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Summary and outlook

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As we are running short of time, I will be brief. I would like to begin by repeating the platitude (or self-evident truth!) that, just as approaches to a certain problem can be different, so can the motivations. A person may be motivated to study a problem for purely aesthetic reasons, and not out of any practical considerations at all; but the person should have the freedom to do so. To paraphrase what has been said by many eminent scientists: If a fraction of time spent in a subjective argument over different approaches to a problem was spent in actual computation using any of the techniques under contention, there would perhaps be no need for the argument because the problem would be considerably closer to resolution! Similarly, I feel that there is no need here to debate the respective motivations of physicists and metallurgists, including the question of what constitutes a problem and what is meant by a solution to one. All techniques, however mathematical or however empirical, are acceptable and indeed essential to make any headway in a subject as complicated as the one we have been discussing.

I would now like to make two general comments, one taken from Prof. N Kumar and the other based on Dr K R Rao's talk. In any given subject, theorists generally like to have the broad paradigms brought out so as to have a framework for further development. The general paradigm that is used in the area of mechanical deformation is of course that of dislocations. Given a paradigm, the theoretical physicist then faces the interesting question of the adequacy or otherwise of the paradigm in the light of subsequent experimental information. A well-known example is that of the soliton which is the "paradigm of the 80's" in several branches of physics. When certain regimes of essential nonlinearity are reached, the traditional plane wave-cum-superposition principle is no longer adequate, and new basic entities like solitons (or more generally 'lumps') have to be introduced. So, in this sense, it is very relevant to ask whether, when we look at a practical problem like a large-angle grain boundary, the paradigm of the dislocation is still meaningful—notwithstanding the fact that in the limit of small angles, the boundary is describable in terms of an array of dislocations. Thus, a radical approach which gives up the idea of dislocations altogether would be worth examining further. Equally welcome are other approaches which bring out geometrical coincidences like the DSC lattice together with the idea of dislocations on *this* lattice.

My second general comment concerns the experimental side. A large number of possibilites were catalogued systematically this morning by Dr Rao. Some techniques like acoustic emission, which perhaps have not been emphasized sufficiently for lack of time, should be explored more systematically. Turning to questions for theoreticians arising out of experiment, consider, for example, spinoidal decomposition and the various nonlinear theories of diffusion associated with it. What light does experiment shed on the cut-off and truncation procedures employed in Cahn's theory and its various modifications to include 'higher order' nonlinearities? What are the experimental inputs needed for a critical discrimination between the various theories—in the manner, for example, critical experimental inputs to the theory of freezing were spelt out by Prof. Ramakrishnan in his lecture? You may recall that he pointed out how the shape of the structure factor really determines how good the approximation is and also serves as a self-consistency check. Something like that would be worth attempting for theories of spinoidal decomposition too. Perhaps this has already been done. I am tempted to recall a remark I came across once—I think it was made by Frauenfelder in the early days when many nuclear physicists were getting into solid state physics *via* the Mössbauer effect—that it is always a big surprise to a person getting into another field to realize that the "natives were not as stupid as they seemed from outside", and that all the obvious things had been done! I am sure all the physicists here have this realization about metallurgy.

There are some other points of interest which I should like to mention, although my list will surely reflect my personal bias as a theoretical physicist. Consider for example the general question of nucleation and discontinuous transitions. The diversity and richness of the phenomena encountered here were well brought out in the talk by Dr Wadhawan. It seemed to me that considerable work remained to be done to get the 'theory' here to the same stage of development as has been reached for continuous transitions. Another point which struck me relates to describing things in terms of disorder rather than order. For example, when one considers systems with quenched disorder, one starts with simple models, but pretty soon when the defect density becomes high, one introduces things like disorder variables. Starting from the high-temperature phase, one can then use duality properties, for instance, and get information on the ordered phase as well as on the defects in the ordered phase. This is more fruitful than attempting to start at the ground state and work one's way upwards. Perhaps when the dislocation density is very high, this kind of approach may be useful—indeed, I should think that recent ideas on 'solid-state turbulence' are preliminary steps in this direction. But a great deal of work remains to be done to 'quantify' the properties of random tangles of dislocations, etc. Starting from the theory of liquids is of course carrying the idea of coming in from the disordered phase to its ultimate, but this may be *too* basic for the purpose I have in mind here. The theory of *freezing* we heard about was aptly named, for it did not deal with the question of how to go from the solid to the liquid phase, *i.e.*, how to restore the broken symmetry. A complete picture of the transition should of course give us an idea of this obverse side of the coin, too.

I would also like to emphasize a point that Dr Ramaswamy made during his presentation. When one tries to apply ideas like chaos to mechanical behaviour (as has been initiated by Dr Ananthakrishna), it would be good to try and obtain first a *model-independent characterization of* the *basic* variables in the mechanical behaviour problem. It is very interesting to demonstrate that chaos appears in specific models of dislocation dynamics. Presumably the phenomenon is much more general. So one would like to know what the minimal set of relevant variables is, and so on, in order to extract model-independent information to the extent possible. This is all the more important in an area like chaos because we know that 'neighbouring' dynamical systems can have widely different behaviour. We should at least be able to put various models of dislocation dynamics into their appropriate 'universality' classes. Only then can one undertake a critical examination of the role of various physical parameters in inducing bifurcations, chaotic behaviour, etc.

Having mentioned chaos, I should also like to make a remark on the slightly older paradigm of solitons, nonlinearities, etc. It may be relevant to ask whether nonlinearities of the kind that arise in the continuum elasticity theory Dr Sahoo talked about, can help to stabilize nondissipative structures like solitons—and if so, what are these? Are they the analogues of dislocations in discrete models? This is no doubt an entirely theoretical kind of question, but I am sure that it would aid progress if we understood the basic reason why these objects get stabilized.

I realize I have essentially been conveying the prejudices of a theorist but that is because I am not really competent to say much about the experimental possibilities. However, I will emphasize that this is a field in which observation is prime. We want facts. However, I should like to heartily endorse a point made by Prof. Kumar. Given the plethora of facts and the very large number of variables involved in the field of mechanical behaviour, physicists generally find the going difficult since they like things to be presented in terms of basic features, or very general arguments and reasons. To snow a physicist with a mass of metallurgical details sounds a bit like French grammar where you list all the irregular verbs and exceptions *ad nauseum*. So, if I may make an appeal—it would be nice for the metallurgists to *intentionally oversimplify* the problems that they face when presenting them to a physicist. *After* he gets interested and starts answering questions, you can 'bring him down to earth', as Prof. Srinivasaraghavan said, by gradually including the complications; otherwise there is a tendency for him to get turned off right in the beginning. This approach will also help sift and place all the facts in order of importance and relevance, in itself a worthwhile and by no means trivial task. Fortunately, in the talks we have had during these three days, this has, by and large, been achieved. Many of us physicists have noted down at least some things that are general features, notwithstanding exceptions, and I am sure some of us will actually explore at least the simplified cases. In that sense, I think, this Meeting has worked out well.