ISSUES AND OPPORTUNITIES IN MATERIALS RESEARCH

Solutions to technologically important problems in materials science—such as pattern formation during solidification—are within reach. Yet, while US researchers may have the tools to find solutions, their opportunities for doing so in the US are slipping rapidly.

James S. Langer

In the fall of 1989 the National Research Council issued a major report on materials science and engineering,[1] known more familiarly as the Chaudhari–Flemings report (after Praveen Chaudhari and Merton Flemings) or simply the MS&E report. This was followed in 1990 by a series of regional meetings in four different parts of the US involving hundreds of participants from industry, academia and government. The results of those meetings were summarized in a 1991 proposal for a "National Agenda" in materials, addressed to the Office of Science and Technology Policy.[2] In turn, this proposal has led to a Presidential initiative for fiscal year 1993 entitled Advanced Materials and Processing.[3]

The result of all this activity is that materials research has been pushed toward—if not yet all the way to—the forefront of national attention in science and technology policy. Unfortunately this is happening at the same time that the rationale for support of science and technology in the US seems to be coming apart at the seams. Although there were strong symptoms of decline in industrial research in the 1980s, few of the authors of the MS&E report anticipated the severity of this trend, and none could have guessed that events in Eastern Europe would so suddenly reduce the perceived urgency of defense research.

Thus major changes are taking place within science itself and in the relationships between scientists, government, industry and the nation. Materials science and engineering—standing as it does with one foot in high-technology fields such as advanced electronics and biomolecular synthesis and the other in the "rust belt" industries—may be the area in which the tensions accompanying these changes are most obvious. The most bewildering aspect of this situation is that while there is growing consensus that materials research is of critical importance to the US, there seems to be no national consensus about who is responsible for supporting this research or seeing that it is carried out in such a way as to be of long-term value.

Materials science and engineering has prospered greatly in the last few decades. As documented in the MS&E report, advances in materials underlie all modern technologies. Every manufacturing industry depends on

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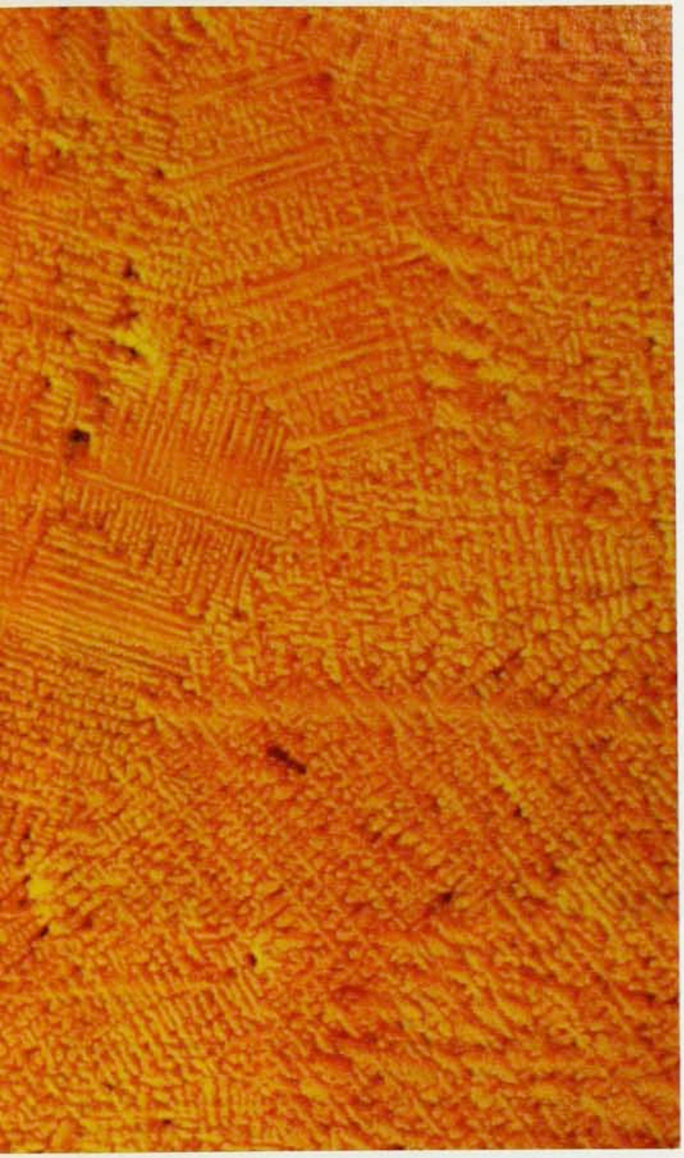


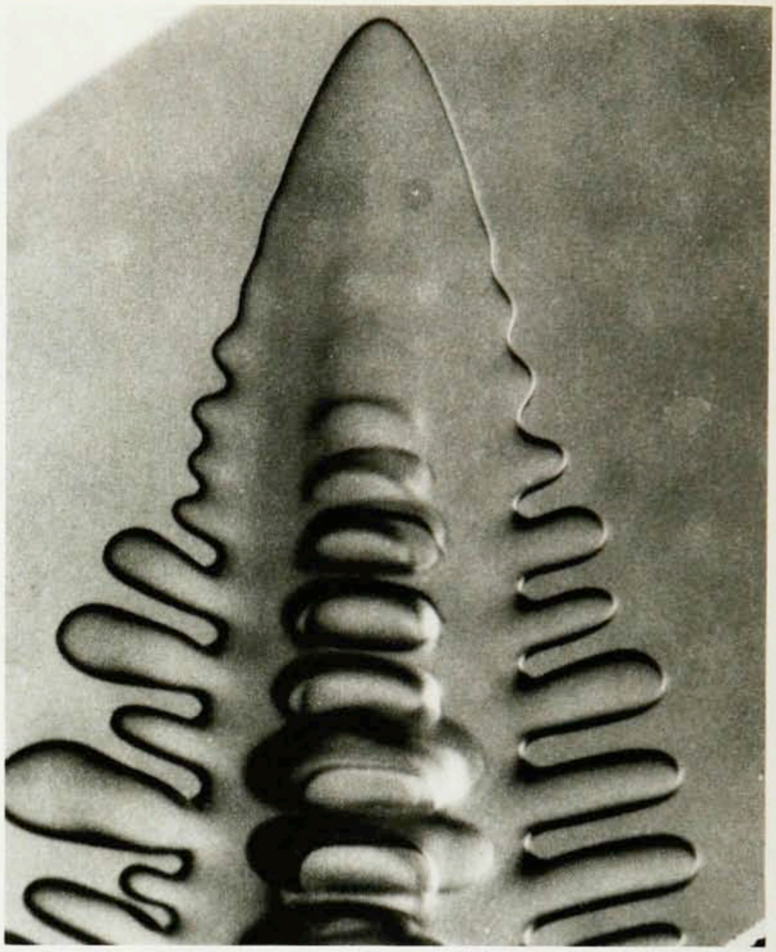
Dendritic microstructures. Left: Micrograph of a cast Cu-Zn alloy (commercial 70/30 brass). In this sample, etched to show its microstructure, the dendrites solidified first, leaving impurity-rich liquid to solidify later in the interstices. The region swept out by each primary dendrite and its array of sidebranches is (very nearly) a single crystal, or grain, whose symmetry and orientation are the same as those of the dendrite. Thus, the dendritic mechanism determines both the grain structure and the patterns of chemical composition within the grains. (Courtesy of J. P. A. Löfvander, University of California, Santa Barbara.) Above: Solidification pattern obtained by quenching a thin film of an initially uniform mixture of two molten salts, CuCl and $PbCl_2$. The $PbCl_2$-rich crystals are dendritic. Because the photograph was taken through crossed polarizers, the birefringent $PbCl_2$ crystals appear as brightly colored regions, each color corresponding to a different crystalline orientation. (Courtesy of J. van Suchtelin, Philips Research, Einthoven, The Netherlands.) **Figure 1**

materials research and development—either its own or someone else's—for the quality and competitiveness of its products. The field also has been remarkable in its intellectual vitality: Major surprises are occurring at a rate of about one per year. A list of recent unexpected discoveries, which would not have seemed credible to a conscientious National Science Foundation or Department of Energy program officer had they appeared prematurely in research proposals, includes the quantized Hall effect, the scanning tunneling electron microscope, high-temperature superconductivity, quasicrystals and Buckminsterfullerenes. The first three of these discoveries have already been recognized by Nobel Prizes.

This double-edged intellectual prosperity—presenting irresistible opportunities in both fundamental research and practical applications—would be fine if there were an unlimited supply of people and resources to take advantage of it or, as John Rowell pointed out in a recent PHYSICS TODAY article (May, page 40), if there were an unlimited market for the products of materials research. However, because we are entering an era of limited funding and uncertain priorities, we have some hard decisions to make. We must develop a new understanding of how large our field ought to be and where professional opportunities are going to be found. As part of this process, we must start to do a better job of selecting research goals and developing strategies for achieving them.

New fields in materials research

The best way to understand the issues facing materials research is to look at a few examples. I have been asked to focus my technical remarks primarily on a part of the field close to my heart—the solidification of metallic alloys and some related topics relevant to the processing of structural materials. Before doing that, however, I want to broaden the perspective of this article by commenting briefly on several other current topics. Materials science is an extraordinarily broad field, and its different components relate to the world in many different ways. Solidification processing is just one relatively small subfield that has its own special opportunities and difficulties. It is not an area of research that ordinarily has been thought to be in the mainstream of modern physics, and unlike most mainstream areas in physics, it has a very long history in applied technology. Thus I think that my later remarks will make better sense if I touch a few extra bases now.



Region near the tip of a growing dendrite in a slightly undercooled and very pure sample of succinonitrile. The emerging solid is a plastic crystal with cubic symmetry. This growth pattern is controlled by the transport of latent heat away from the solidification front. Note the smooth, nearly paraboloidal tip and the emergence of sidebranches with fourfold symmetry about the growth direction. (Courtesy of Martin Glicksman, Rensselaer Polytechnic Institute.) **Figure 2**

Recently high-temperature superconductivity has attracted much public attention. It is clear that the new oxide superconductors are not going to solve all our energy and transportation problems in the next year or so. It seems equally clear, however, that these materials, or others like them, eventually will become technologically very important, and that success in making them useful will go to those who have the most persistence and vision. Low-power applications for sensors and other electronic devices are now beginning to emerge. High-power applications such as magnets, motors and transmission lines await solutions to challenging problems in the processing of the materials, in particular, the problem of preparing these intrinsically brittle ceramics in such a way that they can carry large electric currents and withstand the resulting electromagnetic forces. Today, despite enormous effort by a large fraction of the world's condensed matter theorists, we still lack a fundamental understanding of high-temperature superconductivity, which might be useful in the search for new and perhaps more easily processed superconducting materials.

The most important lesson to be learned from the new superconductors, however, goes beyond the interpretation of their special properties. Their discovery has given scientists an entirely new perspective about the potential advantages of complexity in materials. The high-temperature superconductors contain four or more elementary atomic constituents arranged in intricate crystalline patterns. Until recently, most scientists had believed that such complexity was unlikely to produce qualitatively new behavior. The discovery of these materials has thus broken a conceptual barrier, and researchers throughout the world are now actively examining much broader classes of multicomponent materials in search of new properties and new phenomena.

Another area where the interaction between science and technology ought to be especially strong is research on nanostructures, the artificially structured materials now being explored for use as ultraminiature electronic or photonic devices. The new technical development in this field is the ability to synthesize materials in effect atom by atom. Thus one can make integrated circuits with features so small that entirely new physical principles are needed to understand their behavior. The circuit elements—transistors, capacitors, connecting wires—behave in many respects like quantum mechanical atoms or molecules. The problems that arise both in learning how to fabricate these devices and in predicting their properties are qualitatively different from anything that we have seen before.

A third example is the novel and potentially important class of substances coming to be known as biomolecular materials. There are at least three distinct kinds of activity in this area, each of which seems ripe for growth. At one level chemists and physicists are looking at certain combinations of large, inorganic molecules in solution that tend to organize themselves into complex patterns known as microemulsions or sometimes into objects that resemble membranes or cells. At another level, materials scientists are trying to learn some of the basic principles that govern biological synthesis of naturally occurring polymers and composites. For example, natural abalone shell has properties superior to those of the most nearly analogous man-made material. It might be very useful to be able to duplicate the shell-forming chemical reactions in the laboratory. At yet a third level, biologists, by inserting the proper DNA sequences into organisms, are learning how to induce living cells to build polymers that they do not naturally produce. These lines of research should lead to the production of complex artificial materials that mimic natural substances such as shells or perhaps even muscles and photoreceptor arrays. They also should lead to new, robust, nonbiological materials that imitate some of the "smart" behaviors of living systems.

Each of these three examples—high-temperature superconductors, nanostructures and biomolecular materials—is unquestionably at the frontier of materials research. The fields are new, and because of their novelty, their potential importance for practical applications is both uncertain and exciting. In a technologically competitive world, it would be foolhardy for an advanced society to neglect research in any of them. My main topic, solidification patterns,[4] seems equally important technologically, equally risky as a research investment, but not ostensibly new. In fact, as I shall argue, the emergence of new concepts and new research tools has made this area every bit as urgent as the "sexier" frontier areas mentioned above. But the political and sociological challenges, as well as the scientific ones, that more mature fields present are different in important respects.

Understanding metallurgical microstructures

Freshly solidified metallic alloys—for example, steel, brass or a titanium-based alloy used for jet engines—are made up of individual crystallites, or "grains," that are visible in an ordinary optical microscope. The interiors of these grains may look like a collection of overly ambitious snowflakes. (See figure 1.) This pattern is what is called the "microstructure" of the solid material. Each grain is formed by a dendritic process in which a crystal of the primary composition grows out rapidly in a cascade of branches and sidebranches, leaving solute-rich melt to solidify more slowly in the interstices. The speed at which the dendrites grow and the regularity and spacing of their sidebranches determine the microstructure, which in turn governs many of the properties of the solidified material, such as its mechanical strength and its response to heating and deformation.

Metallurgists have long sought to predict and control alloy microstructures. The development of automated, cost-effective manufacturing techniques ultimately depends on the precision with which we can solve this problem in nonequilibrium pattern formation. In principle, we would like to incorporate fundamental understanding of microstructures into computer codes that will simultaneously help us design materials with made-to-order properties and optimize their manufacturability and performance. Other useful processes in which better understanding of the principles of microstructural pattern formation will be important include the joining of materials—by welding, for example—the modification of surfaces to make them harder or more resistant to corrosion, the growth of semiconductor crystals and a variety of technologies involving nonmetallic materials such as ceramics and polymers.

Some major advances in the microstructure problem have occurred in just the last few years. As so often happens in such situations, however, new insights have overturned preconceptions about the nature of the problem and in some ways may have widened the communication gap between the scientists and the engineers who work on it. Dendritic growth is a case in point.

It has been clear for about 50 years that one starting point for understanding metallurgical microstructures must be a full understanding of the free dendrite (see figure 2), that is, the dendritic solid growing in an undercooled (or chemically supersaturated) liquid, well removed from other dendrites or the boundaries of the container. We know from experiment that the growth rate, the sharpness of the tip, the spacing of the sidebranches and essentially all other features of the free dendrite are determined uniquely by the undercooling (or supersaturation) and not, for example, by the initial shape of the seed crystal or the thermal history of the melt during prior stages of growth. The crux of the theoretical problem is that simple considerations of steady-state heat (or solute) diffusion predict just the opposite—that a wide range of growth modes, varying continuously from slowly growing, thick dendrites to fast-growing, sharp ones, should be equally possible. From a theorist's point of view, the question could hardly be more enticing: What selection mechanism determines the dynamic behavior of this system?

The history of attempts to solve this pattern-selection problem has, I suppose, perplexed and disheartened the metallurgists who have a real need to understand what is going on.[5] In the mid-1970s, Martin Glicksman and colleagues[6] provided experimental evidence to contradict what was then called the "maximum velocity" theory. At about the same time, Heiner Müller-Krumbhaar and I achieved considerable success by hypothesizing that the dendrite grows at a speed for which the shape of its tip is just marginally stable, but we have never been able to find a firm theoretical foundation for that assertion. In the mid-1980s a number of us announced with glee that we had finally solved the problem—that relatively weak, anisotropic surface effects act as singular perturbations and thereby control the growth process in a mathematically very subtle way. The resulting "solvability" theory is almost certainly correct for two-dimensional dendrites or for three-dimensional ones with cylindrical symmetry about the growth axis. Unfortunately there are now reasons to believe that this theory still falls short of providing a complete and accurate picture for real three-dimensional dendrites with fully three-dimensional crystalline anisotropies.

One feature of the newer solidification theories that will certainly remain when we finally know all the answers is the delicacy of these processes. Dendritic crystal growth turns out to be controlled by weak effects that had once seemed negligible. For example, small changes in the surface tension of the crystal or, perhaps, even microscopic temperature fluctuations in the solidifying liquid may determine whether the growing solid looks like a snowflake or like seaweed. The subtle way in which tiny perturbations are amplified in these systems has become an important research topic in mathematics, and it certainly will have to become part of the education of numerical analysts who aspire to write computer codes to improve techniques for casting alloys.

Directional solidification

The dendrite problem has been so challenging that many of us tend to forget that solving it would be only a first step toward a full understanding of practical solidification technologies. The results of an experiment that we might think of as a second-step experiment are shown in figure 3. This is a directional solidification experiment, carried out by Rohit Trivedi and colleagues at Iowa State University,[7] in which an initially flat interface becomes unstable and eventually forms an array of dendrites. The instability occurs when the interface is forced to move by the experimenter's suddenly putting the sample in motion relative to the temperature gradient in which it is sitting.

Directional solidification is an industrially useful process, but Trivedi's experiment was not intended to be an accurate reproduction of any commercial process. Rather, its purpose was to obtain a quantitative understanding of just a few aspects of the process, particularly the spacing of the dendritic array as a function of the pulling speed and the strength of the temperature gradient. The sample was a thin film of an organic substance—the same substance, succinonitrile, that Glicksman used, but with a small amount of impurity added to make the process analogous to directional solidification of an alloy. Great care was taken to make sure that the motion was controlled precisely and reproducibly.

The challenge to the theorist or the would-be code writer is to predict the spacing of the final array. Apparently, for this particular class of experiments, the spacing depends on the way the system is set in motion and not just on the final growth speed. Thus to predict the spacing one must compute how the initially stationary flat interface accelerates in response to the moving temperature gradient, how the local concentration of impurities adjusts to this motion, how the flat interface destabilizes and becomes dendritic, how the dendrites interact with each other and how the dendritic array coarsens and ultimately finds a steady-state configuration. I think that all of this can be done with crude but reasonable levels of approximation; in fact, James Warren and I will report soon on an attempt to do just that. But our calculation, even if successful, still will not be of direct technological interest.

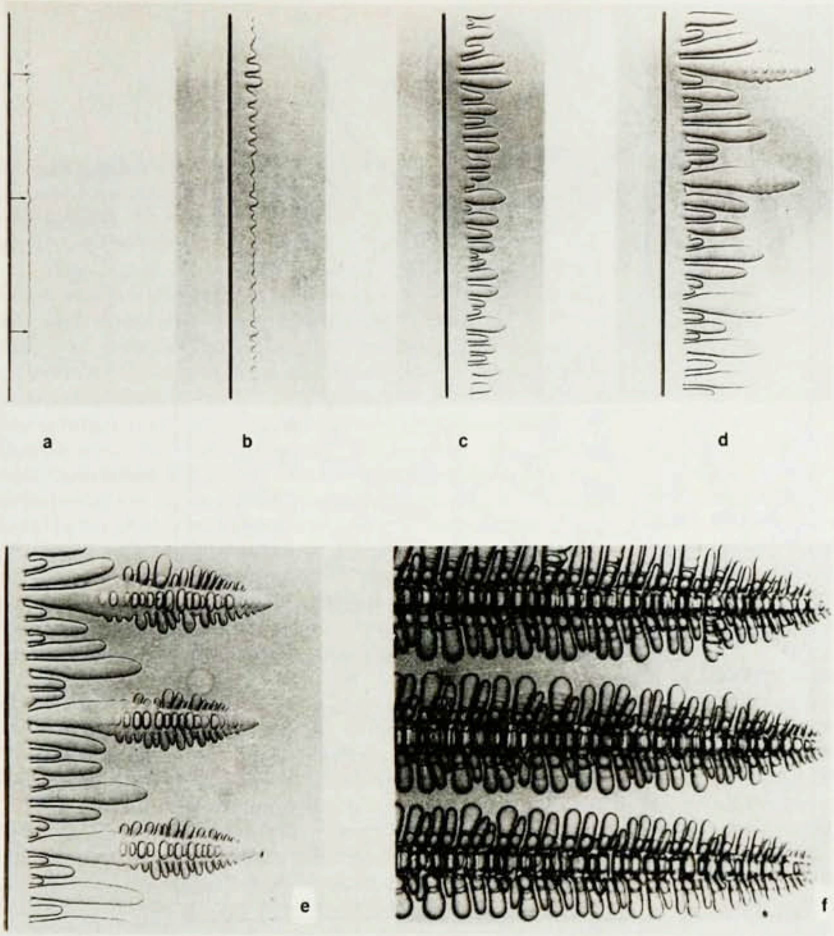
My theorist's caricature of an industrially relevant solidification process, such as occurs in a vacuum arc furnace or during welding, is shown in figure 4. A great deal of energy is added to the system to melt the alloy, so the melt undergoes turbulent convection. Because the fluid has a finite viscosity, the convective flow must disappear in a boundary layer ahead of the solidification front. This front, however, is not a smooth solid surface but rather the locus of the tips of the dendrites where solidification is starting. The region behind this front, composed of dendrites and interdendritic melt, is called the "mushy zone." This region most directly determines the microstructure of the solidified material.

I suspect that, unlike the regular dendritic array shown in figure 3, the mushy zone in a real solidification process is intrinsically chaotic and that therefore the pattern-selection problem is intrinsically different from the one that pertains, for example, to Trivedi's carefully controlled version of directional solidification. My suspicion is based in part on an impression that such processes often operate close to thresholds of instability in which the mushy zone develops pockets of abnormal structure or chemical composition—precisely the kind of defects that must be avoided in high-performance materials. So far as I know, no one has yet tried to understand the dynamics of the mushy zone from this point of view. It ought at least to be possible, with a rudimentary understanding of how perturbations are amplified in chaotic systems, to estimate how precisely one needs to control the growth conditions in this process to control the quality of the finished product. And it may even be possible to make more complete predictions.

Clearly, producing accurate predictive models of processes such as this one will be a truly interdisciplinary endeavor. At the scientific level we need expertise in fluid dynamics, metallurgy, nonequilibrium thermodynamics, nonlinear phenomena and numerical analysis. But that is the relatively easy part of the exercise. We also need the process engineer and the marketing expert to tell us precisely what problems ought to be solved and what kinds of solutions might be useful. That is the more difficult issue—one about which I shall have more to say shortly.

Broader perspectives

Research in metallurgical microstructures is a relatively small and not outwardly visible part of materials science. It occupies one corner of a larger area sometimes called "structural materials," where the term "structural" is used to mean that the materials are the structural elements of anything from electronic devices to ceramic automobile engines or steel beams. The topics of interest are the strength of materials and the way they perform in service—their yield strength and fracture toughness, their resistance to wear and corrosion, and their ability to withstand high temperatures or other hostile environments. My impression is that research in this area is given too little attention in the US, both by scientists and by funding agencies. Real solids, even the most nearly ideal ones, are intrinsically more complicated than most of



Directional solidification of a dilute solution of acetone in succinonitrile in a moving temperature gradient. The initially flat interface between the liquid and the solid (**a**) first undergoes an instability in which an almost periodic pattern of bulges forms (**b**). These bulges then grow into dendrites that crowd one another out and finally settle into a steady-state array (**c–f**). Prediction of the spacing of this array is an outstanding problem in the theory of nonequilibrium pattern formation. (Courtesy of Rohit Trivedi, Iowa State University.) **Figure 3**

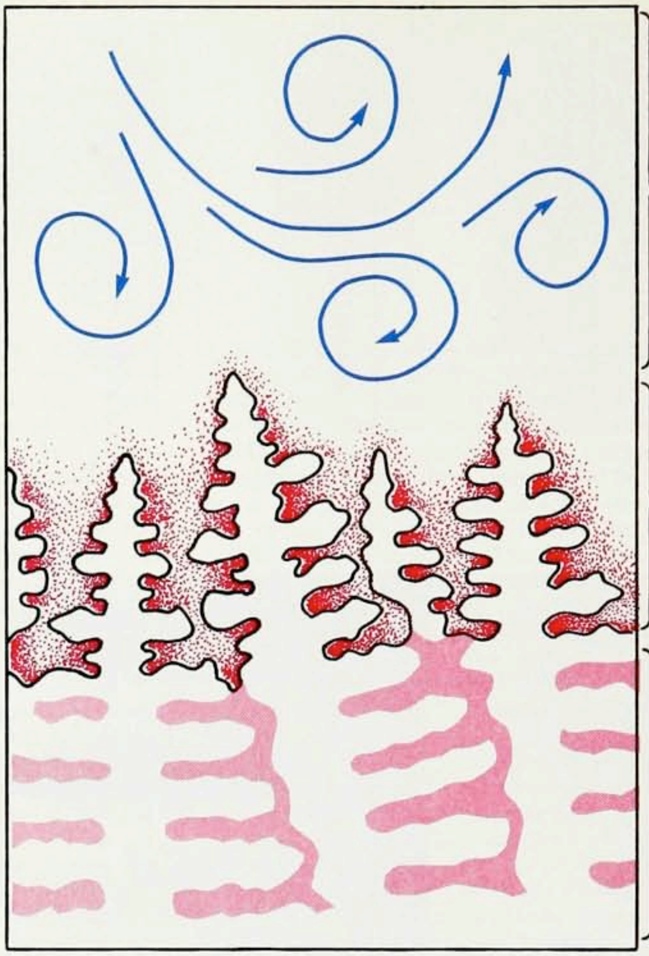
us would like to believe, and we will have to deal with these complications if we are to achieve new levels of performance. As in the microstructure problem, many of these complications take us to the forefront of modern mathematics and science. For example, to understand fracture, adhesion or friction, we certainly shall need to learn more about the molecular bases for these phenomena, but we may also need to understand more about chaotic systems and fractal geometries.

There is an even broader perspective from which it is interesting to look at the microstructure problem. The conceptual underpinnings for much of our modern understanding of phase transformations such as solidification have been taken over as paradigms in elementary-particle physics and cosmology. In fact, much the same mathematical equations are currently used to describe both pattern formation in crystal growth and "symmetry breaking," the origin and distribution of elementary particles, in the early universe. I wonder whether the surprises that we have encountered in the solidification problem, where we are perpetually being kept honest by abundant and relatively inexpensive experimental data, might be bad news for the cosmologists. I also find it sobering to realize that the cosmologists who are working at the furthest frontiers of natural philosophy have so much in common with the engineers who are trying to improve the manufacture of engine blocks or brake drums.

Interface between science and technology

One conclusion to be drawn from this discussion is that while materials research has made great strides in changing from an applied art to a quantitative science, the next steps in this transition will be difficult. We have the necessary tools to take those steps: laboratory instruments that can make measurements with atomic-scale precision and computers that provide previously undreamed-of capabilities for analysis and modeling. We do not at present have adequate, thoughtfully planned Federal support for materials research in the US, and my next remarks will touch briefly on that situation. But the main message that I want to transmit is that our toughest long-term challenges have to do with our institutions and our priorities.

We face a dilemma today, one that we have lived with for many years, but which never before has seemed so striking or urgent. The problem has much to do with "technology transfer," but that term understates the issues involved. If quantitative, predictive solutions of a wide range of practical and complex problems have suddenly come within our reach, then what people and what institutions do we call upon to find them? For example, if finding a qualitatively new solution to some manufacturing problem requires a combination of atomic-resolution microscopy, nonequilibrium statistical mechanics and process engineering, how are we to arrange that the appropriate facilities and skilled people are brought together effectively? How are we even to make sure that the key participants recognize the desirability of working together on this problem and that they are motivated to do so? And, most critically, how do we arrange that some company or agency has the capital and persistence to see the work through to completion?



Solidification of an alloy in an industrial process such as vacuum arc melting or welding. At the top of the picture, at some distance from where solidification is taking place, the molten material undergoes turbulent motion. A "mushy zone," consisting of dendritic crystals and interdendritic melt, lies between the fluid and the fully solidified region. Some chemical constituent of the alloy is concentrated in the interdendritic regions (red) and ultimately is segregated in a dendritic pattern in the solid. Unlike the two-dimensional array of dendrites grown under carefully controlled conditions (figure 3), these dendrites grow in many directions, both in the plane of the picture and perpendicular to that plane. As a result, both the mushy zone and the microstructure of the final solid are highly irregular. **Figure 4**

These questions seem to me to be more urgent for mature industrial technologies such as solidification processing than for new fields. In new areas, it is possible to start small efforts, find venture capital and avoid competition with established interests. By contrast, in mature areas the very basic coexists with the very applied, the old coexists with the new, and it is often extremely difficult to understand when a new material or processing technique or even a new conceptual point of view can displace established ways of doing business. Moreover, it is not just lack of vision or pigheadedness that causes US manufacturing industries to resist introducing new materials or advanced processes. The cost of introducing new technologies in this country is enormous: Capital is expensive, licensing can be risky and time consuming, and if the product is truly novel, the materials manufacturer is exposed to a variety of legal hazards.

Nevertheless it seems obvious that in a free-market system private industry must play a leading role in materials research and development. This is not to say that industry must support an enormous amount of nondirected basic research, but that it should support enough basic research to keep the system working. Industry must take some responsibility for enunciating fundamental problems that need to be solved, and it must remain in a position to take advantage of scientific developments as they occur. We also must be able to count on industry to provide professional opportunities for scientists and engineers. One of this nation's most successful innovations has been the coupling of advanced research to education at our universities. If the need for scientists and engineers in industry declines—and this will certainly happen if we continue to lose manufacturing industries—then advanced research at universities will decline as well. Thus the health of manufacturing industries is of overwhelming importance for materials research, and for US science and technology in general.

A second necessity is energetic leadership by Federal agencies. Here the dilemma seems most acute. The logical conclusion of the argument so far is that more basic research needs to be motivated by applied problems—in the jargon, it needs to be "pulled" by technology. But the Federal role in applied research, especially in commercially relevant applications, has always seemed controversial. In his famous 1945 report "Science, the Endless Frontier,"[8] Vannevar Bush stated explicitly that the principal responsibility of the agency that was to become the NSF should be to support basic rather than applied research. Indeed, his reason for recommending the establishment of a new agency instead of, say, leaving responsibility for research in the hands of the Army or some other Federal department was that "research is the exploration of the unknown and is necessarily speculative. . . . It cannot be satisfactorily conducted in an atmosphere where it is gauged and tested by operating or production standards."

Nearly half a century later we have come to depend almost entirely upon the Federal government for support of basic research. It has become a *de facto* Federal responsibility to insure that the nation has an adequate number of well-trained scientists and to sustain innovative research that delves deeply and takes risks. Whether recent governments in Washington have taken this responsibility seriously enough is a matter of some debate these days. There can be no doubt, however, that the

Reagan and Bush Administrations have tried vigorously to avoid becoming involved in commercially applicable technology, to the extent that they have disavowed anything that could be called an "industrial policy."

This hands-off interpretation of the role of government in a free-enterprise system has put both industrialists and scientists in an awkward situation. If solving many of today's important technological problems requires new ideas and deep understanding of fundamental principles, then it would seem that the government also has a major responsibility in applied research. Vannevar Bush's concern would be that in the face of pressing national needs, the government might adopt a too narrow interpretation of its mission in applied research—that of looking for short-term solutions using whatever means are available rather than probing deeply enough to develop fundamentally new technologies. As we have seen, the opposite mistake has been equally damaging. The inability of our government to help US industry be economically competitive is having disastrous results.

A wish list

What is to be done? I would like to look toward the future in these remarks, so I shall conclude not with recommendations or predictions, but with a brief, personal wish list.

To start, I hope to see a turnaround in the decline of US manufacturing industries. I particularly would like to see the day when graduate students in condensed matter physics can once again look forward to industrial careers. Materials research can play an important role in industrial revitalization and indeed will be essential for it. But the real problems are not the kind that are going to be solved primarily by scientists or by the Federal funding agencies, and I wish that our policymakers would take this fact into account when setting goals for research initiatives. A major Federal effort to work with US industry, perhaps through the government laboratories, would be much more to the point.

I hope that the Federal funding agencies can find a more coherent way to deal with the whole of materials science and engineering. I do not know whether this requires organizational changes, as some of my colleagues have recommended. But it does mean that the engineering programs should be more supportive and understanding of the science base, and vice versa.

More generally, I would like to see our Federal agencies and the scientific and industrial communities together develop a strategic, goal-oriented approach to materials research. In the "National Agenda" report to the Office of Science and Technology Policy,[2] my colleagues and I strongly urged such an approach and made some specific suggestions about how it might be implemented. Our concern was that in a time of limited resources the nation can no longer afford a system in which it decides first on politically feasible projects—a space station, for example—and later figures out how to use them for scientific purposes. It turns out to be easier than one might suppose to identify goals for materials research and to recognize that achieving those goals requires coordinated use of facilities and concerted efforts by scientists and engineers in a wide range of institutional settings.

One especially urgent but, sadly, unrealistic wish: I would like to see a more rational approach to support for science in the US Congress and a less adversarial relationship between the funding agencies and the scientific community. We desperately need *somehow* to control the competitive pressures and restore a sense of inquiry and adventure to scientific research. For many reasons—Congressional earmarking and misunderstanding of priorities among the most important—there is a real crisis of confidence throughout all of science in the US. This crisis is particularly acute for young men and women who should be given a chance to devote their energies to innovative, interdisciplinary materials research. Tight funding in our system inevitably enforces conservatism in research, and it takes unusual courage for even well-established scientists to risk unfavorable reviews of their work by tackling hard problems or by crossing disciplinary boundaries. The Federal agencies, and those of us who are asked to advise them, have a special obligation to set a positive tone for materials research by being open-minded and supportive of new topics and uncommon points of view.

Finally, I hope that the rate of important, surprising discoveries in materials research will continue unabated for the indefinite future. The field is so rich and so wide open for both conceptual and practical advances that at least one truly amazing development each year has come to seem like the normal state of affairs. So long as we persist in making new materials, exploring their properties and discovering how to predict and control materials processes, this part of science and technology will be a continuing source of excitement.

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