What is a good theory?

To begin with, Jaynes (2003: 327) emphasizes that "a theory" isn't just some hypothesis, easily made up and rapidly discarded, or, in the worst case, simply an (educated) prejudice. Quite the reverse is true. A good theory is as helpful as it is hard to create. It uses suitable concepts, combines them in a nontrivial way and succeeds in predicting interesting phenomena. Speaking as a physicist, he says that, "having created this knowledge of physical law, we have a vested interest in it and want to see it preserved and used." It is no coincidence that he bashes Popper to whom progress first of all consists in the falsification of hypotheses.

Standard criteria

The properties of good theories that are most often named are explanatory and prognostic power. That is quite obvious: On the one hand, you use a theory to understand what is "really" (i.e., on a more fundamental level, often invisible to the naked eye) going on; on the other hand you use a theory to predict what will happen – given the relevant circumstances and conditions. To understand and to predict is often tantamount to having an elegant model that accounts for all relevant facts. Although it is rather difficult to understand the formalism of quantum chemistry, it often suffices to combine intuitively electrons, ions, atoms and their ilk according to simple rules (deduced from this formalism). Thus, one may classify chemical reactions and figure out what the product of a certain chemical process should be (given certain conditions like temperature, acidity, pressure, etc.).

Combining the external merits of a theory with its internal elegance almost immediately leads to the characterization that a good theory should maximize the ratio of predictive power and model complexity. A classical way to express this can be found in Feigl (1970a: 12), see Saint-Mont (2011: 481): "It is the fact-postulate ratio that represents the explanatory power of theories. The aim of scientific explanation throughout the ages has been unification, i.e., the comprehending of a maximum of facts and regularities in terms of a minimum of theoretical concepts and assumptions."

Of course, classical physical theories were the forerunner of all this. With just a few, very natural, assumptions, Newton, Einstein and their colleagues were able to account for an enormous number of physical facts, to explain how they came about and were able to predict what was going to happen. Also, Einstein's proverb that one should "make everything as simple as possible, but not simpler" fits in nicely here. Motivated by such ideas, I use a rather primitive Input-Transformation-Output model to characterize the modern economy and its probable future in Saint-Mont (2013). Also, a look at an entrepreneur, a money lender and their environment is sufficient to explain why capitalism has worked excellently during the last few centuries.

Extended criterion no. 1: Real applications

Laughlin (2006: 83) goes further: "Nevertheless the invention of the original transistor set the standards of our discipline, and it still leads most of us to consider the highest achievement in science to be the rendering of facts down to their essentials so effectively **that some practical invention becomes possible**" (my emphasis). That is, if you have (at least partially) understood a class of phenomena, i.e., if you possess a good theory, then you should be able to use this knowledge in order to create something remarkable that really works. That's the idea of science-based technology, present in almost every non-trivial product around you. But, as a matter of fact, it has been around ever since the advent of Western thought and experiment: Renaissance man Leonardo da Vinci not only painted and organized the festivities of Francis I, but, based on physics and experience, he also constructed innovative machines and earned money with ballistic calculations. Already the Ancients knew enough about chemistry to invent and apply "Greek fire" that sent their enemies' fleets to the bed of the ocean.

A major problem of contemporary science consists in its non-applicability. So Laughlin and a growing number of physicists are increasingly sceptical of string theory. This theory has been around for decades; it is rather elegant and although it has been elaborated mathematically in some detail, no empirical test is in sight, let alone any technical application. Or look at new strategies in medicine. Although almost every new chemical substance is tested for its pharmaceutical potential, innovations in that area have become sparser and more expensive. Despite the fact that there has been a lot of fundamental research on pathogens (e.g. problematic substances, viruses, bacteria etc.), biochemistry (e.g. cloning) and mechanisms fostering common diseases, progress in therapeutic applications has been rather disappointing. In other words, although we know a lot more about cancer than one or two generations ago, say, survival rates have not surged accordingly. Therefore "bench-to-bedside" has become a popular slogan, and there is a general initiative on "translational medicine" which is aiming at "transferring findings into diagnostic tools, medicines, procedures, policies and education" (see en.wikipedia.org/wiki/Translational_medicine).

Extended criterion no. 2: Problem-solving power

Bandura (2008: 121) completes the argument: "The value of a psychological theory is judged not only by the usual criteria of explanatory and predictive power. In the final analysis, its worth is evaluated by its **operative power to solve problems** and provide reliable guides for effecting personal and social change" (again, my emphasis).

Thus, more generally speaking, there is the "output side" of a theory, in particular, remarkable applications - be they nuclear power plants or "social change" - and there is the "input side": problems of some practical importance that could not be tackled satisfactorily before the development of a certain theory. Typically, these problems are difficult to grasp, they have many facets, they are complex and cumbersome, far from trivial and intimately connected with other poorly understood fields. So straightforward approaches are either doomed to fail or solutions are of a limited value, often restricted to a particular situation.

A lot of work has to be invested in order to develop appropriate **general** concepts, ideas and methods. However, having done so, there is a good chance that these elaborated strategies – at times unified in a general theory – save the day. That is, upon encountering a new problem you don't have to start anew. Often, merely a modicum of creativity combined with a lot of consistent, methodologically sound work suffices. Utilizing a good theory is like travelling on a paved road: it provides a straightforward, systematic way towards a destination or goal. In the end, applying and extending a successful theory in a reasonable way proves to be much more efficient than trying to find one's way through the jungle without it or on one's own.

If a theory can be couched in mathematical terms, matters become much clearer and thus easier to handle. Often, to paraphrase Laplace, common sense then just boils down to calculation. Jaynes (2003: 268) explains in more detail: "Therefore we shall continue expounding the systematic calculation methods, because they are the only ones which are guaranteed to find the solution. Also, we try to emphasize general mathematical techniques which will work not only on our present problem, but on hundreds of others [...] For us, as for Euler, these are the solid meat of the subject, which make it unnecessary to discover a different new clever trick for each new problem."

Altogether “there is nothing more practical than a good theory" (Lewin 1952: 169). A good theory describes, explains, and has considerable prognostic value. With a minimum number of assumptions a stupendous number of results can be derived and understood. But more than that it rather straightforwardly leads to amazing technique and remarkable applications. Finally, when confronted with a difficult real-world problem, it is a powerful tool, a potent method for solving it.

Epilogue

May I add that the above is another reason why I am sceptical of qualitative methods? Many popular qualitative methods (grounded theory, conversation analysis, objective hermeneutics, the analysis of biographies, case reconstruction…) in the social sciences aim at analysing verbal statements (interviews, written accounts etc.) and behaviour. (Obviously, it is even more difficult to get hold of the non-verbal layer or even non-observable processes like “implicit memory”.) But even if one admits, for the sake of argument, that they by and large succeed, i.e., that qualitative lines of inquiry typically lead to reasonable, non-trivial results, the data these methods are based on are texts, written or verbal accounts, occasionally video sequences. The same with the results: They may be keen-eyed, fine-tuned, thoughtful, inspiring and excellently written. Nevertheless, one always remains on the verbal layer. Like (most) verbal input, the output is also just words: often helpful, at times misleading, but almost never sharp enough to break through the surface of the phenomena.

Let us look at a concrete example: It is a good idea to talk to a depressive person, and to reason that person out of his or her disease. That is what psychoanalysts have been doing for a century or so, and ministers for a thousand and more years. At times, they were surely successful and could help a person overcome a crisis. Often they were not and quite a few suicides must be attributed to the disease or failed interventions. Thus, a really effective strategy has to go further and has to dig deeper.

Quite obviously, emotions play a major role in depression, thus it seems to be a good and surely straightforward idea to improve them. Cheering patients up, jokes, laughter, fun, music etc. should enhance one’s personal situation. Instead of a focus on thinking – and too many (sombre) thoughts are a major symptom of depression – it seems wise to distract patients, to help them escape their cognitive feedback loops and prisons of thought.

More generally, since a sane mind resides inside a sane body (a fact known since Juvenal’s satires), a reasonable “integrated” strategy should consider the whole person and its surroundings. Consistently, a major pillar of contemporary therapy is amplified physical activity, i.e., a lot of sport and exercise. In the case of a “burnout” (a rather “popular” disease closely affiliated with depression), it is also important to understand a patient’s driving motives, his social interactions and working environment, since there is a consensus that these factors contribute considerably to wellbeing and mental health. It may even be worthwhile to go further and to take a closer look at contemporary modern societies where this disease is spreading like an epidemic.

Last but surely not least, in recent decades we have learnt a lot about the brain's anatomy and physiology. Basic research uncovered the areas of the brain afflicted by certain diseases and the changes that take place at the macro and the micro-level. In the case of depression, specific neural mechanisms together with their pathological aberration could be identified. To cut a long story short, it has become more and more obvious that the malfunctioning of certain neurotransmitter-systems, in particular in the so-called lymbic system (see en.wikipedia.org/wiki/Limbic_system) are strongly associated with the salient symptoms of depression. Therefore, in addition to attitudes, words and sports, ataractics specifically improving the brain's metabolism, are another major option. A few milligrams of Mirtazapin, Fluoxetin etc. may help more than tons of talk. All combined, these measures provide a much more potent therapy than following Freud and analysing the patient's early biography.

Surely, the current state of affairs won't be the last word on the matter either. However, it should have become clear that we have made real, non-trivial progress, and that our (deeper) understanding contributes a lot to effective treatment. How do we know that a certain treatment really works? Again, it is empirical-quantitative methodology, here in the guise of clinical trials. Such trials are experiments that put our ideas to the crucial test of practice. If they fail we ought to modify our beliefs or even abandon them. (At least we should not spend too much time and resources on efforts that do not work.) If our imaginations succeed there is at least some guarantee that they really help, and they constitute a promising starting point for further investigations.

In a nutshell, it is no coincidence that, in most sciences, unsophisticated and coarse qualitative methods are considered rather insignificant whereas evidence-based medicine has become a prototype for other "softer" sciences. Experiment, quantitative argument and conceptual-theoretical thinking have come out on top, as have the fields that adopted them. One hundred or even fifty years ago, psychoanalysis and traditional sociology may have been on a par. Today, however, psychology is closely affiliated with the natural sciences which has boosted its reputation.

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