Frustration: Theory and practice

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Frustration is often dismissed as a transient by-product of thwarted aspirations, a disruptive and uncivilized mark of Cain. Amsel's work, however, shows the creative and enabling role that frustration can play in the behavior of organisms. The book epitomized here first clarifies the basic phenomenon and its causes, and then extends it by mapping its development, along with that of other behavioral markers, against the development of brain structures. One may take exception to the particulars: Are the chosen variables the best ones to measure? Is frustration an autonomous motive or is it the liberation of the arousal normally focused on the instrumental response? Is the best reading always given to the large and heterogeneous literature? But the whole of Amsel's work transcends these particulars and exemplifies, as do few other curriculum vitae, the ideal of systematic scientific inquiry that is praised more often than practiced.

Bug or Feature?

Who has never lost a coin in a vending machine, only to find himself, a newly born Pepsi-Luddite, thrashing its panel; or watched a driver confronting a flat tire perform the autoballet—raising then lowering the hands, a shuffle for balance, and a final grand *battement* to the flat; or scowled at a child, not yet fully socialized/inhibited, as it turned its thwarted reach into a noisy pounding of the table? Frustration is ubiquitous; a large part of maturation involves learning how to deal with failure, impediment, and loss in a “mature” manner. To the layperson, frustrative responses— r_{fs} —seem a breakdown mode, a bug in an otherwise smoothly running machine, an atavism in a Dr. Spock, a circuit malfunction in his epigone Data. But experimental psychologists of biological persuasion assume adaptive value, and ask what benefit might be served by this feature shared by so many organisms, this ability to be empowered by failure.

Amsel muses over this issue in chapter 10 of *Frustration Theory* (1992), directing us to Nation and Woods (1980) for a compelling analysis of persistence training in psychotherapy, and noting other implications of the phenomenon for the human condition. Until recently, however, Amsel himself was more interested in establishing the scientific credentials of frustration as a critical source of motivation and discriminative control in behavior, as reviewed in the prior nine chapters of his book. Establishing those credentials has been an uphill battle, and Amsel's persistence in this task makes this book something of an autobiography. What early frustration can explain his admirable perseverance in the study of this phenomenon?

Amsel's Frustration

Scientists, by and large, are levelers or sharpeners—fastidious seekers of a few general laws, or gourmets of diversity. Hypothetically there may exist a continuum of explanatory styles, but in reality the extrema attract most explainers. On the far left are the rich, descriptive, humanistic, Geisteswissenschaftler; on the right are the parsimonious, rigorous, scientific Naturewissenschaftler. Geisteswissenschaftler who omit a potentially useful descriptive construct are quickly improved by embellishment. Naturewissenschaftler who add a potentially useful explanatory construct are quickly improved by reduction.

Amsel's audience is the community of behaviorists, paragons of the right. But *frustration* expands the list of prime movers from fear and hunger, the most commonly studied, to a new response with its own stimulus properties. Furthermore, it is a drive without a supporting need state of its own, a parasite on other drives. Who needs that? Worse yet, it seems so—so anthropomorphic!

Amsel's *frustration* is not without precedent. Skinner (1953), always ready to fire Morgan's Canon across the bows of (other) embellishers, discussed the role of emotional responses in extinction and even reported an experiment in which he extinguished the leg flexion response of a pigeon (suspended in a harness), as well as a concurrently conditioned keypecking response. “The extinction curves, recorded separately, are slightly displaced in time, but the major oscillations occur simultaneously. This suggests that the rise and fall of frustration is a single process in the whole organism, while the change due to extinction is separately determined in each response” (p. 209). But frustration, to Skinner, seemed more of a nuisance variable than a key motivational force; he believed that with repeated conditioning and extinction, such responses would “adapt out.” And he never gave any thought to the implications of the Pavlovian conditioning of their stimulus concomitants as S^d s for persistence—and *that* is the key to Amsel's theory.

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Amsel's Frustration Theory

Amsel explains in his précis (1994), and in greater depth in his book, the Pavlovian implications of a frustrative motivational state that bears a regular relationship to reinforcement. At first the state is aversive and impedes progress; but it is also energizing, and this invigorates responses in general where they are not being inhibited by closer contiguity with the goal; eventually, as discriminative stimuli for eventual reward, they serve to mediate extended responding in extinction. It is as though learning is state dependent; finding oneself in a frustrated state becomes predictive of eventual reward for partially reinforced subjects, but not for continuously reinforced subjects. This is not generic Pavlovian conditioning, but a species of it that Amsel characterizes as dispositional learning (see his Table 1). There is a wealth of data here, taken from classics deep in the archives as well as from contemporary research on the ontogeny of dispositional learning and its utility as a marker of brain development. Amsel's compass of this large range of results within traditional neobehaviorist theory does credit both to him and to that framework. While lionizing Hull in an earlier compendium (Amsel & Rashotte, 1984), Amsel brings many more data to converge on his theory than Hull could ever muster for his own. Do the data support the theory? Yes. But ...

Truth, Bohr argued, is complementary to clarity: The more we attempt to communicate, leaning on familiar analogies and verisimilitudes, the farther we depart from an accurate description of a phenomenon *sui generis*. To speak the truth, in all its detail, requires qualifications and particulars that overwhelm. (Not surprisingly, Bohr's lectures were opaque.) Amsel is very clear, taking pains to order, organize, illustrate, and summarize. But still the details overwhelm. I don't have the conviction that, given all the inevitable particulars of an evolved system such as rats or humans, Amsel's reading is the best possible. It is the best available, and unless one can improve it (a task that will require considerable assiduity), one must respect it—both for its substance, and as a model of programmatic scientific endeavor. Perhaps many readers will find their residual, unsettled dubiety invigorating; Amsel has organized many data, and an energetic young scientist has a fine place to start if he or she hungers, as good theorists do, for a yet simpler or grander overview. Amsel alludes to Grand Unified Theories that may eventually incorporate his analyses with others, and provides a substantial amount of this unification himself, relating frustration theory to theories of arousal, habituation, regression, brain development, and so on. As our database becomes richer, there is both increasing need for such syntheses, and increasing likelihood of their success. Those who enjoy such jigsaw puzzles should study *Frustration Theory*, for Amsel has completed some borders and has aligned bunches of other pieces.

Even while I am not completely sold on Amsel's theory, I rather like his metatheory, the Six Steps up the

Altar of Psychobiological Science (Table 3). The ascent is something of a Pilgrims' Progress, however, with a significant fraction of the aspirants stopped by each riser. (1) "Observing and describing" are commonsensical first steps, but who has ever started there? Too many authorities save us that effort by telling us what to study, in what species, under what conditions. Ultimately, their help hinders. Picasso struggled for years to divest himself of the theories his conditioned eye imposed on the world. We likewise need conceptual lenses to correct the astigmatism imposed by our theories. (Remember Guthrie's treatment of modal-action patterns as learned responses?—Moore & Stuttard, 1979.) We must decontextualize to achieve theoretically useful units of behavior, but never without an eye to the evolutionary and ecological context left behind, back on the first step, as we ascend to: (2) "Develop theory," which is the fun part. Many linger here, never stepping back for a reorganization of the givens, nor assaying a higher step. I myself am content to reside on these first two steps, while admiring the insights achieved by those who: (3) "Study the effects ontogenetically," which can clarify the provenance of the chosen behaviors. As one of many possible examples, Timberlake and Lucas (1990) note the possibility that a significant "adjunctive" behavior of pigeons, chesting into the wall where they are occasionally fed, is of the same form as the food-begging behavior of young squab. It pays to know your subjects, and Amsel has studied his well, both developmentally and neurologically (Step 4), permitting him to correlate the development of each (Step 5), and modify his theory as a result (Step 6). This is the right stuff. Of course, one might prefer to: (3') "Study the effects phylogenetically," effecting a comparative neurological analyses across species (4'), correlating structures with functions (5'), etc. (6'). Such comparative neurobiology has made substantial contributions to our knowledge of brain-behavior relationships, even as simple comparative psychology has to the study of animal learning (e.g., Bitterman, 1975; Domjan, 1987). Amsel's version minimizes extraspecific variability, but introduces the extraneous variability of intraorganismic maturation co-occurring in many unspecified systems. The approaches are complementary; because Amsel's is relatively less exploited, it has special promise.

What of Skinner's (1950) warning against premature reliance on "conceptual" nervous systems? Behaviorists had better first get their own theories straight, he opined, before using them to explain—or be explained by—the nervous system. But that was 50 years ago; we've all matured a bit since then, and perhaps it's worth a try. Not that we yet have our own theorizing straight; but some of our problems are local minima, and this larger perspective may help us route around them. Myself, though, I'm staying down on the first few steps, because I don't think we even have the basic variables straight. Take, for instance, runway speed.

Ballistic Rats and Qwerty Runways

Some years ago, on sabbatical at Texas, I pondered the dependent variable in runway studies, speed of locomotion. Pellets in the goal box don't *speed* animals down the runway, they *accelerate* them; yet we find the derived measure *speed* to be ubiquitous in traditional runway research, and the primary dependent variable in Amsel's theory. Unfortunately, speeds in the different portions of the alley change in different ways under various experimental manipulations. Which speed is diagnostic for the theory—speeds near the startbox, in the runway, or near the goal box? Measurement of acceleration might simplify analyses, but who owns accelerometers? As a first analysis, Abe and I assumed that the food in the goal box attracted the rat with a constant force, up to a point in the alley at which the rat must start decelerating to come to rest over the food cup. With at least three speed measures, we could determine the three necessary parameters in this recoding of the data: latency to start running, acceleration, and point at which the animal begins to decelerate (*brakepoint*). Writing and applying the equations was simply a matter of ballistics (Killeen & Amsel, 1987). Of course, with only three measurements, we could not test the goodness of fit of the model, but we could ask whether the transformed variables changed in interesting ways. We found that they did. For instance, when plotted over training trials, start latency decreased, acceleration increased, and brakepoint moved earlier in the alley. We found that, as one might expect, large rewards accelerate animals more than small ones, that 50% partial reward accelerates animals more than comparable 100% reward (the partial reinforcement acquisition effect), and that animals stop accelerating sooner under partial reinforcement than under continuous reinforcement.

One can also plot the inferred velocities through the alley as a function of experimental conditions. Figure 1 shows one diagram, in which groups of rats received continuous reinforcement in both black and white alleys (continuous between-group: CB), or partial reinforcement in both alleys (partial between: PB). Other groups received partial reinforcement in one alley *and* continuous reinforcement in the other; their speeds in the continuously reinforced alley are reported as CW (continuous within-group), and in the partial alley as PW. Note that the acceleration of the CB group was greatest, with little difference in the acceleration of the other groups; however, the brakepoint of the PB group was earlier than that of the other groups. The PB was the only group not to experience some continuous reinforcement. And, significantly, it is the only condition to reliably show persistence in extinction—the partial reinforcement extinction effect. So our recoding is getting at something important—longer term persistence is correlated with a more deliberate final approach to the goal.

So what? Well, let's face it, I'm a leveler. When I face an encyclopedia such as Mackintosh's (1974) or Amsel's, my brain quickly saturates; I relieve the pressure by grabbing a pencil and scribbling, searching for ways

Figure 1. Speed at various points in a runway under four different conditions of reinforcement. The data are from Amsel, Rashotte, and MacKinnon (1966). This figure is duplicated from "The Kinematics of Locomotion Toward a Goal," by P. R. Killeen and A. Amsel, 1987, *Journal of Experimental Psychology: Animal Behavior Processes*, 13, p. 97. Copyright 1987 by the American Psychological Association. Reprinted by permission.

to condense the memory load. Now, Amsel with his theory of frustration has made the assimilation of all this information much easier, but that means it only takes a little longer for me to be overwhelmed. I like to hope that lower level analyses that distill multiple measurements into a few psychologically important variables such as acceleration and brakepoint will further reduce the things we need to remember—the differences between experiments, the differences in effects when measured in start- versus goal box, and so on. Lower level models need not be perfect to be useful. The discontinuity in the curves of Figure 1 certainly doesn't exist in the data (but it would require another parameter [or perhaps just a better model] to round off). Constant acceleration is a dubious assumption—it is more likely that both acceleration and deceleration increase as the goal is neared. The anatomy of the subject is important: Animals move in gaits, and once a gait is achieved, speeds are much less sensitive to motivational effects (remember Premack's insight to use delay of reward to pull runway speeds down from their ceiling gait in order to measure a positive contrast effect—Premack, 1969; see also Amsel, 1971). Acceleration *into* the gait is likely to be the most important psychological variable. In all areas of research, more systematic accounting for the physiological constraints on an organism—whether the measures are alley speed, pecking rate, or choice reaction time—will reduce the explanatory burden that must be carried by the higher level, more psychologically interesting theories.

So why didn't Amsel, who knows all this, couch his theories in terms of acceleration? Several reasons, I suspect. Certainly a better model than ours can be written to clean up runway data; it would have been premature to take a traditional dependent variable and transform it in our provisional way when presenting his massive opus

to the public. But more importantly, many of the classic studies were conducted and reported in ways that make the transformations impossible. People used traditional runways because they had them on the shelf, because they were familiar gizmos to journal editors, and because the results could be compared with those from the use of other similar runways. Like qwerty typewriters, whose traditional anti-ergonomic keyboard is retained even as it becomes an interface to space age computers, our experimental apparatuses are evolved machines (Amsel, 1992, pp. 213–215; Timberlake & Lucas, 1990), mixtures of functional keys and anachronistic bells. Where we lack good lower level models of our interface with the organism, often the best we can do is hold all that constant. When the force of experimentation is great enough, new apparatuses—Skinner boxes, double alleys, radial mazes—are conceived, engendering in turn their own literatures and traditions. These are often driven by higher level theories (and as often by frustration with the too large literature amassed with the use of older apparatus: when a finding can't be encompassed in comps, it leaves the canon to become a curiosity). The lower level ergonomics and ecological sensitivities of the organism are less often considered and then only intuitively, as the designer briefly wonders what it is like to be a rat, before reaching for the plywood.

The Culture of Frustration

Such apparatus-driven traditions segregate the research community into schools that may respect another's work, but rarely replicate it. Such is the case with frustration theory. It is unfortunate that the study of frustrative persistence has not attracted as many students as the study of, say, reinforcement schedules, for it holds poignant implications for us as parents and citizens, as well as scientists. The ghetto riots of the 1970s were not so much due to long-standing suppression of people, we are told, but to the contemporary thwarting of people's rising expectations. As Amsel's book clearly shows for lower organisms, persistence—that most American of virtues—arises from early experiences with intermittent reinforcement. Such persistence may generalize widely, to become the kind of tenacity that breeds success in our culture (Eisenberger, 1992). The discriminating, consistent, and affluent parent may protect her child from those very unhappinesses that build character. Sparing the rod does not spoil the child, but sparing early frustration might. First-generation immigrants are less able to cosset their children, who manifest proverbially greater

need for achievement. The poorest lack opportunities to bind their frustrations to eventual success, and they become bound instead to failure. Helplessness is too easily learned along the mean streets; impoverishment does not, *pace* 19th-century industrialism, build a pool of motivated workers so much as a swamp of despair. Frustration theory needs its students, for they could have much to teach us about ourselves and our culture.

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