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The Frank–Read source

[An account prepared from F. C. Frank's remarks at the meeting on 1 May 1979]

Frank, Sir Charles, F.R.S. Born Durban, South Africa, 1911. *During World War II, with R. V. Jones as Assistant Director Intelligence (Science). From* 1947 *until retirement, research fellow and subsequently Professor at University of Bristol. Research work on crystal growth, crystal dislocations and liquid crystals.*

Frank first became interested in dislocations through reading Taylor's 1934 paper. He wrote to Taylor, pointing out that his model was two-dimensional, and asking if a dislocation had to be a straight line. This letter was not answered, and Frank did not think further about dislocations until he came to Bristol at Mott's invitation after the war, when he read the paper by J. M. Burgers early in 1947 and realized that the question was answered, and that one should think in three dimensions instead of two. Mott asked him to give a series of seminars on nucleation and crystal growth, and he then realized that the predicted rate of growth was 10^{1000} times too small, and – the key point – that if a screw dislocation emerged from a crystal face, nucleation was unnecessary. The 'spiral' theory of Burton, Cabrera & Frank followed. There was a summer school in Bristol at which Frank explained the theory, and Griffin, from Tolansky's laboratory, in the audience, produced photographs showing not only spirals, but other more complex features predicted by the theory (see Griffin 1951).

Bardeen visited Bristol in 1947, interested Thornton Read in dislocations on his return, and the Frank–Read source emerged as follows, in Frank's words:

> It was a very remarkable coincidence in time. I was going to a conference in the U.S., the Pittsburgh conference on crystal plasticity. I was invited to go to that meeting and one of the things that I had very definitely in mind was that the first point I must make was that dislocations existed, because I understood that there were quite a number of respectable American metallurgists and so on who simply said 'they don't exist – show us'; 'this is just a figment of the theoretician's imagination'. So the first point that I wanted to make was that dislocations do exist, they are as real as circles and triangles, they are a necessary geometrical configuration and when crystals are slipping they have to be present. The second point was that according to the then accepted picture they went out of the free surface of the crystal or they stopped at a mosaic boundary and that was the end of it, and therefore you could only get a finite amount of plastic deformation unless you had many more dislocations in the crystal than seemed reasonable. Also you got the wrong size dependences and so on. So it was necessary to have some other process which either preserved them or kept on manufacturing them. I wanted to deal with polycrystals. So I said in polycrystals these dislocations only move through one grain; they stop when they reach the

grain boundary, but then they pile up and can exert a stress at that boundary and they can start some slip on the other side. So these were the three main points I wanted to make in my paper.

I had produced a solution to the problem of how to preserve and multiply them. If they travel fast enough, if they travel at more than 0.866 of the speed of sound, then they have enough energy to make more dislocations. I said that they move fast, they have to have some considerable kinetic energy and can be reflected at the free surface and come back again. They should have enough kinetic energy to deal with any dissipation when they hit the surface. If they have a lot of kinetic energy, dislocations can collide and not only pass through each other but create another pair of dislocations. That is what was written down in the paper I was taking to Pittsburgh. I had published the idea in the *Proceedings of the Physical Society*.

I arrived with a lot of time in hand. A programme had been planned, whereby I spent a week at the naval research laboratory and then I stopped off and gave a lecture at Cornell. When I got to Pittsburgh I received a letter from Jock Eshelby, which had arrived during my touring. This letter caught me at Pittsburgh, and it said, 'Have you read Leibfried's paper which shows that the speed of dislocations under typical test conditions will not be greater than 0.07 of the speed of sound?' I had not seen Leibfried's paper. I went to the Carnegie Tech. library and said 'Has the *Zeits. f. Phys.* (or whatever it was) come in?' They said no, but there is a parcel in from Germany today, it might be in that, so I waited around and they opened up the parcel and the requisite journal was in it, so I read Leibfried's paper and just had time to go and catch the train. I arrived at Ithaca at a time they did not expect; I arrived there before lunch and they had not expected me until tea time, so they dumped me in my hotel, said 'Have lunch, we have a meeting this afternoon; we are very sorry, can you look after yourself until 5 o'clock?' So I went for a long walk by myself on the Cornell campus that afternoon from 3 o'clock to 5 o'clock. And I had read this paper, it might be wrong, it was not conclusive that he was right, but what do I do if he is right? While I was walking on the campus I had the idea, of course dislocations in the interior of a crystal are not awfully different, geometrically they are the same thing, as the growth step on the surface of a crystal and I already knew that that thing turned into spirals. So I said "Yes, dislocations can wind themselves up into spirals on the slip plane.' So then I gave a lecture at Cornell about crystal growth theory and I went to a party and woke up next morning and went to Schenectady, and sitting on the floor in Orowan's house with John Fisher and a few other people, drinking beer, I said, 'Do you see anything wrong with this idea?' And John looked up and said, 'No, I can't see anything wrong with that.' The following day I rode with the General Electric people in a car to Pittsburgh and we all assembled in a hotel lobby and were being introduced to each other. John Fisher brought Thornton Read. Thornton, as soon as he was introduced to me, said 'Frank, there is something I want to tell you' and John Fisher replied, 'Frank has got something to tell you.' So we started talking and we found that we were telling each other what was in all basic principles the same. So I said, 'When did you think of that?' and he said, 'When I was drinking my tea last Wednesday afternoon about 4 o'clock.' I said, 'I was walking on the Cornell campus from 3 till 5.' John Fisher said, 'There is only one solution to that, you and I must write a joint publication' (Frank & Read 1950).

It was a remarkable coincidence. The preconditions were there, I was preparing for the same conference as Thornton Read and we both had good reasons to focus our thoughts on the same problem and we got them into focus precisely simultaneously.

References

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