Social Learning Theory and the Dynamics of Interaction

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The recent controversy between Bandura (1983) and Phillips and Orton (1983) about the causal relations involved in social interactions prompted a discussion of the proper role for formal models in the analysis of social interactions. I argue that (a) Bandura's attempt at formal modeling of patterns of causation is vague and misleading; (b) internal variables, such as expectation and self-monitoring, can easily be handled by formal models; (c) simple deterministic models can behave in unexpectedly complex ways, so cannot be ruled out in principle as explanations for social interaction; and (d) unaided verbal reasoning cannot hope to come to grips with the dynamics of even simple interacting systems.

The recent argument between Phillips and Orton (1983) and Bandura (1983) over Bandura's (1978) earlier article on modes of causation in social learning prompted the following article on the proper role of formal models in the analysis of social dynamics. The issues are general but are best illustrated by example. They are (a) how to represent modes of causal interaction, (b) the amenability of internal variables to treatment by formal models, and (c) the ability or inability of formal models to handle the dynamics of social interactions. I argue that there is a nonarbitrary way to represent social interactions, that formal models are well able to handle internal and external factors in social interactions, and that dynamics cannot adequately be described in any other way.

Modes of Interaction

At the center of Bandura's (1978; 1983, Figure 1) presentation are three alternative conceptions of interaction that he represents as follows:1

Model 1. Unidirectional: \( B = f(P, E) \)
Model 2. Partially bidirectional: \( B = f(P \leftrightarrow E) \)
Model 3. Reciprocal: \( B \leftrightarrow E \),

where \( B \) represents behavior, \( P \) personal factors, and \( E \) environmental factors. For reasons spelled out in his 1978 article, Bandura plumps for reciprocal interaction as opposed to unidirectional or partially bidirectional causation, the mechanical servocybernetic metaphor, which he eschews because "a wholly automated psychocybernetic self system would produce completely predictable responsiveness but at the heavy price of rigidity (p. 352)."

Phillips and Orton (1983), after some interesting excursions into the history and philosophy of social science, concluded that Bandura's argument for reciprocal determinism is holistic, not new, and not right. Any example of apparent reciprocity, they argued, can be reduced either to a functional law (such as the inverse-square law) or to moment-by-moment unidirectional causation. Bandura (1983), in his response, seems to agree, arguing that that is what he meant all the time: "The interactants in triadic reciprocity work their mutual effects sequentially over variable time courses" (p. 166).

How could Phillips and Orton have failed to understand what Bandura meant? The reason seems to be that Bandura's models, although stated in an apparently precise way, are poorly defined. Consider the equations in Bandura's (1983) Figure 1. In words, \( B = f(P, E) \) means that behavior depends on personal factors and environmental factors. The next equation, \( B = f(P \leftrightarrow E) \), says that behavior depends on personal and environmental factors and that these two dependent on

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each other. At the verbal level at least, Model 2
does not conflict with Model 1; it merely adds
another relation—between environmental and per-
sonal factors. The equation form is completely
abandoned in Model 3, but the verbal statement
is clear enough: All three factors depend on one
another. Again, there is no conflict with the other
models; Model 3 seems little more than a combi-
nation of Models 1 and 2, although it is hard to
be certain because the symbolism for the three
models follows no consistent rule.

My Figure 1 shows what seems to be intended
by these three models. It also shows that Models
1–3 are an arbitrary selection from a much larger
number of ways that three variables can affect one
another. Models 4–6 are discussed later on.

At the verbal level, then, there is no reason why
Bandura's scheme should cause alarm. The main
difficulty seems to arise from the formalism, which
satisfies none of the conditions that make formal-
isms useful. How are the terms in the equations
measured? When are they measured? Are the
models static or dynamic? That is, do they refer
to equilibrium relations such as might exist between
body weight and rate of eating, or are they dynamic,
so that B, P, and E are functions of time? Without
answers to these questions, the equations are noth-
ing more than a confusing shorthand for the verbal
descriptions. Until these models are specified more
precisely, we cannot know whether "the process
departs from the mechanical servodynamic meta-
phor" (Bandura, 1978, p. 352).

Internal and External Variables in Interactions

Figure 2 shows a less arbitrary way to describe
interactions between individuals and their environ-
ments (the environment for one individual can of
course be another individual). The figure shows
only two entities, organism and environment, be-
cause measurements are made on only two. The
organism acts in time; hence x, the measure of
behavior, must be a function of time, as y is the
measure of environment. x and y can be single
variables or, more probably, sets of variables, that
is, vectors. Variables discussed in Bandura's article
are TV-channel selection and the occurrence or
nonoccurrence of some undesirable behavior by
aggressive children. For a dynamic analysis we
need a time series, representing aggression bouts
or TV-channel selections in successive time periods,
plus a comparable time series for the presumed
independent variable—for example, a vector rep-
resenting the set of available programs or a variable
representing frequency of punishment in each
time period. With such data, we can represent
behavior by x(t) and the environment by y(t).

Does this approach abandon reciprocal inter-
action, the self system, and the like in favor of the
supposedly inadequate servodynamic metaphor?
Has the confining notation prejudged the theoret-
al issue? By no means. We can represent anything
we wish about the internal properties of the indi-
vidual within this new formalism and do so in a
way that does no violence to standard notions of
causality or even to (nonradical) behaviorism.
Moreover, we are forced to do it in a way that
clearly differentiates between what can be measured
and what must be inferred, a distinction lacking
in Bandura's version of social learning theory.

There are only two functions in Figure 2, one
representing the properties of the individual, the
other the properties of the environment. Thus, the
most general representation is

\[ x(t) = f(y(t)) \]
(1)

and

\[ y(t) = g(x(t)) \]
(2)

where \( f \) represents the properties of the individual
and \( g \) the properties of the environment.\(^2\)

\(^2\) Strictly speaking, Equations 1 and 2 should probably
relate the time derivatives of \( x \) and \( y \) to the variable
values, as in the example I discuss later on. The point is
that exact specification of any dynamic, dyadic system
requires a pair of simultaneous differential equations.
servocybernetic fashion, Equation 1 is often termed the \textit{control function} and Equation 2 the \textit{feedback function} (Staddon, 1983).

This formalism can encompass all of the possibilities discussed by Phillips and Orton, Bandura, and others:

1. If \( g[x(t)] \) is a constant, then the environmental input, \( y(t) \), is independent of \( x \). This leaves \( x(t) = f(y(t)) \) as the only relation—Bandura's Model 1 without variable \( P \). Unidirectional causation. This arrangement corresponds to pure classical conditioning, the only case where an organism's behavior has no effect on what happens to it. This is not Bandura's model. It is not the behaviorist's model either, of course, because operant conditioning by definition demands reciprocity. What happens to you must depend on what you do—\( g[x(t)] \) cannot be a constant for operant conditioning. All will agree, therefore, that Equations 1 and 2 represent the minimum for any adequate model of behavior. This minimal view corresponds to Model 4 in Figure 1.

2. What of internal determinants of behavior? These can be incorporated into the equations, but because they cannot be measured (if they can, they become components of \( x \) or \( y \)) their properties need to be specified in relation to the things we can measure. For purposes of illustration, take the idea of expectation that figures prominently in social learning theory. Consider a specific example of Bandura's: the aggressive child's response to punishment. He proposed that the effects of punishment may depend on the child's expectation, where expectation, in turn, depends on the child's past history of punished behavior. How many things need to be postulated to deal with these dependencies? One is sufficient, an internal variable, \( z(t) \), which represents the effectiveness of punishment and may be termed \textit{expectation}. Equation 1, which represents the properties of the child, then breaks down into two equations:

\[
x(t) = f_1[z(t), y(t)]
\]

(3)

and

\[
z(t) = f_2[x(t), y(t)].
\]

(4)

In words, Equation 3 says that behavior, \( x \), depends on the value of \( z \) (expectation) and the environmental input, \( y \) (punishment). Equation 4 says that \( z \), expectation, depends on both behavior, \( x \), and environmental input, \( y \), punishment. The causal relations here correspond to Model 5 in Figure 1, equivalent to none of Bandura's models. (The effects represented by Equation 4, particularly, will be lagged, of course, in the sense that \( z \) is affected by both present and past values of \( x \) and \( y \). I give examples later on.)

3. So far, Equation 2 is unchanged; that is, I am not assuming any change in the relation between what the child does and what happens to it (i.e., the feedback function, \( g \)). But this restriction can be broken in precisely the same way as for Equation 1: A fourth variable, \( w \), would be introduced, dependent on both \( y \) and \( x \), according to a fourth function, \( g_2 \). Concretely, \( w \) might correspond to the parent's expectation of the child's response to punishment or to the TV network's expectation of audience preferences. If the child becomes more or less sensitive, we may expect the parent to adjust punishment levels accordingly. This modification yields two equations exactly parallel to Equations 3 and 4, and a causal structure corresponding to Model 6 in Figure 1.

It seems unnecessary to prolong the argument. Obviously straightforward causal principles, perfectly congenial to behaviorism, are sufficient to cope with any degree of autonomy, self-reaction, and self-control, providing these concepts are clearly defined in terms of their theoretical relations to observables. "They [people] are also self-reactors with capacities for reflective self-awareness that are generally neglected in information-processing theories based on computer models of human functioning" (Bandura, 1978, p. 356). Maybe so; my argument shows that this neglect stems not from the inability of formal models to handle these subtle concepts but from the requirement to be precise in specifying relations between inferred and measured variables that they impose. Bandura's models incorporate internal factors with ease, not because they go beyond information-processing theories but because they are not (formal) theories at all.

\textbf{Formal Models: An Example}

For the foregoing models to be useful, functions \( f, g, g_2 \), and so on must be defined. We have more equations than unknowns so that unmeasured variables \( z, w \), and so on can always be eliminated, and the expected relations between \( x \) and \( y \) can then be obtained directly. For example, Equations 3 and 4 can be combined

\[
x(t) = f_1[f_2(x(t), y(t)), y(t)],
\]

which can always be rewritten

\[
x(t) = h[y(t)],
\]

where \( h \) is some new function. All models thus reduce to Model 4. Carrying out these steps in practice is often very difficult, of course, because of the numbers of internal variables and functions necessary to describe even quite-simple social behavior. Nevertheless, it may help to fix ideas if I conclude by illustrating with specific functions how this approach might begin to describe interaction between aggressive child and punishing parent, mentioned earlier.

Although we know that interactions between parent and naughty child involve expectations on
both sides \(z[t] \text{ and } w[t]\), above), we also know that all models reduce to Model 4. So it might be parsimonious to begin with the simple case, just to see how far we can get in the way of behavioral complexity. What are the basic properties of the relation between punishment and the behavior that elicits it? Perhaps three basic properties are that (a) the punishment follows the crime, that is, there is some lag between the two; (b) the more serious the crime, the greater the punishment; and (c) punishment reduces the level of crime, also with a delay. I show that these three properties alone, apart from the additional complexities of expectation by either party, already imply quite complicated patterns of interaction.

To come up with a model, I need to assume something about what will happen to the level of aggression, \(x\), in the absence of any punishment, \(y\), and then add to that the effect of punishment. These two together make up the control function, Equation 1. Similarly, I need to specify what will happen to the level of punishment in the absence of aggression, and add to that the effect on punishment level of the occurrence of aggression. A simple possibility is to assume that \(x\), aggression, grows without limit in the absence of punishment. (In other words, I assume that the stimuli for aggression are constantly present, so that the level of aggression depends only on the child's past history of punishment; later I relax this assumption.) Conversely, if punishment occurs, aggression declines at a rate proportional to the product of aggression and punishment levels. Similarly, punishment grows at a rate proportional to the product of punishment and aggression (so in the absence of aggression does not grow at all) and otherwise declines at a constant rate.

A simple and well-studied scheme that embodies these ideas is shown as Equations 5 and 6:

\[
\begin{align*}
dx/dt &= Ax - Bxy \\
dy/dt &= BCxy - Dy,
\end{align*}
\]

where \(A, B, C,\) and \(D\) are positive constants. Notice that these assumptions do no violence to what Phillips and Orton (1983) term traditional unidirectional causation: The levels of \(x\) and \(y\) at one instant cause small changes in the levels of \(x\) and \(y\) in perfectly standard fashion. Equations 5 and 6 are one form of the Volterra equations, discussed in any population biology text (e.g., Roughgarden, 1979).

It is easy to show that these equations embody the three basic properties of the interaction between crime and punishment. When punishment levels are high, aggression levels are low; punishment lags behind aggression, and the two are in a sort of equilibrium in the sense that neither punishment nor aggression increases without limit. But the model has other, unexpected, properties. For example, under free conditions (that is, without any special external stimulus for aggression) the levels of aggression and punishment vary in a cyclic fashion, with punishment lagging behind aggression. Moreover, the amplitude of the cycle depends entirely on the initial conditions—if a stimulus is briefly presented to raise the level of aggression, cycles ever after will be at a new, higher amplitude. This improbable behavior is eliminated if the equations are modified to enforce a ceiling on the free level of aggression. Now the equilibrium is steady, although there may be cycles as it is approached (see Roughgarden, 1979, for details).

Another modification that has a stabilizing effect is to allow parameter \(B\), which measures the effectiveness of punishment, to vary as a function of the level of punishment. This is a simple way to introduce some idea of expectation; for example, suppose intuition tells us that children become habituated to high levels of punishment, so that punishment becomes less and less effective the more it is used. This assumption is embodied in Equation 7:

\[
dB/dt = 1 - EBy - B,
\]

where \(E\) is a constant. In words, the equation says that \(B\) grows at a rate inversely related to itself (i.e., it is negatively accelerated) and to the product of itself and \(y\); as \(y\) grows larger, \(B\) grows more and more slowly or declines. Because the effectiveness of punishment depends on the value of \(B\), Equation 7 suggests that punishment should lose its effectiveness at high levels. The causal relations now resemble Model 5 in Figure 1, with \(z\) replaced by \(B\), but no direct causal link between \(x\) and \(B\). With this modification, Equations 5–7 describe a system that usually settles down to a constant relation between crime and punishment, but shows some oscillation when perturbed by an external stimulus. An unexpected result is that introducing a simple sort of expectation does not have much effect on the overall level of aggression, but just makes interactions more stable. These effects are all illustrated in Figure 3.\(^3\)\(^4\)

\(^3\)These curves were obtained using the following discrete-time approximation to Equations 5–7:

\[
\begin{align*}
x(n + 1) &= (A + 1)x(n) - Bx(n)y(n) \\
y(n + 1) &= [b(n)Cx(n) + 1]y(n) - Dy(n) \\
b(n + 1) &= 1 - Eb(n)y(n),
\end{align*}
\]

where \(A, C, D,\) and \(E\) are constants. For the graphs in Figure 3, \(A, C,\) and \(D\) were 0.2, \(E\) was 0.5.

\(^4\)One reader of the manuscript remarked that the behavior in Figure 3 weakens my case for formal models "because aggression, \(x(t)\), continues to be effectively reduced by smaller and smaller increases in punishment,
Equations 5–7 are intended as nothing more than illustrations of how interactions between individuals might begin to be modeled in quantitative fashion. They should be judged by the complexity of behavior in relation to the simplicity of initial assumptions, rather than by conformity to the real dynamics of social interaction. The equations illustrate complexities of two kinds: the complexity of the problem of understanding interactions between two systems, each of which can adapt and change in time, and the complex behavior of nonlinear equations. It is not possible to conclude that the first complexity is reliably greater than the second; the ability of even quite simple nonlinear systems to display periodic, aperiodic, and even chaotic (i.e., completely unpredictable) behavior is now well known (e.g., Gollub, Brunner, & Danly, 1978; May, 1976).

It should be obvious that making a realistic model for the dynamics of any social interaction is an ambitious undertaking, demanding quantitative methods that go well beyond what can be accomplished by verbal reasoning alone. Numerous problems must be solved before a satisfactory model can be devised: Simultaneous, nonlinear equations (or computer programs) tend to be unstable, yielding unrealistically large or negative values for the variables under many conditions; results are usually highly sensitive to particular parameter values; and the behavior of the equations is rarely obvious to intuition. I have omitted discussion of problems of measurement, which interact with, and are partly resolved by theoretical exploration. The proper things to be measured depend on how we think they are related, and the possibility of a particularly simple relationship may often dictate the choice of variables. It is

\[ y(t), \] whereas it should have the opposite effect." This observation well illustrates several points about formal models: (a) Intuitive reactions cannot be trusted as clues to underlying process. My colleague felt that Figure 3 shows increasing sensitivity to repeated punishment, yet there is nothing in the formal model corresponding to sensitization. Indeed, I justified Equation 7 by assimilating it to habituation, the opposite process. In fact, Equations 5–7 can show either reduced or increased effectiveness of repeated punishment, depending on parameter values. (b) People’s intuitions differ. Someone else might well feel that if punishment is sufficiently severe, the effective dose should get smaller and smaller. (c) People’s perceptions differ. Someone else might attribute the effectiveness of smaller and smaller punishment doses apparent in Figure 3 to the declining base level of aggression. (d) The decisive test is to see whether the detailed dynamics of behavior correspond to our model, not whether the model’s behavior satisfies someone’s intuitions.

5 One reviewer commented that Equations 5–7 are too simple because they are path independent; that is, the behavior of the system depends only on its current state and not on how it got there. There is more to the concept of path dependence than meets the eye. All deterministic systems are by definition path independent. Nevertheless, as I have noted, systems of deterministic nonlinear equations can produce chaotic (i.e., apparently path dependent) behavior (the pseudo-random-number algorithms employed by many computer programs are a commonplace example). The resolution of this paradox is that path dependence is a relative property and may just reflect ignorance of essential variables: A system may be path dependent in terms of one set of variables and path independent in terms of a more comprehensive set. Thus, Equations 5–7 are path dependent in terms of \( x \) and \( y \), that is, a given pair of \( x \) and \( y \) values can be followed by different behavior at different times. But the system is path independent in terms of \( x, y, B, \) and their time derivatives.
often assumed that behavioral interactions must include a stochastic element. Perhaps, but as I have argued, many fully deterministic systems appear highly unpredictable so that stochasticity cannot be assumed just because a system behaves in an apparently capricious way.

Conclusion

I conclude that (a) social learning theory should dispense with ill-defined equations that give the illusion of precision without the reality, (b) Internal variables—expectation, self-monitoring, internal standards, and the like—can be handled perfectly well by servocybernetic models, which have the added virtue of forcing us to distinguish between things we can measure directly and things that are inferred or assumed, (c) Even quite simple deterministic systems can have very complicated dynamics, so social learning theorists should be very cautious indeed before assuming that their subject matter is beyond the reach of formal theory. An adequate test demands careful comparison of real time series with theoretical predictions, (d) Unaided verbal reasoning is almost useless in the analysis of the dynamics of interactions, human or mechanical.

References


Received July 6, 1983
Revision received April 17, 1984

Corrections to Navon

In the article “Resources—A Theoretical Stone Soup?” by David Navon (*Psychological Review*, 1984, Vol. 91, No. 2, 216–234), there were two errors in wording on page 220. First, in column 2, line 21, the word *but* should be changed to the words *that is*. Thus the sentence “In both cases, task interference is not due to scarcity, but to inability of the processing system to furnish the concurrent tasks with the provisions each would have when performed in isolation” should read “In both cases, task interference is not due to scarcity, that is, to inability of the processing system to furnish the concurrent tasks with the provisions each would have when performed in isolation.”

Second, in column 2, line 38, the phrase “seems more convincing of task interference” should read “seems more convincing of the economic interpretation of task interference.”