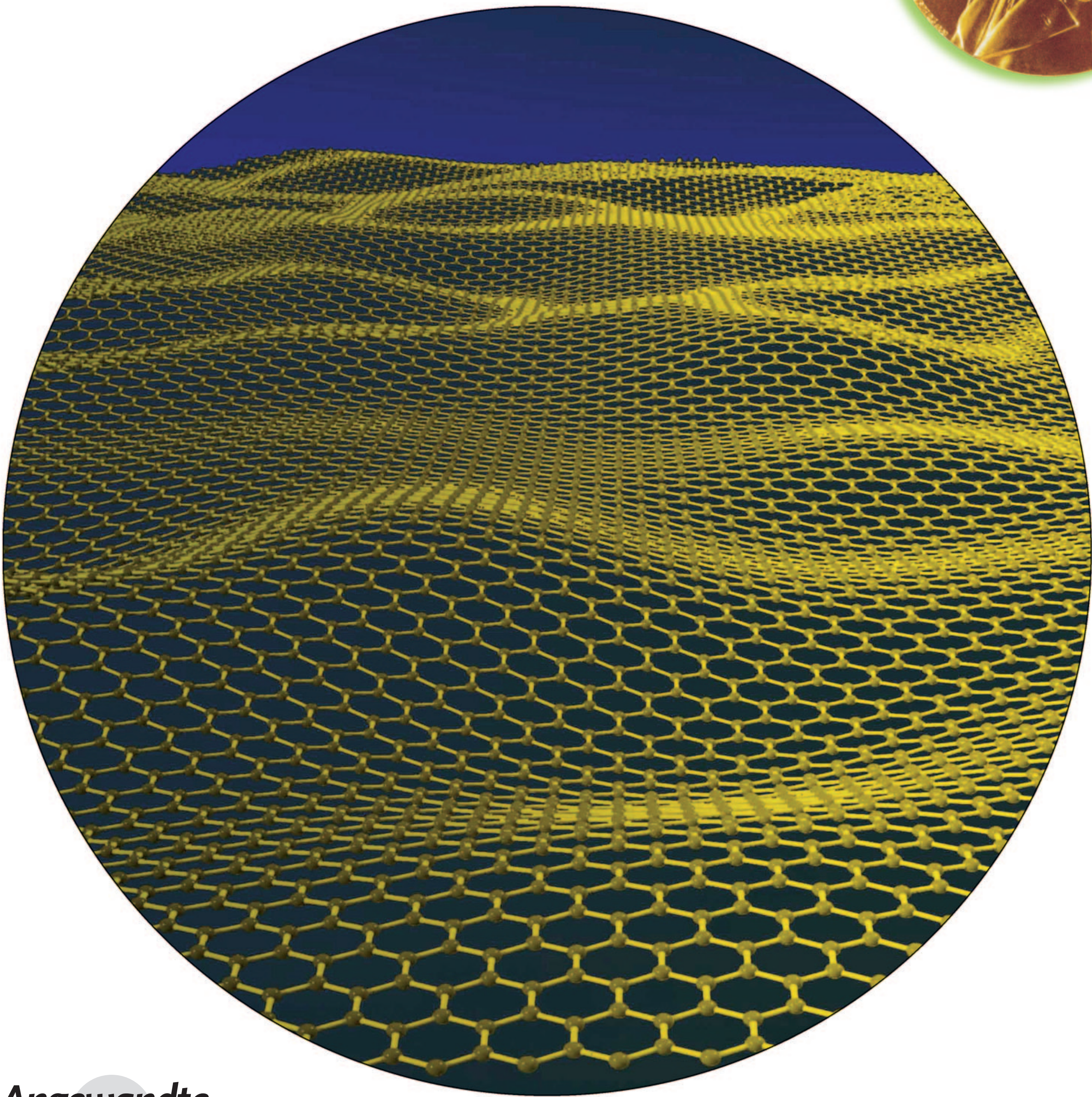
THE NOBEL PRIZE IN PHYSICS 2010





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Random Walk to Graphene (Nobel Lecture)**

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Biography

"I don't think anyone should write their autobiography until after they're dead."
Samuel Goldwyn

Several years ago I was on a trekking trip in a Jordanian desert with a large group of Brits. We were camping and, as usual, there was not much to do in the evenings, so we filled the hours by sitting around a campfire, playing the popular British game "Call My Bluff". In it a player makes several statements only one of which is true, and the rest of the group have to guess which one it is. All other statements are called "bluffs". I teased my fellow hikers with statements like "I was born in the Mediterranean climate", "I was a lieutenant in the Red Army", "I have won an Ig Nobel prize", "I climbed several five kilometer high mountains", "I fell down a 100 m deep crevasse without a rope", "I was called "Russian" for the first time at the age of 32", "At my University I studied intercontinental ballistic missiles", "I was a bricklayer north of the Arctic circle", "I knew Michael Gorbachev personally" and so on. What surprised me was that all but the last statement were dismissed by most of the group as "bluffs", while people found it easy to believe that it is typical for any Russian to know personally their political leaders. I won every single game because the truth was a complete opposite: Apart from knowing Gorbachev (whom I only ever saw on TV) all the other statements were true. This made me think for the first time that, perhaps, my life had not been as trivial as I thought.

Still, with reference to the epigraph, I am not dead yet. I think it is too early for me to write an autobiography, as doing so somehow implies that one's life story is finished. I am only 52 and plan to actively continue my research work. However, I am a law-abiding citizen (of course!) and, according to the rules of the Nobel Foundation, I must provide an autobiography. So, below I have conceded a sort of it, a literary exercise. Although I do not expand on any of the non-bluff statements above, the reader is still likely to find my life path atypical. I do not know whether this somehow influenced my way of doing things or it is just a separate story, having little in common with my research career.

The timeline of this autobiography ends in 1987 when I received a PhD. After that point, my scientific biography is given in the Nobel lecture "Random Walk to Graphene".

Andre Geim

Soviet Taxonomy

I was born on October 21, 1958 in a small Black Sea resort of Sochi, the second son to Nina Bayer and Konstantin Geim. The first seven years of my life I spent there with my grandmother Maria Ziegler and grandfather Nikolai Bayer. I remember little of my grandfather because he died when I was only six, but my grandmother was my best friend and an important part of my life until the university years, when I left home. At the age of seven, it was time to go to school and, reluctantly, I had to leave Sochi and go to live with my parents and my elder brother Vladislav in the city of Nalchik, where they worked. Nalchik is the capital of the small Republic of Kabardino-Balkaria in the foothills of the Caucasus Mountains and can be found on the world map as a host to Europe's highest peak, Elbrus, and in proximity to the infamous Chechnya. For the next ten years I spent the schooltime there but returned to Sochi every year to stay with my grandmother during the summer months.

At this point, it is probably right to mention my ethnic origins, because for certain groups of people in the Soviet Union ethnicity was a very important factor and often defined their life choices and eventually their life path. I belonged to one such group. Despite the great ethnic diversity of the Soviet population (the official census of 1989 listed over 100 ethnicities), the authorities managed to keep track of each and every one of them by having a special line in the Soviet passport ("line 5: nationality"). In my passport this line stated "German". This is because my father came from the so-called

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Volga Germans, descendants of colonists from Germany who settled on the Volga River banks in the 18th century. My mother's bloodline was also mostly German. I have long-believed that my maternal grandmother Maria was Jewish but, according to my brother's recent research into family history, her father was also German. Therefore, to the best of my knowledge, the only Jew in the family was my great-grandmother, with the rest on both sides being German.

A note is needed here to explain why I devote so much space to explaining my ethnicity. Firstly, of course, the word "German" in my Soviet passport had a very real effect on my life, as the reader will find out below. Secondly, the issue of my ethnicity unexpectedly surfaced again after the announcement of the Nobel Prize—suddenly there have been a lot of discussions whether this prize is British, Dutch, Russian, German, or Jewish. To me these discussions seem silly. Having lived and worked in several European countries, I consider myself European and do not believe that any further taxonomy is necessary, especially in such a fluid world as the world of science.

Skeletons in the Old Chest

My knowledge of our family history is rather sketchy and, for a Western person, it is perhaps difficult to understand why. The reason goes back to well before I was born. In Stalin's time, family history was a dangerous subject to discuss, and stories were not passed from generation to generation because parents deliberately concealed their history from the children in order to protect them. A telling example can be found in the many documents that I had to fill out when applying to university, for a job, and so on. Among such documents there was always a questionnaire asking whether you had relatives abroad, whether any of your relatives were prisoners in forced labor camps (the infamous Gulags) or were prisoners of war. I always answered "No" to all those questions, in good faith, believing this answer to be true. It was only in the late 1980s that I learned that nearly everyone in my family, including my father and grandfather, had spent many years in the Gulag, that some of the family had been prisoners in German concentration camps, and that I had an uncle living in Bavaria. This was deliberately and successfully concealed from me during my first 30 years of life.

Below is what I learned since then from my few living relatives. My grandfather Nikolai Bayer was a professor at Kharkov University who specialized in aerial cartography. In 1946, documents were found by the Soviet Army in post-war Poland, which revealed that, after the First World War, he was a junior minister in Petliura's short-lived Ukrainian nationalist government. This anti-Bolshevik past, together with his German ethnicity and the fact that at the time he was compiling maps of Eastern Siberia, was apparently enough reason to accuse him of passing state secrets to the Japanese and send him to a northern Gulag camp near Vorkuta. He was released only in 1953, after Stalin's death.

When I was born, my father was 48 years old and already had quite a long and difficult history behind him as well, which I managed to learn from him bit by bit over many years. Until his last years, he avoided discussing it, even when I asked, and those bits came out mostly accidentally. Before the Second World War he was a young professor at Saratov State University, lecturing physics and math. However, when the war broke out in Europe, being an ethnic German became a political crime and he was sent to a Gulag camp in Siberia, where he spent many years building a hydroelectric power station and a railway. In 1949, he was allowed to join his family who in the meantime had been deported to Novosibirsk.

An episode I vividly remember from my early years is finding a box of old medals at the bottom of an old chest hidden in my grandparents' garden shed in Sochi. One of them was the Cross of St. George, an award of high military distinction in the Russian Empire (before the revolution). I showed my findings to my grandmother and, being confronted, she explained that the Cross belonged to her father who served as an army surgeon in World War I, whereas other decorations were related to the nobility status of her grandfather, a descendant of German aristocrats. In the 19th century, her family lived in Poland (then a part of the Russian Empire), where they took part in the 1863 uprising and, consequently, were deported to Siberia that was to become such a familiar place to my forebears a century later. The next time I tried to find those medals, they were long gone. It was only many years later that I found that my grandma Maria threw them all away immediately after the episode. Incomprehensible as it sounds to us today, this kind of behavior became imprinted in the DNA of people who lived through the Stalinist terror. She was afraid I would talk about the medals to my friends and, if the story got around, the whole family would be in trouble. This already happened in Khrushchev's times, when the terror receded, but "bourgeois" reminders were still deemed unacceptable by "the proletariat" until the 1990s.

By the time I went to school the mentality of Stalin's times was largely gone from the Soviet system. Except for some remnants, such as the "nationality" line and all those family questionnaires, young people like me were largely unaware of the recent terror. The only time I really suffered because of my ethnicity was when trying to get to a top university, as described later. Otherwise, it was just being occasionally called "fascist" in the playground, or a "bloody Jew" ("ЖИД"), because a foreign name was often associated with being a Jew (in Russian, the word ЖИД sounds very offensive). Maybe because of the latter, I am particularly keen to emphasize that some small portion of my blood is likely to be Jewish.

Schooling as Usual

Despite the somber family history, I myself was lucky enough to be born late and had a happy childhood. My best childhood memories are associated with my birthplace, Sochi. My grandma Maria was a meteorologist and I spent my first years of life on the beach, around the weather station where she worked. My mother was a head of quality control and my father chief engineer at a very large vacuum electronics factory (chief engineer would be equivalent to a CEO in the West). After two decades, many people in Nalchik still remember him as a hard-working and influential person. Perseverance and hard work are the qualities I probably inherited from him. My parents' occupations placed our family in the top layer of technocrats in the Soviet Union. They were not within the communist party elite who enjoyed all the perks of the Soviet system and, as ethnic Germans, they could not possibly be. Nevertheless, their status allowed the family a relatively comfortable existence.

My school in Nalchik was called a specialist English language school and considered to be the best in town. Despite its name, the teaching of English was not its strongest point. Looking back and comparing how we were taught English then and how I was taught Dutch 30 years later, the notion of English specialization in my old school seems nothing but laughable. On the other hand, mathematics was taught at an extremely high level, especially in senior forms, thanks mostly to our math teacher, Valenida Sedneva. I may not have realized this at the time, but when I looked at my old exam papers several years later and already a student at an elite university I was amazed at how tough and challenging those papers were. Some of them required not only powers of recall but also imaginative and nonstandard thinking. Physics and chemistry were taught at a good level, too. I once won a regional chemistry Olympiad, which however was not so much due to my love of the subject as to the fact that in a couple of days I managed to memorize a whole chemistry dictionary some 1000 pages long (happily forgotten in the following few days).

I also fondly remember Olga Peshkova, our teacher of Russian and Literature. Despite getting excellent marks in these two subjects, I did not excel in either of them. Still, I like to think that her lessons were helpful in learning—eventually—how to write research papers in a clear and concise way. There is nothing else particularly remarkable to mention about my schooling, except for the brain-washing Soviet propaganda that was penetrating every aspect of our lives at that time. As a counterbalance, schoolchildren often listened to the Voice of America and similar radio stations, and this small rebellion helped us to develop healthy skepticism about many things (albeit not all) that propaganda told us. Of course, as everyone around, I played my due role of a disciplined Soviet pupil.

Failing the First Hurdle

At the age of 16, I graduated from school with a gold medal, a distinction given to those who achieved the perfect score in all subjects (typically, the top 5%). My parents encouraged me to go to the best possible university, and my sights were set on a couple of elite universities in Moscow. At school I was doing well in all exact sciences, including physics and chemistry, but my strongest subject was math. However, my parents persuaded me that pure math would not offer good career prospects. Hence, my decision was to study physics. The very top university for Physics in Russia was (and still is) the Moscow Institute of Physics and Technology (Phystech). However, the entrance examinations to Phystech were famously competitive and extremely tough and, as I grew up in a provincial town, I believed they were beyond my ability. So, I chose to go to another leading university, Moscow Engineering and Physics Institute (MIFI). In the way of preparation I solved problems from sample MIFI and Phystech exam papers and felt ready, even if still not very confident. Little did I know that the main obstacle for me would turn out to be my ethnicity.

The first exam in MIFI was written math, and I was pretty confident that I solved all the problems correctly and would get an "excellent" (the marking system in Russian schools and universities consists of four grades: "excellent", "good", "satisfactory", and "fail"). However, I then found it was only a "satisfactory" and, even worse, my mark for the oral math was a "fail". I attributed this failure to poor preparation and my inexperience in sitting real tests: problems at my oral exam seemed a lot harder than those from the sample MIFI papers that I did at home. So, I decided to go home, continue to study and take my chances a year later.

That gap year turned out to be very important for me. My parents were supportive and found a job for me at the factory where they worked, as a technician responsible for calibration of measurement equipment, and also paid for tuitions in Math, Physics, and Russian literature (these were standard entrance exams at my chosen universities). After a couple of weeks I found that I knew math better than my tutor (who was considered to be the best in the town), so these tutorials stopped. On the other hand, my physics tutorials were the best I could wish for. My tutor was a physics professor from Nalchik's University, Valery Petrosian. I thoroughly enjoyed every lesson. We solved many problems from old exam papers either from Phystech or, even harder, from international Olympiads. But even more helpful was the way he taught me to deal with physics problems: it is much easier to solve a problem if you first guess possible answers. Most problems at Phystech level require understanding of more than one area of physics and usually involve several logical steps. For example, in the case of a five-step solution, the possibilities to deal with the problem quickly diverge and it may take many attempts before one gets to the final answer. If, however, you try to solve the same problem from both ends, guessing two or three plausible answers, the space of possibilities and logical steps is much reduced. This is the way I learned to think then, and I am still using it in my research every day, trying to build all the logical steps between what I have and what I think may be the end result of a particular project. After a couple of months, my tutor no longer asked me to write up a solution. Instead, I just explained verbally the way I would solve a particular problem—all the logical steps required to get to its end without describing routine details. This allowed us to go through the problems at lightning speed.

I also learned an important lesson from my tutorials in Russian literature. My tutor said that what I was writing was good but it was clear from my essays that I tried to recall and repeat the thoughts of famous writers and literature critics, not trusting my own judgment, afraid that my own thoughts were not interesting, important, or correct enough. Her advice was to try and explain my own opinions and ideas, and to use those authoritative phrases only occasionally, to support and strengthen my writing. This simple advice was crucial for me—it changed the way I wrote. Years later I noticed that I was better at explaining my thoughts in writing than my fellow students.

Enemy of the State

After this year of intensive preparations I felt I knew enough and was much more confident than the previous year and ready for MIFI. I easily solved all the problems in the written math exam (which again was first), polished the presentation, and expected an "excellent" mark. However, at the next exam (oral math) I was told that the mark was only "good", and the examiner refused to explain what was wrong or to show me the script, even though it was right there, in front of him. He gave me three further math problems, the hardest I had ever seen. I managed to solve one, partially solved the second one, with a minor mistake, and provided the correct answer to the third one. However, I could not explain how I came up with this answer. It just appeared in my head and I still remember it now: the answer was 998. The mark I got for these efforts was "satisfactory", which was clearly not enough to be admitted to the university. In addition to the rather harsh treatment from the examiner, I noticed more odd things about the exam—apart from me, not one single person in the same room (about 20 candidates) managed to get even a "satisfactory" mark; they all failed. Even more curiously, the names of all the candidates were either Jewish or foreign sounding. I went to look at the lists of people in other examination rooms and most of the names sounded Russian, with a very few exceptions.

Even for someone as naïve as I was at 17, it was clear that there was a policy in place to fail certain ethnic minorities. In hindsight this can be easily explained because this particular University specialized in nuclear physics and, at that time, if you were a Jew or a German, you were assumed to be a potential emigrant who would learn "state secrets" and then go abroad. That was always considered a threat in the Soviet Union. So in a sense it was clearly a policy, and even an understandable policy, but not much advertised. Several years later, I found that there were a few Jewish people who attended and successfully graduated from MIFI. To achieve this, their parents had to go to KGB representatives at MIFI (they were present in every Soviet organization at the so called First Departments) and persuade them that their children were reliable Soviet citizens and had no intention to leave the country. Apparently, these tactics did work but neither I nor probably my parents even suspected that it was needed. Or, maybe, my parents were too aware of the true lies in my family questionnaires.

Accidental Physicist

This was the first time I experienced discrimination at an official level and it was quite a shock. Fortunately, there was still a week left to try my luck at another university. I said to myself "what the hell" and applied to Phystech. The way I was treated there was a shocking experience in itself, as it was so different from MIFI. The examiners were friendly and even helpful, the exam problems interesting and the whole environment welcoming. I felt as if by mistake someone put me in a wrong room, away from a firing squad of examiners. Perhaps, this was the case.

My examination marks were comfortably above the threshold required for admission, even though I got only one "excellent" mark out of four exams, with the rest "good". I felt that I could have done better, but my MIFI experience was still fresh, and the memories of those failed exams kept coming back, affecting my concentration and sometimes my judgement of the difficulty of the problems. This was especially apparent in my oral physics exam, which I still remember well. The first problem given to me seemed easy and I quickly solved it, but the examiner said "It's a wrong answer". I tried to protest, and it took us a few minutes to understand that I solved a much harder problem than the one he gave me; even though the answer to the problem I actually solved was correct, it was still a fault. Incredibly, the same story happened with the second problem. So, when giving me the third one, the examiner repeatedly asked whether I was sure that I understood what was being asked.

The last hurdle at Phystech was an admissions interview and I was scared that the question of my ethnicity would arise again and they may not accept me despite the good marks. It was well known that, on the basis of the interview, sometimes candidates with marks just below the threshold were accepted and those with marks above rejected. The ethnic question did arise in the form of "How is your German?" I answered "Barely" and started thinking what else to add. One of the panel members (Seva Gantmakher, as in the Gantmakher effect) quickly interjected saying "Then he is not a real German". As it turned out, this remark, as well as his following interventions, influenced all of my further life by putting me on the path of solid-state physics.

Like many would-be students of that age, I dreamt of doing astrophysics or particle physics and aspired to solve "the greatest mysteries of the universe". But there was a rumor among Phystech candidates that saying so was considered to be very naïve by interviewers. I remembered that but did not want to cheat. So, when asked about my aspirations, I said that I wanted to study neutron stars (true) because I wanted to understand how matter behaved at extremely high densities (an excuse, not to sound so naïve). A prompt reply from Seva was "Good, you can then study high-pressure physics at our Institute [of Solid-State Physics]".

Another memory of that interview is being asked to estimate the weight of the earth atmosphere (it was customary to give candidates some tricky mental problems to solve). I spent most of my three minutes multiplying the numbers in my head (atmospheric pressure multiplied by the surface area of the earth divided by gravity, all in the SI units) and when I gave an answer in trillions of trillions of kg, everyone was surprised because I was only expected to give a general answer, not a specific number.

This is how I entered Phystech. In the end my rejection from MIFI turned out to be a blessing in disguise because Phystech was a two-notch higher level university. The only reason I did not go there first was because I did not believe I was up to it. Basically, circumstances forced on me my first choice rather than the second one!

Mother of all Grilling

Phystech is quite an exceptional university not only by Russian standards, where it is considered crème de la crème, but also with respect to any other university I know. The only reason that it is not found in any world league tables is that it is a purely teaching university. (Teaching and research are traditionally separated in Russia—research is done mainly at the Academy of Sciences and teaching at universities.) In addition to the very rigorous student selection, a well-known reason for Phystech being so good was that, unlike other Soviet universities, all specialist and some general courses were taught by practicing scientists from the Academy institutes from all over the Moscow region. Of course, in the West it is a standard to have active researchers giving undergraduate courses, but in Russia it is an exception.

Even more importantly, as Phystech students, we were forced to think and find logic in everything we studied, as opposed to just memorizing facts and formulas. For a large part, this was due to the examination style: when it came to specialized subjects, many of the exams we took every year were open-book. This meant that there was no need to remember formulas, as long as one knew where to find them. Instead, the problems were challenging, requiring combinations of different subject areas and thus teaching us to really understand science rather than merely to memorize it.

From the moment of its establishment, Phystech was led by prominent Soviet scientists such as Kapitsa, Landau, and many others. Among my own lecturers and examiners were many eminent scientists such as Emmanuel Rashba, Vladimir Pokrovski, Viktor Lidskii, Spartak Belyaev, Lev Pitaevskii, Isaak Khalatnikov, and Lev Gorkov, to name but a few. I have to admit that their names did not tell me much at the time, which was helped by the fact that I was not very good at attending lectures. I rediscovered some of the names only recently, when I saw their signatures in my old exam certificates, which Phystech put on the web after the Nobel Prize announcement.

The workload at Phystech was heavy and the courses extremely challenging. It is probably enough to say that our standard textbooks for quantum mechanics, statistical physics, electrodynamics, and classical mechanics were from the Landau-Lifshitz Theoretical Physics Course. Perhaps they are not the best textbooks for undergraduate students, but they are a good indication of the expected level of achievement. Not all students managed to sustain the psychological pressure imposed by this teaching style and some dropped out not only because of bad marks but, more often, because of nervous breakdowns. I personally knew several students who developed suicidal tendencies and psychiatric problems. My own sanity was perhaps saved by the amount of alcohol that I and some of my friends consumed after each exam to release the accumulated stress.

The first two and a half years of foundation courses were particularly tough. After that the pressure subsided, as we moved on to specialist courses. From year three, we started attending lectures at the so-called base institutes of the Academy of Sciences. In my case it was the Institute of Solid-State Physics in Chernogolovka, chosen at the discussed interview due to my love for high-density neutron stars. From year five, we also started working in research labs—not on some specially designed undergraduate projects but on real ongoing projects, where we worked as part of an academic research team. Year six was a Master's year and 100% research based. After that, the normal route (if you wanted to stay in academia) was two years of research probation and, if you were successful, you were eligible for a PhD studentship which lasted another 3 years. It was an 11 year long process to get a PhD—6 years at Phystech plus 5 years leading to a viva.

For me personally, only the first half a year at Phystech was a struggle. I came from a provincial town, while some of my classmates were graduates of elite Moscow schools specializing in physics and math. Quite a few were winners of international Olympiads in physics or mathematics. The first few months were essentially designed to bring everyone to the level of those guys; they were nearly a year ahead of the rest of us in formal topics, especially math. Only after I got all the highest marks in the first set of mid-year exams did I start feeling confident enough in this wunderkind environment and was able to relax somewhat. Despite all the pressure and grilling, every single one of us who managed to graduate from Phystech have great memories of those hard years and are most proud of our alma mater.

Go with the Top Flow

I graduated from Phystech with a so-called "red diploma", which meant within top 5 to 10% of my class. Out of 50 or so final exam marks, I got only two "good". One of them was for a course on "political economy of socialism", which I attributed without much shame to my inability to find any logic in the subject. By contrast, I got "excellent" for the political economy of capitalism and to this day have fond memories of reading *Das Kapital* by Karl Marx, whom I occasionally quote to tease or, perhaps, shock my Western colleagues. My second "good" was for the course on superconductivity taught by Lev Gorkov himself, who also was my examiner. Oddly for Phystech, he did not allow us to use textbooks during the exam (shame on him), and I made a mistake in one of the derivations. This is funny because in the 1990s, when I was already a professor in the Netherlands, superconductivity became my research subject.

Despite the exam success, I do not believe I particularly stood out among the students in my class. In my year there were one or two students with only "excellent" marks, and some were digging deeper and understood the courses better than I did. At that time, I did not really try my best; I worked just hard enough to guarantee myself maximum marks and stay at the top of the class. I was successful at that, but it did not take all of my time or effort. In fact, in my university years I was not at all an exemplary student. With excellent marks, I normally was entitled to a scholarship awarded every half a year, but it was quite regularly (four or five times) withdrawn as a punishment for missing some mandatory lectures, being late from holiday breaks, organizing those after-exam parties that sometimes saw some people end up in a hospital, and similar misbehavior. Missing lectures was generally allowed (unless it was a political subject) and I managed to miss most of them. I learnt from textbooks and attended group tutorials, unless I disliked particular tutors. I would not recommend this style of learning to aspiring students as a recipe for success, but it may well suit some people as it suited me and a few other students in my class.

My attitude of doing alright to reach a goal but not doing my utmost persisted through all the university and PhD years.

I only started to really enjoy physics and do my absolute best, for the sake of it, much later when I became an independent researcher.

From the Sublime to the Ridiculous

The topic of my Master's project was electronic properties of metals, which I studied by exciting electromagnetic waves (so-called helicons) in spherical samples of ultrapure indium. From the helicon resonances I could extract information about the resistivity of those samples. The competitive edge of this research was the extreme purity of the indium I was working with, such that at low temperatures electrons could shoot over distances comparable with the sample diameter (ca. 1 cm). After graduating, I started working towards my PhD in the same laboratory, as was customary for many Phystech graduates. Looking back, those five years of doing PhD seem remarkably uneventful in terms of the science I was doing.

My first year as a PhD student was signified by an event that was to become a rather regular perturbation in my life: moving from one institute to another. This was when my PhD supervisor, Victor Petrashov, moved from the Institute of Solid-State Physics to the newly established Institute of Microelectronics Technology. Although the two were only 200 m apart, it meant a serious disruption of work, losing some equipment, and setting everything up again. Initially, I did the metal physics research with some enthusiasm, but it gradually faded away as I realized that no one, except perhaps my supervisor, was interested in what I was doing. Nevertheless, educationally, those years were very important for developing experimental skills and making my fingers "green". This experience played a crucial role in my further research career, including the graphene story. In this respect, I owe a lot to Victor, whom I count as one of the most skilful experimentalists I ever met. With the help of a shoestring and sealing wax he could do amazing things, and a shoestring and sealing wax was what, in those days, we typically had in research labs in Chernogolovka.

I meet quite a few people who feel nostalgia for the "golden era" of the Soviet science, but I myself never saw those times, even in Chernogolovka, which was a rather elitist academic place. My recollection is that the arrival of almost any material important for research, be it copper wire or GE varnish, was a cause for celebration, almost on a par with the arrival of a multimillion piece of equipment in the West. Once Victor was lucky to borrow a US-made lock-in amplifier to do some measurements, which we usually had to do using a Soviet equivalent (the word "equivalent" does not describe the entirety of the difference). In just a couple of weeks I was able to get results that I could not dream of with the "equivalent". The availability of resources (or the lack of them) essentially dictated what I could possibly do. I believe experimentalists who claim to have witnessed "the greatness of the Soviet science" either belonged to the select few who had benefactors among the top academicians or, more likely, fool themselves, choosing to believe that the skies were bluer in the old days.

Having said this, it is true that in the Soviet Union there was a huge difference between being an experimentalist and being a theorist. The theory school was extremely strong, especially what people referred to as "Landau theory school". Those guys did things at the highest possible level. The roots of this strength were partly in education, but also in the way Soviet theorists worked. I witnessed it by attending many research seminars. A lot of time was spent in discussions and heated debates where there were no questions that could not be asked and no authority that could not be questioned. In the West, this style is still remembered well by those who "experienced" Soviet scientists in the 1980s and 1990s. It could be a dreadful experience for the participants, but sometimes I really miss this style. The nostalgia usually comes after coming across certain papers in today's scientific literature: If they were to be first presented at such seminars, even the authors would not dare to put them in print. Those debates were very influential and allowed people to learn quicker and to develop a broad and informed view of many areas of physics. I myself benefited greatly from such seminars, and consider them the second most important part of my education in Chernogolovka. Many of the seminars I attended were organized by Seva Gantmakher. His care for detail and breadth of experimental knowledge were a great example for me and my fellow students.

Despite the great atmosphere in theory departments, even theorists suffered from the state of the Soviet science, and in the late 1980s many of the best of them moved to the West. I do not think that better living conditions were the only reason for this brain drain: Theoretical ideas do not come out of vacuum; they are often born in interaction with experimentalists, as experimental results serve as a trigger for new ideas. This was completely lacking in Chernogolovka, because new results were hard—if at all possible—to get with the existing equipment. By the time of my PhD, Soviet experimental science had decayed to the point where it was considered that the most appropriate route to reach the top of fame and glory for an experimentalist was to confirm a theory produced by an eminent Soviet theoretician. Indeed, many experimentalists in Chernogolovka were doing just that.

This was my scientific life. Parallel to that, there was another life, busy with events. Chernogolovka is a nice Moscow suburb, quiet and peaceful, surrounded by forest. Life was generally pleasant, even though my living conditions were austere to the extreme—for most of my years there I lived in a hall of residence, sharing a room with two other young researchers. One of my roommates was Sergey Dubonos, who over the years became my regular co-author and also played an important role in the graphene paper recognized by the Nobel award. In addition to research, my other hobbies were mountaineering and white-water canoeing. Every year I spent more than a month in the mountains and on the rivers in different corners of the Soviet Union, from Caucasus to Central Asia, sometimes managing to fit in as many as four trips in a year. Those travel experiences were often shared with Max Maximenko and Phystech friend Stas Ionov. It was at this time that I met my wife, Irina Grigorieva, who was also working towards a PhD in the neighboring Institute of Solid-State Physics. She later became my collaborator and significantly contributed to the graphene work.

In a way, Chernogolovka offered ideal conditions for scientists—there were hardly any distractions, which allowed us to concentrate on research. Except for queuing for hours for sausages and cheese (which had become a regular scene in the 1980s), most of the time was spent in the labs. Even without much enthusiasm, my research advanced at a steady pace, with a few papers published and due progress made. But it was only when I became an independent researcher, and especially after the move to the West in 1990, that I started to do my real best and the pace of my life changed dramatically, as described in my Nobel lecture "Random Walk to Graphene".

RANDOM WALK TO GRAPHENE

If one wants to understand the beautiful physics of graphene, they will be spoilt for choice, with so many reviews and popular science articles now available. I hope that the reader will excuse me if on this occasion I recommend my own writings.[1–3] Instead of repeating myself here, I have chosen to describe my twisty scientific road that eventually led to the Nobel Prize. Most parts of this story are not described anywhere else, and its timeline covers the period from my PhD in 1987 to the moment when our 2004 paper, recognized by the Nobel Committee, was accepted for publication. The story naturally gets denser in events and explanations towards the end. Also, it provides a detailed review of pre-2004 literature and, with the benefit of hindsight, attempts to analyze why graphene has attracted so much interest. I have tried my best to make the article not only informative but also easy to read, even for non-physicists.

Zombie Management

My PhD thesis was called "*Investigation of mechanisms of transport relaxation in metals by a helicon resonance method*". All I can say is that the stuff was as interesting at that time as it sounds to the reader today. I published five journal papers and finished the thesis in five years, the official duration for a PhD at my institution, the Institute of Solid-State Physics. *Web of Science* soberly reveals that the papers were cited twice, by co-authors only. The subject was dead a decade before I even started my PhD. However, every cloud has its silver lining and what I uniquely learnt from that experience was that I should never torture research students by offering them "zombie" projects.

After the PhD, I worked as a staff scientist at the Institute of Microelectronics Technology, Chernogolovka, which belongs to the Russian Academy of Sciences. The Soviet system allowed and even encouraged junior staff to choose their own line of research. After a year of poking in different directions, I separated research-wise from my former PhD supervisor, Victor Petrashov, and started developing my own niche. It was an experimental system that was both new and doable, which was nearly an oxymoron, taking into account the scarce resources available at the time at Soviet research institutes. I fabricated a sandwich consisting of a thin metal film and a superconductor separated by a thin insulator. The superconductor served only to condense an external magnetic field into an array of vortices, and this highly inhomogeneous magnetic field was projected onto the film under investigation. Electron transport in such a microscopically inhomogeneous field (varying on a submicron scale) was new research territory, and I published the first experimental report on the subject,[4] which was closely followed by an independent paper from Simon Bending.[5] It was an interesting and reasonably important niche, and I continued studying the subject for the next few years, including a spell at the University of Bath in 1991 as a postdoctoral researcher working with Simon.

This experience taught me an important lesson: that introducing a new experimental system is generally more rewarding than trying to find new phenomena within crowded areas. Chances of a success are much higher where the field is new. Of course, fantastic results one originally hopes for are unlikely to materialize, but, in the process of studying any new system, something original inevitably shows up.

One Man's Junk, Another Man's Gold

In 1990, thanks to Vitaly Aristov, director of my Institute in Chernogolovka at the time, I received a six month visiting fellowship from the British Royal Society. Laurence Eaves and Peter Main from Nottingham University kindly agreed to accept me as a visitor. Six months is a very short period for experimental work, and circumstances dictated that I could only study devices readily available in the host laboratory. Available were submicron GaAs wires left over from previous experiments, all done and dusted a few years earlier. Under the circumstances, my experience of working in the poverty-stricken Soviet academy was helpful. The samples that my hosts considered practically exhausted looked like a gold vein to me, and I started working 100 h per week to exploit it. This short visit led to two *Phys. Rev. Letters* of decent quality,[6,7] and I often use this experience to tease my younger colleagues. When things do not go to plan, and people start complaining, I provoke them by proclaiming "*there is no such thing as bad samples; there are only bad postdocs/students*". Search carefully and you always find something new. Of course, it is better to avoid such experiences and explore new territories but, even if one is fortunate enough to find an experimental system as new and exciting as graphene, meticulousness and perseverance allow one to progress much further.

The pace of research at Nottingham was so relentless and, at the same time, so inspiring, that a return to Russia was not an option. Swimming through Soviet treacle seemed no less than wasting the rest of my life. So, at the age of 33 and with an h index of 1 (latest papers not yet published), I entered the Western job market for postdocs. During the next four years, I moved between different universities, from Nottingham to Copenhagen to Bath and back to Nottingham, and each move allowed me to get acquainted with yet another topic or two, significantly broadening my research horizons. The physics I studied in those years could be broadly described as mesoscopic and involved such systems and phenomena as two-dimensional electron gases (2DEGs), quantum point contacts, resonant tunneling, and the quantum Hall effect (QHE), to name but a few. In addition, I became familiar with GaAlAs heterostructures grown by molecular beam epitaxy (MBE) and improved my expertise in microfabrication and electron-beam lithography, technologies I had started learning in Russia. All these elements came together to form the foundation for the successful work on graphene a decade later.

Dutch Comfort

By 1994 I had published enough quality papers and attended enough conferences to hope for a permanent academic position. When I was offered an associate professorship at the University of Nijmegen, I instantly seized upon the chance of having some security in my new post-Soviet life. The first task in Nijmegen was of course to establish myself. To this end, there was no start-up and no microfabrication to continue any of my previous lines of research. As resources, I was offered access to magnets, cryostats, and electronic equipment available at Nijmegen's High Field Magnet Laboratory, led by Jan Kees Maan. He was also my formal boss and in charge of all the money. Even when I was awarded grants as the principal investigator (Dutch funding agency FOM was generous during my stay in Nijmegen), I could not spend the money as I wished. All funds were distributed through so-called "working groups" led by full professors. In addition, PhD students in the Netherlands could formally be supervised only by full professors. Although this probably sounds strange to many, this was the Dutch academic system of the 1990s. It was tough for me then. For a couple of years, I really struggled to adjust to the system, which was such a contrast to my joyful and productive years at Nottingham. In addition, the situation was a bit surreal, because outside the university walls I received a warm-hearted welcome from everyone around, including Jan Kees and other academics.

Still, the research opportunities in Nijmegen were much better than in Russia and, eventually, I managed to survive scientifically, thanks to the help from abroad. Nottingham colleagues (in particular Mohamed Henini) provided me with 2DEGs that were sent to Chernogolovka, where Sergey Dubonos, a close colleague and friend from the 1980s, microfabricated requested devices. The research topic I eventually found and later focused on can be referred to as mesoscopic superconductivity. Sergey and I used micron-sized Hall bars made from a 2DEG as local probes of the magnetic field around small superconducting samples. This allowed measurements of their magnetization with accuracy sufficient to detect not only the entry and exit of individual vortices but also much more subtle changes. This was a new experimental niche, made possible by the development of an original technique of ballistic Hall micromagnetometry.[8] During the next few years, we exploited this niche area and published several papers in *Nature* and *Phys. Rev. Lett.*, which reported a paramagnetic Meissner effect, vortices carrying fractional flux, vortex configurations in confined geometries, and so on. My wife Irina Grigorieva, an expert in vortex physics,[9] could not find a job in the Netherlands and, therefore, had plenty of time to help me with conquering the subject and writing papers. Also, Sergey not only made the devices but also visited Nijmegen to help with measurements. We established a very productive modus operandi where he collected data and I analyzed them within an hour on my computer next door to decide what should be done next.

A Spell of Levity

The first results on mesoscopic superconductivity started emerging in 1996, which made me feel safer within the Dutch system and also more inquisitive. I started looking around for new areas to explore. The major facility at Nijmegen's High Field Lab was powerful electromagnets. They were a major headache, too. These magnets could provide fields up to 20 T, which was somewhat higher than 16 to 18 T available with the superconducting magnets that many of our competitors had. On the other hand, the electromagnets were so expensive to run that we could use them only for a few hours at night, when electricity was cheaper. My work on mesoscopic superconductivity required only tiny fields (< 0.01 T), and I did not use the electromagnets. This made me feel guilty as well as responsible for coming up with experiments that would justify the facility's existence. The only competitive edge I could see in the electromagnets was their room-temperature bore. This was often considered as an extra disadvantage because research in condensed matter physics typically requires low, liquid-helium temperatures. The contradiction prompted me, as well as other researchers working in the lab, to ponder on high-field phenomena at room temperature. Unfortunately, there were few to choose from.

Eventually, I stumbled across the mystery of the so-called magnetic water. It is claimed that putting a small magnet around a hot water pipe prevents formation of scale inside the pipe. Or install such a magnet on a water tap, and your kettle would never suffer from chalky deposits. These magnets are available in a great variety in many shops and on the internet. There are also hundreds of articles written on this phenomenon, but the physics behind it remains unclear, and many researchers are skeptical about the very existence of the effect.[10] Over the last 15 years I have made several attempts to investigate "magnetic water", but they were inconclusive, and I still have nothing to add to the argument. However, the availability of ultrahigh fields in a room-temperature environment invited lateral thinking about water. Basically, if magnetic water existed, I thought, then the effect should be clearer in 20 T rather than in typical fields of < 0.1 T created by standard magnets.

With this idea in mind and, allegedly, on a Friday night, I poured water inside the lab's electromagnet when it was at its maximum power. Pouring water inside a magnet is certainly not a standard scientific approach, and I cannot recall why I behaved so "unprofessionally". Apparently, no one tried such a silly thing before, although similar facilities existed in several places around the world for decades. To my surprise, water did not end up on the floor but got stuck in the vertical bore of the magnet. Humberto Carmona, a visiting student from Nottingham, and I played for an hour with the water by breaking the blockage with a wooden stick and changing the field strength. As a result, we saw balls of levitating water (Figure 1). This was awesome. It took little time to realize that the physics behind was good old diamagnetism. It took much longer to adjust my intuition to the fact that the feeble magnetic response of water (ca. 10^{-5}), that is billions of times weaker than that of iron, was sufficient to compensate the earth's gravity. Many colleagues, including those who worked with high magnetic fields all their lives, were flabbergasted, and some of them even argued that this was a hoax.

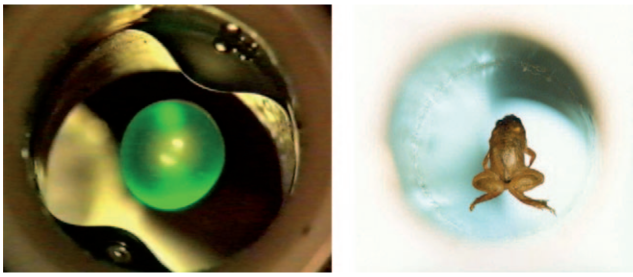

Figure 1. Levitating moments in Nijmegen. Left: A ball of water (about 5 cm in diameter) freely floats inside a vertical bore of an electromagnet. Right: The frog that learned to fly. This image continues to serve as a symbol, showing that magnetism of "nonmagnetic things", including humans, is not so negligible. This experiment earned Michael Berry and me the 2000 Ig Nobel Prize. We were asked first whether we dared to accept this prize, and I take pride in our sense of humor and self-deprecation that we did.

I spent the next few months demonstrating magnetic levitation to colleagues and visitors, as well as trying to make a "non-boffin" illustration for the beautiful phenomenon. Out of the many objects that we had floating inside the magnet, it was the image of a levitating frog (Figure 1) that started the media hype. More importantly, though, behind all the media noise, this image found its way into many textbooks. However quirky, it has become a beautiful symbol of ever-present diamagnetism that is no longer perceived to be extremely feeble. Sometimes I am stopped at conferences by people exclaiming "*I know you! Sorry, it is not about graphene. I start my lectures with showing your frog. Students always want to learn how it could fly.*" The frog story with some intricate physics behind the stability of diamagnetic levitation is described in my review in *Physics Today*.[11]

Friday Night Experiments

The levitation experience was both interesting and addictive. It taught me the important lesson that poking in directions far away from my immediate area of expertise could lead to interesting results, even if the initial ideas were extremely basic. This in turn influenced my research style, as I started making similar exploratory detours that somehow acquired the name "Friday night experiments". The term is of course inaccurate. No serious work can be accomplished in just one night. It usually requires many months of lateral thinking and digging through irrelevant literature without any clear idea in sight. Eventually, you get a feeling—rather than an idea—about what could be interesting to explore. Next, you give it a try and, normally, you fail. Then, you may or may not try again. In any case, at some moment you must decide (and this is the most difficult part) whether to continue further efforts or cut your losses and start thinking of another experiment. All this happens against the backdrop of your main research and occupies only a small part of your time and brain.

Already in Nijmegen, I started using lateral ideas as under- and postgraduate projects, and students were always excited to buy a pig in a poke. Kostya Novoselov, who came to Nijmegen as a PhD student in 1999, took part in many of these projects. They never lasted for more than a few months, in order not to jeopardize a thesis or career progression. Although the enthusiasm inevitably vanished towards the end, when the predictable failures materialized, some students later confided that those exploratory detours were invaluable experiences.

Most surprisingly, failures sometimes failed to materialize. Gecko tape is one such example. Accidentally or not, I read a paper describing the mechanism behind the amazing climbing ability of geckos.[12] The physics is rather straightforward. Gecko's toes are covered with tiny hairs. Each hair attaches to the opposite surface with a minute van der Waals force (in the nN range), but billions of hairs work together to create a formidable attraction, sufficient to keep geckos attached to any surface, even a glass ceiling. In particular, my attention was attracted by the spatial scale of their hairs. They were submicron in diameter, the standard size in research on mesoscopic physics. After toying with the idea for a year or so, Sergey Dubonos and I came up with procedures to make a material that mimicked gecko's hairy feet. He fabricated a square centimeter of this tape, and it exhibited notable adhesion.[13] Unfortunately, the material did not work as well as gecko's feet, deteriorating completely after a couple of attachments. Still, it was an important proof-of-concept experiment that inspired further work in the field. Hopefully, one day someone will develop a way to replicate the hierarchical structure of gecko's setae and its self-cleaning mechanism. Then, gecko tape can go on sale.

Better To Be Wrong than Boring

While preparing for the lecture in Stockholm, I compiled a list of my Friday night experiments. Only then did I realize a stunning fact. There were two dozen or so experiments over a period of approximately 15 years and, as expected, most of them failed miserably. But there were three hits, the levitation, gecko tape, and graphene. This implies an extraordinary success rate: more than 10%. Moreover, there were probably near-misses, too. For example, I once read a paper[14] about giant diamagnetism in FeGeSeAs alloys, which was interpreted as a sign of high-temperature superconductivity. I asked Lamarches for samples and got them. Kostya and I employed ballistic Hall magnetometry to check for the giant diamagnetism, but found nothing, even at 1 K. This happened in 2003, well before the discovery of iron pnictide superconductivity, and I still wonder whether there were any small inclusions of a superconducting material which we missed with our approach. Another miss was an attempt to detect "heartbeats" of individual living cells. The idea was to use 2DEG Hall crosses as ultrasensitive electrometers to detect electrical signals due to physiological activity of individual cells. Even though no heartbeats were detected while a cell was alive, our sensor recorded huge voltage spikes at its "last gasp", when the cell was treated with excess alcohol.[15] Now, I attribute this near-miss to the unwise use of yeast, a very dormant microorganism. Four years later, similar experiments were done using embryonic heart cells and—what a surprise—graphene sensors, and they were successful in detecting such bioelectrical activity.[16]

Frankly, I do not believe that the above success rate can be explained by my lateral ideas being particularly good. More likely, this tells us that poking in new directions, even randomly, is more rewarding than is generally perceived. We are probably digging too deep within established areas, leaving plenty of unexplored stuff under the surface, just one poke away. When one dares to try, rewards are not guaranteed, but at least it is an adventure.

Mancunian Way

By 2000, with mesoscopic superconductivity, diamagnetic levitation, and four *Nature* papers under my belt, I was well placed to apply for a full professorship. Colleagues were rather surprised when I chose the University of Manchester, declining a number of seemingly more prestigious offers. The reason was simple. Mike Moore, chairman of the search committee, knew my wife Irina when she was a very successful postdoc in Bristol rather than my co-author and a part-time teaching lab technician in Nijmegen. He suggested that Irina could apply for the lectureship that was there to support the professorship. After six years in the Netherlands, the idea that a husband and wife could officially work together had not even crossed my mind. This was the decisive factor. We appreciated not only the possibility of sorting out our dual career problems, but also felt touched that our future colleagues cared. We have never regretted the move.

So, in early 2001, I took charge of several dilapidated rooms storing ancient equipment of no value, and a start-up of £100 K. There were no central facilities that I could exploit, except for a helium liquefier. No problem. I followed the same routine as in Nijmegen, combining help from other places, especially Sergey Dubonos. The lab started shaping up surprisingly quickly. Within half a year, I received my first grant of £500 K, which allowed us to acquire essential equipment. Despite being consumed with our one year old daughter, Irina also got her starting grant a few months later. We invited Kostya to join us as a research fellow (he continued to be officially registered in Nijmegen as a PhD student until 2004 when he defended his thesis there). And our group started generating results that led to more grants that in turn led to more results.

By 2003 we published several good-quality papers including in *Nature*, *Nature Materials*, and *Phys. Rev. Lett.*, and we continued beefing up the laboratory with new equipment. Moreover, thanks to a grant of £1.4 M (research infrastructure funding scheme masterminded by the then science minister David Sainsbury), Ernie Hill from the Department of Computer Sciences and I managed to set up the Manchester Centre for Mesoscience and Nanotechnology. Instead of pouring the windfall money into brick-and-mortar, we utilized the existing clean-room areas (ca. 250 m^2) in Computer Sciences. Those rooms contained obsolete equipment, and it was thrown away and replaced with state-of-the-art microfabrication facilities, including a new electron-beam lithography system. The fact that Ernie and I are most proud of is that many groups around the world have more expensive facilities, but our Centre continuously, since 2003, has been producing new structures and devices. We do not have here a posh horse that is for show, but rather a draft horse that has been working really hard.

Whenever I describe this experience to my colleagues abroad, they find it difficult to believe that it is possible to establish a fully functional laboratory and a microfabrication facility in less than three years and without an astronomical start-up. If not for my own experience, I would not believe it either. Things progressed unbelievably quickly. The University was supportive, but my greatest thanks are reserved specifically for the responsive mode of the UK Engineering and Physical Sciences Research Council (EPSRC). The funding system is democratic and non-xenophobic. Your position in an academic hierarchy or an old-boys network counts for little. Also, "visionary ideas" and grand promises to "address social and economic needs" play little role when it comes to the peer review. In truth, the responsive mode distributes its money on the basis of a recent track record, whatever it means in different subjects, and the funding normally goes to researchers who work both efficiently and hard. Of course, no system is perfect, and one can always hope for a better one. However, paraphrasing Winston Churchill, the UK has the worst research funding system, except for all the others that I am aware of.

Three Little Clouds

As our laboratory and Nanotech Centre were shaping up, I got some spare time for thinking of new research detours. Gecko tape and the failed attempts with yeast and quasi-pnictides took place during that time. Also, Serge Morozov, a senior fellow from Chernogolovka, who later became a regular visitor and invaluable collaborator, wasted his first two visits on studying magnetic water. In the autumn of 2002, our first Manchester PhD student, Da Jiang, arrived, and I needed to invent a PhD project for him. It was clear that for the first few months he needed to spend his time learning English and getting acquainted with the lab. Accordingly, as a starter, I suggested to him a new lateral experiment. It was to make films of graphite "*as thin as possible*" and, if successful, I promised we would then study their "mesoscopic" properties.

Recently, trying to analyze how this idea emerged, I recalled three badly shaped thought clouds.

One cloud was a concept of "metallic electronics". If an external electric field is applied to a metal, the number of charge carriers near its surface changes so that one may expect that its surface properties change, too. This is how modern semiconductor electronics works. Why not use a metal instead of silicon? As an undergraduate student, I wanted to use the electric-field effect (EFE) and X-ray analysis to induce and detect changes in the lattice constant. It was naïve because simple estimates show that the effect would be negligible. Indeed, no dielectric allows fields much higher than $1\,\mathrm{V\,nm^{-1}}$, which translates into maximum changes in charge-carrier concentration n at the metal surface of about 10^{14} per cm^2. In comparison, a typical metal (e.g., Au) contains approximately 10^{23} electrons per cm^3 and, even for a 1 nm thick film, this yields relative changes in n and conductivity of about 1%, leaving aside much smaller changes in the lattice constant.

Previously, many researchers aspired to detect the field effect in metals. The first mention is as far back as 1902, shortly after the discovery of the electron. J. J. Thomson (1906 Nobel Prize in Physics) suggested to Charles Mott, the father of Nevill Mott (1977 Nobel Prize in Physics), to look for the EFE in a thin metal film, but nothing was found.[17] The first attempt to measure the EFE in a metal was recorded in the science literature in 1906.[18] Instead of a normal metal, one could also think of semimetals such as bismuth, graphite, or antimony, which have a lot fewer carriers. Over the last century, many researchers used Bi films ($n \approx 10^{18}\,\mathrm{cm^{-3}}$), but observed only small changes in their conductivity.[19,20] Aware of this research area and with experience in GaAlAs heterostructures, I was continuously, albeit casually, looking for other candidates, especially ultrathin films of superconductors in which the field effect can be amplified in proximity to the superconducting transition.[21,22] In Nijmegen, my enthusiasm was once sparked by learning about nanometer thick Al films grown by molecular beam epitaxy (MBE) on top of GaAlAs heterostructures but, after estimating possible effects, I decided that chances of success were so poor it was not worth trying.

Carbon nanotubes were the second cloud hanging around in the late 1990s and early 2000s. Those were the years when nanotubes were at the peak of their glory. Living in the Netherlands, I heard talks of Cees Dekker and Leo Kouwenhoven and read papers by Thomas Ebbesen, Paul McEuen, Sumio Iijima, Pheadon Avouris, and others. Each time, those exceptionally nice results inevitably triggered thoughts about entering this research area. But I was too late and needed to find a different perspective, away from the stampede.

As for the third cloud, I read a review of Millie Dresselhaus about intercalated graphite compounds,[23] which clearly showed that, even after many decades, graphite was still a material little understood, especially in terms of its electronic properties. This influential review prompted me to look further into graphite literature. In doing so, I encountered papers from Pablo Esquinazi and Yakov Kopelevich, who reported ferromagnetism, superconductivity, and a metal-insulator transition, all in the same good old graphite and at room temperature.[24,25] Those provocative papers left me with a distinct feeling that graphite was much worth having a careful look at.

The three thought clouds (and maybe some more that I cannot recall) somehow merged into Da's project. I reckoned that if we were to succeed in making thin films of graphite, instead of Bi, they could exhibit some electric field effect and/or some other interesting properties resembling those of carbon nanotubes. In the worst case scenario, our mesoscopic samples would be monocrystals and this could help to clarify those controversies about graphite. Why not try to poke in this direction for a few months, I thought.

Legend of Scotch Tape

To make thin graphite films, I provided Da with a tablet of pyrolytic graphite, which was several millimeters thick and an inch in diameter, and suggested using a polishing machine. We had a fancy one that allowed submicron flatness. A few months later, Da declared that he reached the ultimate thickness and showed me a tiny speck of graphite at the bottom of a Petri dish. I looked at it in an optical microscope and, by focusing on the top and bottom surfaces, estimated that the speck was about 10 μm thick. Too thick, I thought and suggested trying a finer polishing liquid. However, it turned out that Da polished away the whole tablet to obtain this one speck. It was actually my fault: Da successfully finished his PhD later, but at that time he was just a fresh foreign student with a huge language barrier. Moreover, by mistake I gave him high-density graphite instead of highly oriented pyrolytic graphite (HOPG), as was intended. The former does not shed as easily as HOPG.

Oleg Shklyarevskii, a senior fellow from Kharkov, Ukraine was working nearby and had to listen to the typical flow of my teasing remarks, this time about polishing a mountain to get one grain of sand. Oleg was an expert in scanning tunneling microscopy (STM) and worked on a project that later turned out to be another bad "Friday night" idea of mine. He interjected by bringing over a piece of sellotape with graphite flakes attached to it. Allegedly, he just fished out the tape from a litter bin. Indeed, HOPG is the standard reference sample for STM, where a fresh surface of graphite is normally prepared by removing a top layer with sticky tape. We used this technique for years, but never looked carefully at what was thrown away along with the tape. I looked in the microscope at the remnants of graphite (Figure 2) and found pieces much thinner than Da's speck. Only then did I realize how silly it was of me to suggest the polishing machine. Polishing was dead, long live Scotch tape!

This moment was not a breakthrough yet, but things started to look promising and required more people to get involved. Oleg did not volunteer to take on yet another project, but Kostya did. "Volunteer" is probably not the right word. Everyone in our lab has always been welcome to move around and participate in whatever project they want. At that time, Kostya was working on a nicely moving project on ferromagnetism.[26] He was also our "caretaker" when things went wrong, especially with measuring equipment. As for me,

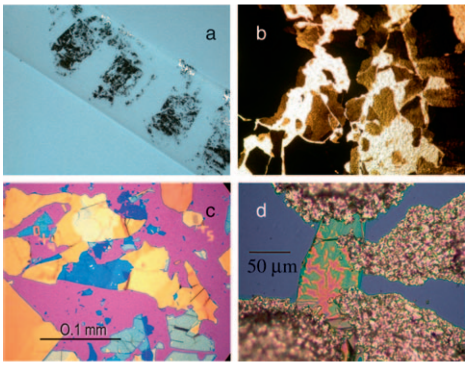

Figure 2. In hindsight, thin crystals of graphite are easy to obtain. a) Remnants of HOPG left attached to Scotch tape. b) Some of the crystals are optically transparent if viewed in an optical microscope or just with a magnifying glass. c) If placed on an oxidized Si wafer, transparent crystals give rise to various shades of blue. d) One of our very first devices made by using "a shoestring and sealing wax": in this case, tweezers, a toothpick, and silver paint.

at that time I used to spend a few hours a day in the lab preparing samples, doing measurements, and analyzing results. It was only after 2006 that I turned into a paper-writing machine combined with a data analyzer. I have always loved the latter, but hated to write papers. Unfortunately, no lab can survive without its Shakespeare.

Kostya and I decided to check out the electrical properties of the graphite flakes found on the sellotape and, to this end, he started transferring them onto glass slides, initially by using just tweezers. A few days later, and keeping in mind the initial motivation, I brought in oxidized Si wafers in order to use them as substrates and detect the EFE. This delivered an unexpected bonus. Placing thin graphite fragments onto those wafers allowed us to observe interference colors that indicated that some of the fragments were optically transparent. Moreover, the colors provided us with a very intuitive way of judging which flakes were thin (Figure 2c). We quickly found that some of them were just a few nanometers thick. This was our first real breakthrough.

Eureka Moment

In graphene literature, and especially in popular articles, a strong emphasis is placed on the Scotch tape technique, and it is hailed for allowing the isolation and identification of ultrathin graphite films and graphene. For me, this was an important development, but still not a Eureka moment. Our goal always was to find some exciting physics rather than just observing ultrathin films in a microscope.

Within a couple of days after Oleg prompted the use of Scotch tape, Kostya was already using silver paint to make electrical contacts to graphite platelets transferred from the Scotch tape. To our surprise, they turned out to be highly conductive and even the painted contacts exhibited a reasonably low resistance. The electronic properties could be studied, but we felt it was too early to put the ugly looking devices (see Figure 2d) in a cryostat for proper measurements. As a next step, we applied voltage, first, through the glass slides and, a bit later, to the Si wafer, using it as a back gate to check for the field effect. Figure 2 shows a photograph of one of our first devices. The central part is a graphite crystal that is approximately 20 nm thick, and its lateral size is comparable to the diameter of a human hair. To transfer the crystal by tweezers from the tape and then make four such closely spaced contacts by using just a toothpick and silver paint is the highest level of experimental skill. These days, not many researchers have fingers green enough to make such samples. I challenge readers to test their own skills against this benchmark!

The very first hand-made device on glass exhibited a clear EFE such that its resistance could be changed by several percent. It may sound little and of marginal importance but, aware of how hard it was previously to detect any EFE at all, I was truly shocked. If those ugly devices made by hand from relatively big and thick platelets already showed some field effect, what could happen, I thought, if we were to use our thinnest crystallites and apply the full arsenal of microfabrication facilities? There was a click in my head that we had stumbled onto something really exciting. This was my Eureka moment.

What followed was no longer a random walk. From this point, it was only logical to continue along the same path by improving procedures for cleaving and finding thinner and thinner crystals, and making better and better devices, which we did. It was both painstaking and incredibly rapid, depending on one's viewpoint. It took several months until we learned how to identify monolayers by using optical and atomic force microscopy. On the microfabrication side, we started using electron-beam lithography to define proper Hall bar devices and started making contacts by metal evaporation rather than silver painting. The microfabrication development was led by Dubonos, aided by his PhD student Anatoly Firsov. Initially, they employed facilities in Chernogolovka but, when our new postdoc Yuan Zhang got fully acquainted with the recently installed lithography system at our Nanotech Centre, the process really speeded up.

The move from multilayers to monolayers and from hand-made to lithography devices was conceptually simple, but never straightforward. We took numerous detours and wasted much effort on ideas that only led us into dead ends. An example of grand plans that never worked out was the idea to plasma-etch graphite mesas in the form of Hall bars which, after cleavage, should provide readily shaped devices, or so I thought. Later, we had to return to the unprocessed graphite. The teething problems we experienced at that time can also be illustrated by the fact that initially we believed that Si wafers should have a very precise thickness of the oxide (within several nm) to allow hunting for monolayers. These days we can find graphene on practically any substrate. Crystal sizes also went up from a few microns to nearly a millimeter, just by tinkering with procedures and using different sources of graphite.

The most essential part of our 2004 report[27] was the electrical measurements, and this required a lot of work. For several months, Kostya and Serge Morozov were measuring full time, and I was around as well, discussing and analyzing raw data, often as soon as they appeared on the screen. The feedback to our microfabrication guys was almost instantaneous. As always in the case of encountering a new system where one does not know what to expect, we had to be particularly careful in those first experiments. We disregarded any curve, unless it was reproducible for many devices and, to avoid any premature conclusions, we studied more than 50 ultrathin devices. Those were years of hard work compressed into just a few months, but we were excited as every new device got better and better, and we could work 24×7, which typically meant 14 hour days and no breaks for the weekends.

Finally, by the end of 2003, we got a reliable experimental picture ready for publication. Between that moment and the end of my timeline when the *Science* paper was accepted in September 2004, there is a lengthy gap. Those nine months were consumed by excruciating efforts to publish the results in a high-profile journal. We continuously added data and polished the presentation. Irina's help was invaluable in this time-consuming process, which can be fully appreciated only by those readers who ever published in such glossy journals. First, we submitted the manuscript to *Nature*. It was rejected and, when further information requested by referees was added, rejected again. According to one referee, our report did "not constitute a sufficient scientific advance." *Science* referees were more generous (or more knowledgeable?), and the presentation was better polished by that time. In hindsight, I should have saved the time and nerves by submitting to a second-tier journal, even though we all felt that the results were groundbreaking. Readers aspiring to get published in those glossy magazines and having their papers recently rejected can use this story to cheer up: Their papers may also be prize winning!

Defiant Existence

One of the most surprising results of our *Science* report was the observation that, after being isolated, atomic planes remained continuous and conductive under ambient conditions. Even with hindsight, there are many reasons to be surprised.

First, for many decades researchers studied ultrathin films, and their collective experience proves that continuous monolayers are practically impossible to make (see, e.g. Ref. [28,29],). Try to evaporate a metal film a few nanometers in thickness, and you will find it discontinuous. The material coagulates into tiny islands. This process called island growth is universal and driven by the fact that a system tries to minimize its surface energy. Even by using epitaxial substrates that provide an interaction working against the surface energy contribution and cooling them down to liquid-helium temperature, which prevents migration of deposited atoms, it is hard to find the right conditions to create continuous nm thick films, let alone monolayers.[28,29]

The second reason to be surprised is that theory unequivocally tells us that an isolated graphene sheet should be thermodynamically unstable. Calculations show that "*graphene is the least stable [carbon] structure until about 6000 atoms*".[30] Until approximately 24000 atoms (that is, a flat sheet with a characteristic size of about 25 nm), various three-dimensional (3D) configurations are energetically more favorable than the two-dimensional (2D) geometry.[30,31] For larger sizes, theory shows again that a graphene sheet is unstable, but now with respect to scrolling. The latter conclusion is based on considering competing contributions from the bending and surface energies.[32,33] These calculations are specific to carbon, but the underlying physics is conceptually connected to the surface energy mechanism that leads to island growth.

Third, 2D crystals cannot be grown in isolation, without an epitaxial substrate that provides an additional atomic bonding. This follows from the Landau–Peierls argument that shows that the density of thermal fluctuations for a 2D crystal in the 3D space diverges with temperature.[1] Although the divergence is only logarithmic, crystal growth normally requires high temperature such that atoms become sufficiently mobile. This also implies a softer lattice with little shear rigidity. The combination of the two conditions sets a limit on possible sizes L of 2D atomic crystals. One can estimate L as approximately $a\exp(E/T_G)$ where $a \approx 1$ Å is the lattice spacing, $E \approx 1$ eV the atomic bond energy, and T_G the growth temperature. This consideration should not be applied to graphene at room temperature (300 K≅0.025 eV), which would yield astronomical sizes. Crystal growth normally requires temperatures T_G comparable to the bond energy and the disorder-generating mechanism is irrelevant at much lower temperatures. Note that, in principle, self-assembly may allow growth of graphene at room temperature but, so far, this has been achieved only for nanometer-sized graphene sheets.[34]

The fourth and probably the most important reason to be surprised is that graphene remains stable under ambient conditions. Surfaces of materials can react with air and moisture, and monolayer graphene has not one but two surfaces, making it more reactive. Surface science research involves ultrahigh vacuum facilities and, often, liquid-helium temperature to keep surfaces stable and away from reactive species. For example, gold is one of the most inert materials in nature but, even for gold, it is hard to avoid its near-surface layer being partially oxidized in air. What then are the chances for a monolayer exposed to ambient conditions to remain unaffected?

Graphene flouts all the above considerations. It is instructive to analyze how. First, any existing method of obtaining graphene starts with 3D rather than 2D growth. Graphene sheets are initially formed either within the bulk or on top of an epitaxial substrate, which quenches the diverging thermal fluctuations. The interaction can be relatively weak, as in the case of graphene grown on graphite,[35] but it is always present. This allows graphene to dodge the Landau–Peierls argument and, also, to avoid coagulation into islands and 3D carbon structures. Second, if graphene is cleaved or released from a substrate, the process is normally carried out at room temperature so that energy barriers remain sufficiently high. This allows atomic planes to persist in an isolated, nonscrolled form without any substrate,[36] even though this is energetically unfavorable. If placed on a substrate, the van der Waals interaction may also be sufficient to prevent a graphene sheet from scrolling. Third, graphite is even more chemically inert than gold. Although graphene is more reactive than graphite and weakly reacts with air and pollutants at room temperature, this does not destroy its crystal lattice and high conductivity.[37,38] It requires temperatures above 300 °C to irreversibly damage graphene in air. Our ambient conditions appear fortuitous enough for the graphene lattice to survive.

Requiem for Brilliant Ideas

Science literature is full of brilliant ideas that did not work. Searching the literature for those is not a good idea at all. At a start of a new project, a couple of decent reviews usually do the job of making sure that one does not reinvent the wheel. The alternative can be truly detrimental. I have met many promising researchers who later failed to live up to their promise because they wasted their time on searching literature, instead of spending it on searching for new phenomena. What's more, after months of literature search, they inevitably came to the same conclusion: Everything they planned had been done before. Therefore, they saw no reason to try their own ideas and, consequently, began a new literature search. One should realize that ideas are never new. However brilliant, every idea is always based on previous knowledge and, with so many smart people around, the odds are that someone somewhere had already thought of something similar before. This should not be used as an excuse for not trying because local circumstances vary and, moreover, facilities change with time. New technologies offer a reasonable chance that old failed ideas may work unpredictably well the next time round.

In 2002/2003, the merged thought clouds that I would not even call a brilliant idea were sufficient to instigate the project. They also provided us with an Ariadne's thread that helped with choosing specific directions. A literature search was done in due course, after we roughly scouted the new area and especially when the results were being prepared for publication. In addition to the literature relevant to the thought clouds, our *Science* paper cited the challenges of obtaining isolated 2D crystals, their thermodynamic instability, and the observation of nanoscrolls and papers on epitaxial growth. Those references were important to show the experimental progress we achieved. The first review of earlier literature was done in our 2007 progress article.[1] Since then, I updated my conference presentations whenever a historically important paper came to light. This is the first opportunity to update the history chapter in writing by adding several new references. Furthermore, my recent call for further historical insights[39] was answered by a number of researchers and, for completeness, I want to acknowledge their early ideas and contributions, too.

Graphene Incarnations

Looking back at graphene history, we should probably start with an observation by the British chemist Benjamin Brodie.[40] In 1859, by exposing graphite to strong acids, he obtained what he called "carbonic acid" (Figure 3 a). Brodie believed that he discovered "graphon", a new form of carbon with a molecular weight of 33. Today we know that he

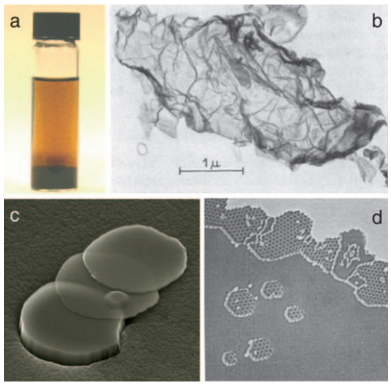

Figure 3. Prehistory of graphene. a) Graphene as probably seen by Brodie 150 years ago. Graphite oxide at the bottom of the container dissolves in water making the yellow suspension of floating graphene flakes. b) TEM image of ultrathin graphitic flakes from the early 1960s (copied with permission from Ref. [43]). c) Scanning electron microscopy (SEM) image of thin graphite platelets produced by cleavage (similar to images reported in Ref. [60]). d) STM of graphene grown on Pt (copied with permission from Ref. [53]). The image is 100 × 100 nm^2 in size. The hexagonal superstructure has a period of about 22 Å and appears due to the interaction of graphene with the metal substrate.

observed a suspension of tiny crystals of graphene oxide, that is, graphene sheets densely covered with hydroxy and epoxide groups.[41] Over the next century, there were quite a few papers describing the laminated structure of graphite oxide, but the next crucial step in graphene history was the proof that this "carbonic acid" consisted of floating atomic planes. In 1948, Ruess and Vogt used transmission electron microscopy (TEM) and, after drying a droplet of a graphene oxide suspension on a TEM grid, they observed creased flakes down to a few nanometers in thickness.[42] These studies were continued by the group of Hofmann. In 1962, he and Boehm looked for the thinnest possible fragments of reduced graphite oxide and identified some of them as monolayers[43] (Figure 3 b).

This remarkable observation received little attention until 2009–2010. I have to mention that the 1962 identification relied on a relative TEM contrast, an approach that would not stand today's scrutiny because the contrast strongly depends on focusing conditions.[44] For example, Rahul Nair and I tried but, predictably, failed to distinguish between monolayers and somewhat thicker flakes by using only their TEM contrast. Graphene monolayers were unambiguously identified in TEM only 40 years after the 1962 paper by counting the number of folding lines.[45–47] Nonetheless, the Boehm–Hofmann work should, in my opinion, stand as the first observation of graphene because monolayers should have been present among the residue, and the idea was correct. Furthermore, it was Boehm and his colleagues who in 1986 introduced the term graphene, deriving it from the combination of the word "graphite" and the suffix that refers to polycyclic aromatic hydrocarbons.[48]

In addition to the TEM observations, another important line in pre-2004 graphene research was its epitaxial growth. Ultrathin graphitic films and, sometimes, even monolayers were grown on metal substrates,[49–53] insulating carbides,[54–57] and graphite[35] (see Figure 3 d). The first papers I am aware of go back to 1970, when Grant and Haas reported graphitic films on Ru and Rh[49] and Blakely et al. on Ni.[50] Epitaxial growth on insulating substrates was first demonstrated by van Bommel et al. in 1975,[54] whereas Oshima et al. found other carbides allowing graphene growth (for example, TiC).[55] The grown films were usually analyzed by surface science techniques that average over large areas and say little about the film's continuity and quality. Occasionally, STM was also used for visualization and local analysis.

Even more relevant were earlier attempts to obtain ultrathin films of graphite by cleavage, similar to what we did in 2003. In 1990, Kurz's group reported "*peeling optically thin layers with transparent tape*" (read Scotch tape), which were then used to study charge carrier dynamics in graphite.[58] In 1995, Ebbesen and Hiura described few-nanometer-thick "origami" visualized by atomic force microscopy (AFM) on top of HOPG.[59] Ruoff et al. also photographed thin graphite platelets in SEM[60] (Figure 3 c). In 2003, monolayers were reported by Gan et al., who used STM for their cleavage on top of HOPG.[61]

Finally, there were electrical studies of thin graphite films. Between 1997 and 2000, Ohashi et al. succeeded in cleaving crystals down to approximately 20 nm in thickness, studied their electrical properties including Shubnikov–de Haas oscillations, and, quite remarkably, observed the electric field effect with resistivity changes of up to 8 %.[62,63] Also, Ebbesen's group succeeded in the growth of micron-sized graphitic disks with thickness down to 60 layers and measured their electrical properties.[64]

As for theory, let me make only a short note (for more references, see Refs. [1,65]). Theoretically, graphene ("a monolayer of graphite") was around since 1947, when Wallace first calculated its band structure as a starting point to understanding the electronic properties of bulk graphite.[66] Semenoff and Haldane realized that graphene could provide a nice condensed-matter analogue of $(2+1)$-dimensional quantum electrodynamics[67,68] and, since then, the material has served as a toy model to address various questions of QED (see, e.g. Refs. [69,70]). Many of the theories became relevant to experiment well before 2004, when electronic properties of carbon nanotubes (rolled-up graphene ribbons) were investigated. A large amount of important theoretical work on graphene was done by Ando, Dresselhaus, and coworkers (see, e.g. Refs. [71–73]).

To complete the history of graphene, let me also acknowledge some earlier ideas. Ebbesen and Hiura envisaged a possibility of graphene-based nanoelectronics in 1995 (as an example, they referred to epitaxial graphene grown on TiC).[59] In patent literature, speculations about "*field effect transistors employing pyrolytic graphite*" go back as far as 1970.[74] Also, it was pointed out to me by Ruoff et al. and Little that their pre-2004 papers discussed possibilities and mentioned an intention of obtaining isolated monolayers.[60,75] Finally, the layered structure of graphite was known since early days of X-ray crystallography, and researchers certainly have been aware of graphite being a deck of weakly bonded graphene planes for an even longer time. This property has been widely used to create a variety of intercalated graphite compounds[23] and, of course, to make drawings. After all, we now know that isolated monolayers can be found in every pencil trace, if one searches carefully enough in an optical microscope.[2] Graphene has literally been before our eyes and under our noses for many centuries but was never recognized for what it really is.

Πλανήτη Graphene

The reader may find some of the cited ideas and historical papers irrelevant, but I tried my best to avoid any pre-2004 result, especially experimental, being overlooked. All the mentioned studies poked in the right direction, but there were no big surprises to spark a graphene gold rush. This is probably because the earlier experiments had one thing in common. They were observational. They observed ultrathin graphitic films, and occasionally even monolayers, without reporting any of graphene's distinguishing properties. The very few electrical and optical measurements cited above were done using thin films of graphite and could not assess the physics that graphene brought to the fore since 2004.

Our *Science* paper provided a clear watershed. Of course, the article reported the isolation of graphene crystals large enough to do all sorts of measurements, beyond the observation in an electron or scanning probe microscope. Of course, the described method of graphene isolation and identification was so straightforward and accessible that even schoolchildren could probably do it. This was important, but, if we were to stop there, just with the observations, our work would only add to the previous literature and, I believe, disappear into oblivion. It is not the observation and isolation of graphene but its electronic properties that took researchers by surprise. Our measurements delivered news, well beyond the Scotch tape technique, which persuaded many researchers to join in the graphene rush.

First, the 2004 paper reported an ambipolar electric field effect, in which resistivity changed by a factor of about 100. This is thousands times more than the few percent changes observed previously for any metallic system and amounted to a qualitative difference. To appreciate the exquisiteness of this observation, imagine a nanometer-thick Au film. No matter what you do with such a film by physical means, it will remain a normal metal with the same properties. In contrast, properties of graphene can be altered by simply varying the gate voltage. We can tune graphene from a state close to a normal metal with electrons in a concentration ca. 10^{21} cm^{-3} to a metal with a similar concentration of holes, all the way through a "semiconducting" state with few charge carriers.

Even more remarkably, our devices exhibited an astonishing electronic quality. Graphene was completely unprotected from the environment, as it was placed on a microscopically rough substrate and covered from both sides with adsorbates and a polymer residue. Still, electrons could travel submicron distances without scattering, flouting all the elements outside. This level of electronic quality is completely counterintuitive. It contradicts the common wisdom that surface science requires ultrahigh vacuum and, even then, thin films become progressively poorer in quality as their thickness decreases. Even with hindsight, such electronic quality is mystifying and, in fact, not fully understood so far.

In semiconductor physics, electronic quality is described in terms of charge carrier mobility μ. Our *Science* paper reported graphene with room-temperature $\mu \approx 10000\,cm^2V^{-1}s^{-1}$ (as of 2010, μ can be 10 and 100 times higher at room and low temperature, respectively[76,77]). For a general reader, 10000 may sound like just another number. To explain its significance, let us imagine that in 2004 we made devices from, for example, reduced graphene oxide, which exhibits $\mu \approx 1\,cm^2V^{-1}s^{-1}$ due to its irreversibly damaged crystal lattice.[78] In our second paper on graphene,[79] we reported 2D dichalcogenides with equally low μ. Since then, there has been little interest in them. The reported ballistic transport over submicron distances was essential to spark the interest in graphene and to allow the observation of many quantum effects reported both in 2004 and later. This would have been impossible if graphene exhibited μ below several $1000\,cm^2V^{-1}s^{-1}$.

If not for graphene's high quality and tunability, there would be no new physics and, therefore, no graphene boom. In this respect, graphene history has something in common with that of solar planets. Ancient Greeks observed them and called them wandering stars, πλανήτες. After the physics behind this wandering was discovered, people started perceiving planets quite differently from πλανήτες. Similarly, during the last six years people discovered what graphene really is, which completely changed the earlier perception. Our *Science* paper offered the first glimpse of graphene in its new avatar as a high quality 2D electronic system and beyond.

Magic of Flat Carbon

What is this new incarnation? For me, 2004 was only the starting point for the unveiling of many unique properties of graphene. Since then, we have demonstrated that charge carriers in graphene are massless fermions described by a Dirac-like equation rather than by the standard Schrödinger equation.[80] In bilayer graphene, electrons receive yet another makeup as massive Dirac fermions.[81] These properties were unveiled by the observation of two new types of the integer quantum Hall effect, which corresponded to the two types of Dirac fermions.[1,65] We also found that graphene remained metallic in the limit of no charge carriers, even when just a few electrons remained present in a micron-sized device.[1,77] Our experiments have revealed that graphene exhibits a universal optical conductivity of $\pi e^2/2h$, such that its visible opacity is just $\pi\alpha$, where α is the fine-structure constant.[82] We suggested that the phenomenon of Klein tunneling, which was known in relativistic quantum physics for many decades but assumed non-observable, could be probed using graphene devices.[83] Several groups later demonstrated this experimentally. We were lucky to be slightly quicker than others in showing that bilayer graphene was a tunable-gap semiconductor[84] and that graphene could be carved into devices on the true nanometer scale.[85] We demonstrated sensors capable of detecting individual molecules, more sensitive than any sensor before.[38] We suggested that strain in graphene creates pseudomagnetic fields that alter its electronic properties[86] and, later, discussed a possibility of creating uniform pseudofields and observation of the quantum Hall effect without an external magnetic field.[87] Pseudomagnetic fields in excess of 400 T were reported experimentally half a year later. We made the first step into graphene chemistry by introducing experimentally its derivatives, graphane and stoichiometric fluorographene.[88,89] This is not even an exhaustive list of the nice phenomena that we and our collaborators found in graphene and, of course, many other researchers reported many other beautiful discoveries that propelled graphene into its new status of a system that can deliver nearly magic.

Ode to One

After reading about the beautiful properties of graphene, the reader may wonder why many atomic layers stacked on top of each other, as in graphite, do not exhibit similar properties. Of course, any graphitic derivative has something in common with its parent, but for the case of graphene, differences between the parent and descendants are fundamental. To appreciate it, let us simplify the task and compare graphene with its bilayer. The crucial distinctions are already there.

First, graphene exhibits record stiffness and mechanical strength.[90] As for its bilayer, this strength is jeopardized by the possibility for the two layers to slide relative to each other. This leads to a principal difference if, for example, graphene or any thicker platelets are used in composite materials. Second, graphene chemistry is different depending on whether one or both surfaces of a monolayer are exposed. For example, atomic hydrogen cannot bind to graphene from one side, but creates a stoichiometric compound (graphane) if both surfaces are exposed. This makes graphene much more reactive than its bilayer. Third, an electric field is screened in graphite at distances of about the interlayer separation, and the electric screening becomes important even for a bilayer. For multilayer graphene, the electric field can dope no more than a couple of near-surface atomic planes, leaving the bulk unaffected. This makes it naïve to speculate about the use of graphitic multilayers in active electronics. Fourth, charge carriers in a monolayer are massless Dirac fermions whereas they are massive in a graphene bilayer. This leads to essential differences in many electronic properties including Shubnikov–de Haas oscillations, quantum Hall effect, Klein tunneling, and so on. The Sorites paradox refers to a moment when a heap is no longer a heap if the grains are removed one by one. For graphene, even its bilayer is so different that two already make a heap.

To Colleagues and Friends

Our *Science* report was a collective effort, and I would like again—on behalf of Kostya and myself—to thank all the other contributors. Serge Morozov was and remains our "multi-tasking measurement machine" working 24×7 when in Manchester. His electrical measurement skills are unmatched, and I know that any curve he brings in is completely reliable and no questions are ever asked whether this and that was checked and crosschecked. Da Jiang was around from the very start, and it is unfortunate that I had to take the project away from him because it was beyond the scope of a single new PhD student. Sergey Dubonos and Yuan Zhang were the ones who made all the devices without which our work would obviously be impossible. I utterly regret that our life trajectories have later diverged and, especially, that Sergey has switched from microfabrication technology to goat farming. I also acknowledge help of Anatoly Firsov in making those devices. Irina Grigorieva helped with scanning electron microscopy but, more importantly, with writing up the 2004 manuscript (Figure 4).

The end of my timeline was only a start for further hard work involving many collaborators. Our rapid progress would be impossible without Misha Katsnelson, who provided us with all the theoretical help an experimentalist can only dream of. Since 2006, I have been enjoying collaboration with other great theory guys including Antonio Castro Neto, Paco Guinea, Nuno Peres, Volodya Fal'ko, Leonid Levitov, Allan MacDonald, Dima Abanin, Tim Wehling, and their co-workers. In particular, I want to acknowledge many illuminating discussions and banter over dinners with Antonio and Paco. As for experimentalists, the list is longer and includes Philip Kim, Ernie Hill, Andrea Ferrari, Eva Andrei, Alexey Kuzmenko, Uschi Bangert, Sasha Grigorenko, Uli Zeitler, Jannik Meyer, Marek Potemskii, and many of their colleagues.

Philip deserves special praise. In August 2004, before our *Science* paper was published, his group submitted another important paper.[91] His report described electronic properties of ultrathin graphite platelets (down to about 35 layers). Except for the thicker devices, Philip's group followed the same route as our now-celebrated paper. How close he was can be judged from the fact that, after adopting the Scotch tape technique, Philip started studying monolayers in early 2005. This allowed him to catch up quickly and, in mid-2005, our two groups submitted independent reports that appeared back-to-back in *Nature*, both describing the all-important observation of Dirac fermions in monolayer graphene.[80,92] Later, I had the pleasure of closely working with Philip on two joint papers, for *Science* and *Scientific American*. For me personally, those back-to-back *Nature* papers signified a watershed. People within the large semiconducting community no longer rumored that "*the results were as difficult to reproduce as those by Hendrik Schön*", and friends no longer stopped me in corridors with "*be more careful; you know …*" I owe Philip a great deal for this, and many people heard me saying—before and after the Nobel Prize—that I would be honored to share it with him.

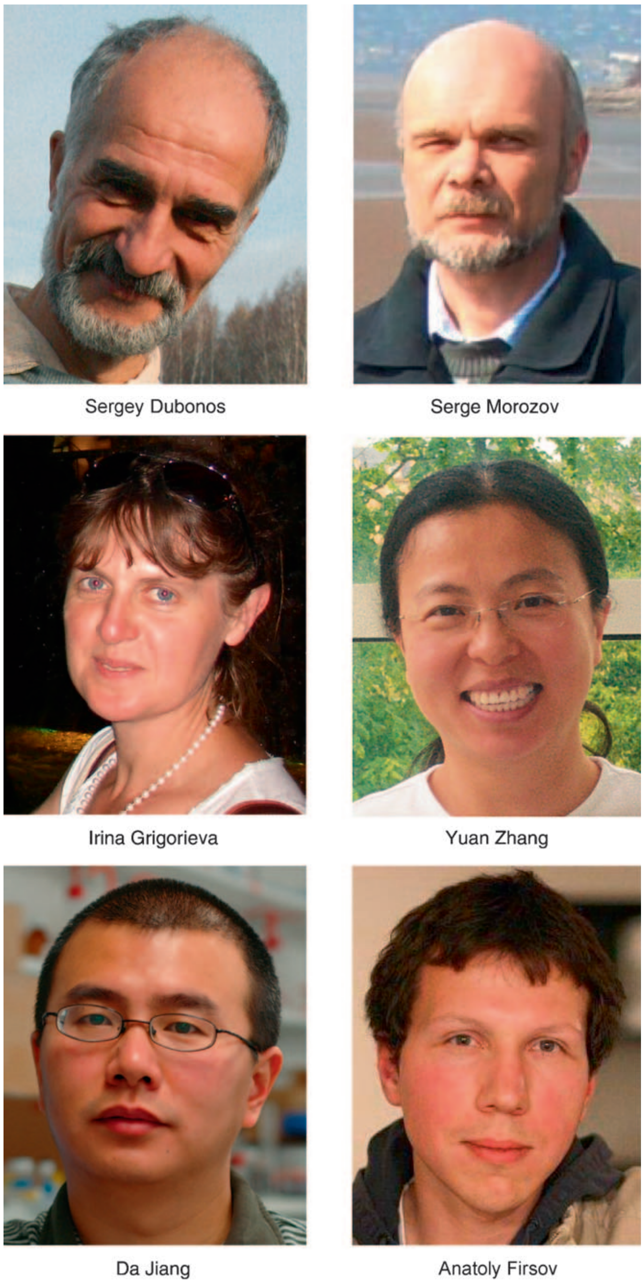

Figure 4. Those who made our first graphene paper possible but did not get the Prize.

Last but not least let me acknowledge many bright young, and not so young, colleagues: Peter Blake, Rahul Nair, Roman Gorbachev, Leonid Ponomarenko, Fred Schedin, Daniel Elias, Sasha Mayorov, Rui Yang, Vasyl Kravets, Zhenhua Ni, Wencai Ren, Rashid Jalil, Ibtsam Riaz, Soeren Neubeck, Tariq Mohiuddin, and Tim Booth. They were PhD students and postdocs here in Manchester over the last six years and, as always, I avoid using the feudal word "my".

Finally, I acknowledge the financial support of the EPSRC in its best, that is, the responsive mode. This Nobel Prize would be absolutely impossible without this mode. Let me also thank the Royal Society and the Leverhulme Trust for reducing my teaching loads, which allowed me to focus on the project. I have also received funding from the Office of Naval Research and the Air Force Office of Scientific Research, which helped us to run even faster. The Körber Foundation is gratefully acknowledged for its 2009 award. However, I can offer no nice words for the EU Framework programs that, except for the European Research Council, can be praised only by Europhobes for discrediting the whole idea of an effectively working Europe.

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