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Priorities and Posteriorities

Adapted from *The Effective Executive*
By Peter F. Drucker

There are always more productive tasks for tomorrow than there is time to do them and more opportunities than there are capable people to take care of them—not to mention the always abundant problems and crises.

A decision therefore has to be made as to which tasks deserve priority and which are of less importance. The only question is which will make the decision—the executive or the pressures. But somehow the tasks will be adjusted to the available time and the opportunities will become available only to the extent to which capable people are around to take charge of them.

If the pressures rather than the executive are allowed to make the decision, the important tasks will predictably be sacrificed. Typically, there will then be no time for the most time-consuming part of any task, the conversion of decision into action. No task is completed until it has become part of organizational action and behavior. This almost always means that no task is completed unless other people have taken it on as their own, have accepted new ways of doing old things or the necessity for doing something new, and have otherwise made the executive's "completed" project their own daily routine. If this is slighted because there is no time, then all the work and effort have been for nothing. Yet this is the invariable result of the executive's failure to concentrate and to impose priorities.

Another predictable result of leaving control of priorities to the pressures is that the work of top management does not get done at all. That is always postponable work, for it does not try to solve yesterday's crises but to make a different tomorrow. And the pressures always favor yesterday. In particular, a top group which lets itself be controlled by the pressures will slight the one job no one else can do. It will not pay attention to the outside of the organization. It will therefore lose touch with the only reality, the only area in which there are results. For the pressures always favor what goes on inside. They always favor what has happened over the future, the crisis over the opportunity, the immediate and visible over the real, and the urgent over the relevant.

The job is, however, not to set priorities. That is easy. Everybody can do it. The reason why so few executives concentrate is the difficulty of setting "posteriorities"—that is, deciding what tasks not to tackle—and of sticking to the decision.

Most executives have learned that what one postpones, one actually abandons. A good many of them suspect that there is nothing less desirable than to take up later a project one has postponed when it first came up. The timing is almost bound to be wrong, and timing is a most important element in the success of any effort. To do five years later what it would have been smart to do five years earlier is almost a sure recipe for frustration and failure.

That one actually abandons what one postpones makes executives shy from postponing anything altogether. They know that this or that task is not a first priority, but making it a posteriority is risky. What one has relegated may turn out to be the competitor's triumph. There is no guarantee that the policy area a politician or an administrator has decided to slight may not explode into the hottest and most dangerous political issue.

Setting a posteriority is also unpleasant. Every posteriority is somebody else's top priority. It is much easier to draw up a nice list of top priorities and then to hedge by trying to do "just a little bit" of everything else as well. This makes everybody happy. The only drawback is, of course, that nothing whatever gets done.

A great deal could be said about the analysis of priorities. The most important thing about priorities and posteriorities is, however, not intelligent analysis but courage.

Courage rather than analysis dictates the truly important rules for identifying priorities:

- Pick the future as against the past;
- Focus on opportunity rather than on the problem;
- Choose your own direction—rather than climb on the bandwagon; and
- Aim high, aim for something that will make a difference, rather than for something that is "safe" and easy to do.

A good many studies of research scientists have shown that achievement (at least below the genius level of an Einstein, a Niels Bohr, or a Max Planck) depends less on ability in doing research than on the courage to go after opportunity. Those research scientists who pick their projects according to the greatest likelihood of quick success rather than according to the challenge of the problem are unlikely to achieve distinction. They may turn out a great many footnotes, but neither a law of physics nor a new concept is likely to be named after them. Achievement goes to the people who pick research priorities by the opportunity and who consider other criteria only as qualifiers rather than as determinants.

As a rule it is just as risky, just as arduous, and just as uncertain to do something small that is new as it is to do something big that is new. It is more productive to convert an opportunity into results than to solve a problem—which only restores the equilibrium of yesterday.