IN RESEARCH, FAILURE IS THE PARTNER OF SUCCESS

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Failure is one of the most common difficulties that researchers face—
and the one least talked about.

The movies have always portrayed research as a glamorous and thrilling enterprise, one in which inspiration and happy persistence have wonderful results, results that bring the researcher fame and glory, all within an hour or two.

But you don't hear very much about the real human difficulties and discouragement that are such an inevitable part of research. And the result of this glamorization is that many gifted young people, who might have made valuable contributions, fail to do so.

You may say that portraying research as only exciting should attract more people to it, and of course you're right. And as a result, a great many bright young people want to do research because they've seen countless movie and TV scientists doing such exciting work.

As a matter of fact, I'm embarrassed to admit that's the way I started out myself, as a boyish admirer of Boris Karloff and Frankenstein.

The most ambitious of these young people are eager to remake the world, to make a major contribution that will benefit mankind. But since they start out unaware of the real difficulties, after a few failures they feel they don't fit the image of a researcher. They don't understand that research is mostly failing and failing again, only occasionally followed by success.

In our success-oriented society, one might say that real research is the hardest occupation, since it consists mostly of failing.

The book *In Search of Excellence* shows us that failure occurs frequently; it observes that the wise research manager will support a lot of far out projects and expect most of them to fail. It points out that only about one in 20 projects make it to the marketplace.

I have seen a number of people whom I thought would make good researchers, but who quickly join projects that have easier, more certain goals, projects that are very likely to succeed. I think they do so because they just can't stand the strain of repeated failures, the damage to their self-esteem that comes from the

disparity between their inflated image of what research should be like and the often harsh reality of actually doing research.

I believe that this tendency to quit research, which I see so much of, is a problem that managers should be more concerned about, because it results in the loss of much talent.

In Search of Excellence repeatedly observes that encouraging people to be innovative often has really astounding results, that many people you might not expect to be creative turn out to make really valuable contributions.

I think it is important to have a policy that teaches the research community about the human difficulties of doing creative work, one that provides encouragement and support in the face of repeated failure. I believe that such a policy will produce a great improvement in productivity in the research area in the same way that the policies of excellent companies produce innovation, "product champions," and a general ferment of creativity and productivity.

The purpose of such a policy is to make sure that people don't feel like failures just because they're failing a lot.

One way to help take the sting out of failing is to make people aware of its benefits. For example, in the course of a long but unsuccessful effort to solve a problem, you generate new ways of attacking the problem and, in addition, a lot of other useful ideas. These methods and ideas often turn out to be valuable later on, in other projects.

These ideas left over from a failure get added to what you might call your "junkyard of ideas," and this junkyard is important. A mechanic rummages in a junkyard of leftover bits and pieces for a part to use in the new machine he's building; in the same way a researcher rummages through his junkyard of leftover ideas for one that will help him with his current problem.

In this way a researcher's junkyard becomes his most important resource for solving new problems, and much of it is the result of past failures.

To make some of these ideas less abstract, let me cite some of my own extensive experience with failing. I've been doing research at IBM for about 33 years. During

John Backus is an IBM Fellow at the San Jose Research Laboratory of International Business Machines Corporation, San Jose, California. His paper was presented at the Industrial Research Institute Fall Meeting, November 1983, where he received the IRI Achievement Award.

the first ten years I had a few successes among many failures. The successes included my part in the Fortran project and in developing a language for describing the syntax of other languages (plus a few other less significant ones).

The failures included an earlier programming language (which was well received by users but which I consider a failure) and an unsuccessful effort to describe the meaning of programming languages. I also had the usual quota of failures that precede the solution of any problem.

During my second ten years I worked on a mathematical problem that I thought had practical, as well as theoretical importance. I worked very hard during those ten years, and kept proving enough theorems to keep me thinking I was making progress. But in the end, I had to face the fact that I wasn't going to get the results I had hoped for, that I just had to quit.

I acquired a sizeable junkyard of ideas during those ten years, but I had little to show for my effort beyond a huge stack of notebooks full of mathematical proofs. IBM's Research Division supported me (with a bit of understandable reluctance) during those ten years, but that failure was really painful. I had enjoyed the work very much, but it is hard to describe the feeling of discouragement, even despair that I felt as I filed away those notebooks for the last time.

That's the way my most recent 13 years of research work began. These have been devoted to finding a completely new way of writing programs, so that people will find it very much easier to tell computers what they want them to do. After about eight years of work in this area, I had some success in finding such a new way of writing programs, and a new concept of what a program should be.

The point I want to make about this work, for which I have good expectations, is that I couldn't have started it without the junkyard of ideas that came from the earlier ten years of failure.

Over the last five years my friends and I have had a lot of both successes and failures in our efforts to produce a practical programming system based on this work. We're not there yet, but we're still making progress.

This last period has been difficult, since we have a feeling of being so close, and yet so far, from the goal we have sought for so long. The hardest part is beginning all over again with the 27th new approach, getting the courage and the energy together to run at the barrier once more, and possibly to fail once more. I should say here that now I think we're really on the right track, but then of course I've thought so before!

I'm sure that many of you have had similar difficulties and discouragements in your own work.

The point I want to make about my own experience is this: first, it illustrates how painful it can be to realize that you have failed in a long-term effort, even though you enjoyed doing the work while it lasted, and it illustrates how difficult the day-to-day trying and failing can be as you get closer to a long-sought goal. Second,

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it shows, I think, that we get much more out of such failures than we are led to believe; that in my case, the junkyard of ideas from my ten-year failure contributed crucially to my present work.

In looking back on my own long-term failure, I feel that I was helped to get over it, and keep doing research, by the recognition I had received for the earlier Fortran work. And of course there is also the fact that I just like doing research, trying to find orderly solutions to problems.

But what would happen to someone who has such a failure earlier in his (or her) research career, someone who has no recognition to support him, who has been taught that success is everything? How would his manager treat him? And most importantly, how would he regard himself? Would he keep trying—would he be allowed to keep trying, or would he tend to leave research?

Given all the psychological hazards of doing research that I've mentioned, and the loss of good researchers because of these hazards, what can research management do to help? I think there are quite a few things that can be done.

If we accept the fact that most research projects will fail, but we realize that each failure contributes to our ultimate successes in many ways, then we can see that managing research is largely the business of not only managing failure, but of encouraging it.

Now of course I don't mean by that that you should tell your troops you'll be glad if they don't succeed. What I'm talking about is the creation of an atmosphere, a set of values about research work, in which success is prized, but also one in which failure is recognized as the norm. After all, it actually *is* the norm, and yet we act as if it were not.

Therefore, if research management gives researchers the feeling that *success* is the norm, rather than failure, then success will *become* the norm, that is to say, research will stop, and product development will begin. Because people will stop taking chances.

Earlier, I was trying to describe how painful real research can be, how hard on one's self esteem it is to fail for so long and so often, and how *many* people leave research, because it hurts their self esteem too much to do it.
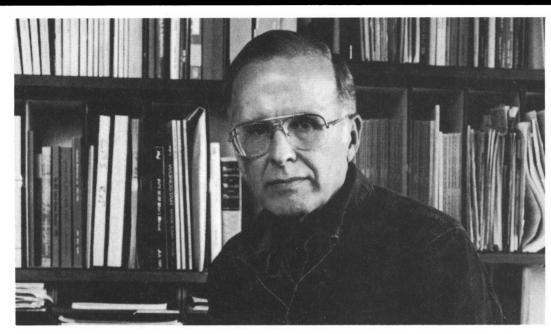
If you accept that, then it seems that here is the ideal   **27**

PRESENTATION OF THE
INDUSTRIAL RESEARCH INSTITUTE ACHIEVEMENT AWARD

TO JOHN BACKUS

For his leadership and central contribution in the development of FORTRAN, the first commercially successful high-level computer programming language, making it feasible for computers to produce their own machine-language instructions, thereby substantially reducing the cost of programming. This development made possible countless new uses of computers for scientific, engineering and mathematical computations and opened the way for widespread use of computers into every fabric of our society, permanently changing the personal and business life of humankind.

John Backus was born in Philadelphia, Pennsylvania in 1924. He received the Bachelor of Science degree and Master of Science degree in mathematics from Columbia University. Upon completion of his schooling he joined IBM in 1950 to program scientific problems on IBM's first high-speed, vacuum-tube machine, the SSEC. This experience in machine-language programming interested him in the question of programming efficiency, and he led development of the "Speedcode" language for the IBM 701, the first large binary computer produced in quantity. Speedcode was an interpretive programming system that made the one-address, binary, fixed-point 701 hardware look to the programmer like a three-address, decimal, floating-point machine.

Mr. Backus next became the central contributor and leader in developing a programming language and compiler for the IBM 704. The result was FORTRAN, the first commercially successful high-level language and still the most widely used language in engineering and scientific computation. By making it possible for computers to produce their own machine-language instructions, FORTRAN substantially reduced the cost of programming, and opened up countless new uses of computers in diverse fields.

This development had a profound impact on the use of computers by the scientific and engineering communities. By drastically simplifying computer programming with little or no loss in machine efficiency, FORTRAN permitted nonprogrammers to make direct use of computers, rather than having to work through professional programmers. The immediate and widespread acceptance of FORTRAN convincingly demonstrated the importance of high-level languages for the future development of computing and provided a powerful stimulus to work on high-level languages for other areas of data processing.

In 1976, Mr. Backus received the National Medal of Science. He is also the recipient of the A. M. Turing Award from the Association of Computing Machinery and the McDowell Award and the Computer Pioneer Award from I.E.E.E.

area to use the lessons that *In Search of Excellence* has taught us. That here, in research, is the place where encouragement, helping people to feel good about themselves, can have the greatest payoff. The message of the book is: that the more people you can motivate to try, the more they will find a way to succeed, and that you will be surprised at how many people do succeed.

So the question becomes: how do we get more people to do research? And that depends on creating the right environment, one that gives good efforts a lot of credit, one in which managers let a researcher know they are pleased to see him persisting in working on an important, hard problem, even though he may have failed with 14 different approaches.

I think some researcher managers feel that they have to be careful about letting just anybody do research, that if you let anyone who wants to do it, do so, that then you will have a lot of inept people wasting a lot of time.

My own feeling is quite the reverse. I thinks lots of people who could do very good research don't try because it's too painful to fail. These people need lots of encouragement; we need to help them be comfortable with failing. People who can't do good research usually won't want to try, and if they do, they will soon ask for more down-to-earth work, after a few failures.

Of course managing research is much more complicated than the picture I'm drawing. There are many questions the manager must ask before he supports a piece of research: Is it a good problem? Is it relevant? Is it suited to the style of the researcher? Thus if you're looking for a breakthrough on a hard, unfamiliar problem, you might want someone more on the creative, rather than scholarly end of the spectrum. On the other hand, many important problems will need a scholarly approach.

But I think one lesson of *In Search of Excellence* is that the *motivation* of the researcher should generally take precedence over these other questions. The main question is: how interested is he in the problem, how much does he really want to understand it, will be he likely to stay with it through difficult times? If so, we should let him work on it, even though we're not convinced he can succeed.

But let me return to the question of how to get more people to try to do research. What specific things can managers do to create an encouraging, failure-supporting environment? It seems that the main thing is to bring failure out of the closet, to make failure-after-a-good-try respectable. We can help to do this in a number of ways.

First, when we give out an award for success, perhaps we can bring out at the same time that we're rewarding not only the success, but all the failures that preceded it, and the contributions they made to the final success, that in part we're rewarding effort as well as success.

As things stand now, awards give too much the impression that success is the norm, that the recipient has only had successes.

Another thing we can do is discuss, in research news articles, ongoing, but not yet successful work, and talk about the courage and persistence needed in pursuing it, the difficulties people have encountered.

Perhaps we might even give awards for long and creative efforts that fail, for persistence and the variety of approaches that are tried.

All of these policies will help make failure respectable and give it its rightful place as the essential partner of success. They will help to give young people a more realistic image of research and help them to be more comfortable about their failures. After all, the pain of failure is determined by your attitude; if you despise it, the pain will be intense, if you accept it, the pain will be less.

These policies will create an atmosphere in research in which everyone can share their discouragements as well as their triumphs, in which people feel closer to one another because each understands that his neighbor suffers the same trials that he does.

And these policies will help us to give the media a more realistic picture of research, one that is much more interesting than their cliches, perhaps even more romantic. After all, research can be wonderfully exciting in reality, even more than in the movies, when we realize that we can get great pleasure out of working on something, no matter whether it succeeds or fails.

So it is very important to prevent success-worship from undermining the pleasure of just doing research. Therefore we must recognize failure as the partner of success. □