PAPER
01

A model of models

Background paper for a departmental seminar
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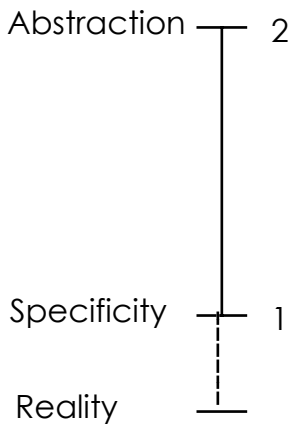
You might measure or classify some of the aspects of some event or instance: the dimensions of an orange for example. If so, your measures would capture or model some select features of the orange: its shape and circumference, perhaps. You would have a model of the orange.

Your model might represent the orange very well for some purposes (deciding if it would fit in your pocket). It would capture the modelled aspects as accurately as your measures allowed. But you would not know how well it modelled other oranges. By repeating your measures on many oranges you would greatly improve the generality of your model. And in doing so you would lose the accuracy with which it modelled the one orange. When we build models we face a trade-off between generality and accuracy or specificity.

So we can picture a continuum stretching from reality at one end to abstraction at the other. There will be a gap near the left-hand end of such a continuum, as our measure will never completely capture reality. But elsewhere on the continuum we have models of varying generality.

Out near the top of the continuum we would have models which are so abstract that they become abstraction only. At the base we would have raw data (at the



point marked "1"). Point 2 represents the *logics* such as symbolic logic, mathematics and the like. Close to it are the theories which aren't theories in the usual sense: information theory, signal detection theory, systems theory. Information theory, for example, is a metric for handling information. Systems theory can be viewed as a set of labels for a set of concepts with defined qualities and interrelationships.

I want to return later to consider another important dimension of this continuum. But first I'd like to pause to ask: Who uses models, and for what purpose?

Users of models

For present purposes I'll limit myself to users of psychological models of humans. To oversimplify a little I'll assume that there are three users: the researcher, the practitioner and the client. The third of these is a recent addition. One person may wear more than one of these hats. But the model in use is then likely to depend on the hat being worn at the time. For the most part we don't have models which suit more than one class of user.

Researchers want models that can be added to, or otherwise modified. That's their job. And they will want the models to fit in with the current philosophy and methodology. They will probably (though not necessarily) accept a philoso-

phy that values a hypothetico-deductive approach, and use statistics which fit most comfortably the biological sciences. Given the choice, they will probably prefer to scale reality down to a manageable complexity.

Practitioners, on the other hand, are obliged to take reality more or less as they find it. When it comes to the point they will probably make do with a model that allows them to make *some* decisions about that reality, if that's the best available. The models may allow practitioners neither to tell how well they are doing, nor how to improve.

Clients often have to be content with the same type of intuitive model that we all use outside our speciality (and that some practitioners use even there). But now that there is often an attempt to involve the clients in their own diagnosis and remediation, the model must be made more explicit.

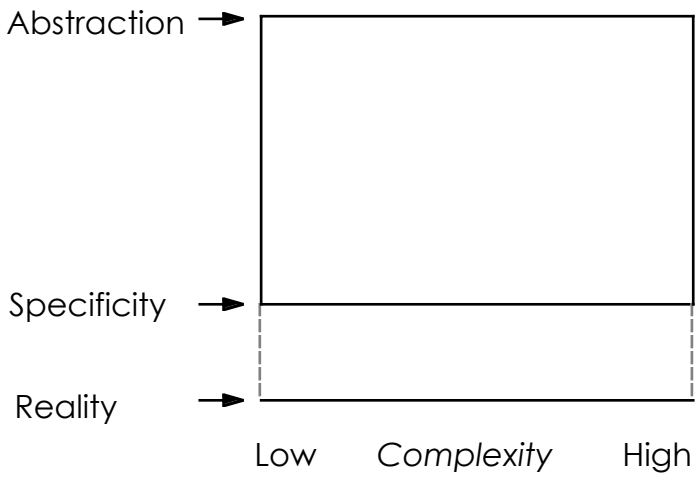
It may sometimes happen that practitioner and client can use the same model, where formerly there was a model for only the practitioner. Compare Transactional Analysis, designed for practitioner-client communication, to Freudian theory, designed for psychoanalysts.

It seems though that research and practitioner can less often use a common model. Yet I presume most researchers would accept as a bonus that their models could be applied. (No, I'm not necessarily arguing for relevance.) The practitioner, I hope, would prefer models to be self-improving. This can happen only if they are testable. Common models would seem to offer advantages.

Researchers need their models to be accurate enough and specific enough to yield testable predictions. Such models will tend towards the bottom end of our continuum. The practitioners require sufficient generality in at least some of their models for them to be able to diagnose a wide variety of client problems, for example. Such models tend towars the top of the continuum.

To make matters worse, the researchers' methodology functions best when interactions between variables are low. The practitioner faces a reality where nearly everything potentially influences nearly everything else. This brings me to the second dimension of the model: complexity of interactions.

My model of models is now two-dimensional. One axis is labelled "specificity" and the other "complexity".



So researchers tend to favour models that are located in the bottom left-hand quadrant. Practitioners are obliged to use models which are more likely to be found in the upper right-hand quadrant. Now, it may be possible in theory to extend a models until it covers both these quadrants. But if this were done it would include so many variables and with such complex relationships that it would be beyond our conceptual abilities to handle it consciously or analytically. It may not be beyond the practioners' intuitions. But they won't be able to communicate it to either researcher or client.

So do we give up? Usually, I think, we do. We accept that science has little place for intuition except for the most important part of deciding what to research. And we leave that out of our reports, as if embarrassed by it. Practitioners use their intuition without quite knowing how or why; they are faced with prob-

lems, and have to do something. In doing so, they must resign themselves to giving up any useful attempt at evaluation. Practitioners can barely talk about what they are doing to themselves, or to other practitioners. They have little hope of being able to talk to a client or a researcher.

But I don't think we have to give up. The more general models are products of the practitioners' intuition. They are built on observations (which are to be found in the lower left-hand corner) and on micro-models. We could accept that, and look for ways of making intuition more scientifically respectable, for example by objectifying it (as the Delphi model for forecasting does). We could accept that a very general model becomes (like logic or systems theory) a conceptual tool. We can't prove it or disprove it. We can decide whether or not it can be applied to a particular situation. And we can strive for as many common models as we can, knowing that they will often not be possible.

Above all, I think we have to develop appropriate criteria which take into account the type and purpose of the model. So we won't criticise Cognitive Dissonance theory because it doesn't generate precise predictions. It isn't that sort of theory. We will criticise its application if we see it applied to a situation which lies outside its boundary of application. And that means we must put more work into defining its boundary of application.

I'm fairly optimistic about the outcome. It seems that we are beginning to develop a methodology which enables us to take more variables and interactions into account. We are less embarrassed about admitting that our intuition may have a part to play. We are less likely to accept the current scientific mythology without question.

I think, too, that there are ways in which we can maximise the fit between models which operate at different levels of explanation. One that seems useful is an analogy of the technique of back-translation used in cross-cultural research. Material is translated backward and forward between two languages until the

successive translations stabilise. So I deliberately try to use the same model whether talking to students, practitioners or clients. The model eventually converges toward something which is useful to each of them.