intension of the class to one or more perceptual or functional properties. They could also apply to theoretical principle. Although psychologists discussing concepts have implicitly limited the meaning of similarity in this way, workers in other disciplines have not. Examination of the practice of biological taxonomists not only illustrates the organization of classes with respect to theory but also illustrates that theory and taxonomy evolve over time as a result of their reciprocal relation. Advances in theory, and the more clearly defined classes to which they lead, give rise to new questions, new investigations, and eventual revision of the theory. These dynamics undoubtedly obtain not only within a particular discipline but also in the individual's evolution of understanding.

Although examination of concepts in terms of their organizing theory may shift emphasis from the form of conceptual structure to the nature of the theory, it eliminates neither the need for nor the utility of accounts of conceptual structure. In fact, each of the views of conceptual structure that has been proposed has a useful role in describing conceptual structure and evolution depending on (a) the purpose of the theorist, (b) the stage in development of the theory, or even, (c) individual differences in conceptual style. Each criterion will be considered briefly.

The goal of experimenters and practitioners is attainment of a *descriptive* theory to describe behavior accurately in terms of its causal determinants. A *normative* theory addresses operation of a system under optimal conditions, an idealization that provides a standard of comparison that may never be realized in actuality. Medin, like others before him, criticized classical views of conceptual structure for failure to provide an adequate descriptive theory for many real-life concepts or for classification behavior in general. As a normative theory, on the other hand, the classical view has played a directing role and will continue to do so not only as a functional null hypothesis, but also as a description of the goal of expert classification. Medin seems to be saying the same thing in his invocation of *psychological essentialism* to account for the intuitively compelling nature of classical views. Failure to attain the normative goal leads to probabilistic organization as an inescapable provisional default.

The very early stages of concept acquisition by a child or novice seem to be well described by exemplar-based models. For example, most analyses of word learning assume that it proceeds through successive encounters and concomitant labeling of a variety of specific instances. Such an assumption almost presumes an exemplar-based structure (although discussions rarely consider possible instances in which the initial or modal example is not prototypical). With progressive encounters of more novel instances, there is increasing need to induce an organizing principle for changing a name into a class. This process has been described more formally (Inhelder & Piaget, 1964) as coordination of the intension (defining properties) of a class to its extension (enumeration of individual members). The nature of the organizing principle induced seems to change systematically with advances in age and knowledge, but at this stage of developmental research it is not yet possible to identify specific stages in evolution or to assess their generality across knowledge domains and cultures. Not all concepts begin life by way of a specific example: Academically acquired concepts are often introduced by way of definition in terms of intensive properties. Vygotsky (1962) called concepts so acquired *scientific concepts* and deplored the lack of systematic study of them. Each of us has experienced the process and is well aware of the need for specific examples and context as an aid to understanding. Here, too, despite differences in the form of initial encounter of academically acquired concepts, the early structure appears to be exemplar-based.

Whatever their stage of development of expertise, individuals undoubtedly differ with respect to their need for coherent theory or explicit formulation of concepts. Because the practical demands of daily life rarely require more than a collection of practiced habits, one can get by with probabilistically organized concepts based on superficial or irrelevant features. Perhaps only academic types and consistently reflective individuals are so concerned with examination of knowledge as to formulate concepts explicitly in relation to knowledge domains and the more demanding definitional criteria associated with them.

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Editor's note: *The following comment is unusual in many ways, and it is not typical of the articles published in* AP *or in other APA journals. It was originally received and accepted by* Psychological Bulletin. *After discussions among the* Bulletin *editor, the previous* AP *editor, and the APA Publications and Communications (P & C) Board, it was agreed that* AP *would be the most appropriate APA outlet.*

The Anticreativity Letters: Advice From a Senior Tempter to a Junior Tempter

Richard E. Nisbett
The University of Michigan

Note. The letters that follow fell into my hands under remarkable circumstances that I am not at liberty to reveal. The correspondence bears a striking similarity to *The Screwtape Letters,* edited by C. S. Lewis. In those letters, a senior devil advises a junior devil about how to win the soul of a human being for Satan. The senior devil is very knowledgeable about human psychology and is a clever student of human frailties and how to make the worst of them. Similarly, in the letters reproduced here, a senior "tempter," as he calls himself, working on behalf of an underworld figure he calls the "Anti-Muse," counsels a junior tempter about how to prevent a young psychologist from being a productive and original scientist. I think the letters constitute an interesting set of hypotheses about how to stimulate, and how to prevent, creative work in psychology. I have edited the letters very lightly, noting one or two obvious errors of fact and making educated guesses about some words that were obscured on singed portions of the manuscript. Phoebe Ellsworth, Susan I. Nisbett, and Lee Ross made comments that were helpful to me.

(1)

My dear Slump:

I am surprised that you are allowing your patient to continue his association with these friends of his. You may have been thinking that they would keep his mind off of his work and therefore help to ensure his lack of productivity. This is dangerous nonsense. They are quite stimulating and unpretentious people for the most part. The fact that they are not psychologists provides no advantage for us. All of life is a source of psychological ideas, and the more interesting the people he associates with, the more interesting are the psychological ideas he is likely to turn up.

What you want to do is to restrict his associations to people in his own field, on the grounds that these people are pleasant

enough and it is really more useful from a career standpoint to hobnob with fellow professionals. Then, of course, you want to steer him away from the people who are absorbed in interesting work, and toward the sneerers. When it comes to associates, there is nothing so useful as sneerers, especially if they are intelligent and witty. They are sure to dismiss your patient's ideas, should he be so foolish as to discuss them, as trivial, old hat, obvious, or patently ridiculous. *Old hat* is really the most useful accusation because it need never be proved. The clever sneerer knows that he or she need merely assert that an idea is an old one. Such an assertion will not be called into question one time in a hundred. On the rare occasion when the sneerer's bluff is called, it is child's play to merely generalize the putatively novel idea up to some suitably vague level of abstraction and then assimilate it to some distant ancestor. My colleague Fallow very nearly succeeded in throwing Leon Festinger off the trail of dissonance theory by encouraging him to associate with people who could be counted on to tell him that dissonance was nothing more than rationalization recycled into new bottles. Festinger's unfortunate self-confidence blocked that move, but with a more humble patient you would have won every time.

I am quite pleased with your reading program for the patient. I note you have steered him away from philosophy and literature by intimations of "hot air," "speculation," "fantasy," "waste of time," and so forth. This is much to be commended because great philosophy and great literature are an unparalleled source of ideas in psychology. I hardly need to remind you that my colleague Doldrum botched the Schachter case in every way. Personally, I think one of his biggest mistakes was that he encouraged Schachter to read *The Magic Mountain* as an adolescent. I think he had some vague idea of steering Schachter toward becoming a mediocre novelist. In any case, Schachter read Mann's description of Hans Castorp's falling in love with Clavdia Chauchat, including the passage pointing out that the palpitations produced by the cold, thin air of the mountains were misinterpreted by Castorp as evidence of his passion. He also allowed Schachter to read Bertrand Russell's observation that an injection of epinephrine, administered by his dentist as a blood clotting agent, had caused him to feel extremely fearful until he recalled that epinephrine produced autonomic arousal. The result is too sordid and notorious to bear repeating.

I am well aware that my story about Doldrum and Schachter bears a superficial similarity to the utterly different case of myself and Daryl Bem. It is perfectly true that I allowed Bem to read the British behaviorist philosopher Ryle, with his statement of the basic notion that beliefs can be inferred from observation of behavior. My action here was quite correct (as the subsequent hearing established beyond a shadow of a doubt). Bem was determined on a career as a physicist. To allow him to read British empiricist philosophers concerned with human behavior seemed a valuable diversion. It could not have been anticipated that he would become a social psychologist. The anti-muse herself stated at the hearing that Bem seemed certain to become a second-rate natural scientist. My conscience, and my record, are clear.

Nevertheless, I will admit to having learned something from the Bem experience. I think it is well to be guided by the motto "Psychologists who are ignorant of intellectual history are condemned to repeat it in their laboratories." The behaviorists' wonderfully philistine attitude made it easy for us in this respect. The intellectual pretensions of the cognitive psychologists have made it much harder. I think the best one can do with some modern patients is simply to steer them toward the pretentious and the obscurantist in philosophy and literature and away from material with real substance.

Of course, you are quite right to have your patient constantly reading the journals as they arrive in the mail and baying after every article of any conceivable relevance to his interests. Once the habit of following the literature is established, it can easily become a lifelong vice. Few things are more calculated to destroy creativity. Whatever original ideas he might have will slowly become assimilated to what he is reading, so that his work will be at best derivative. Unless he is very energetic, in fact, he can be counted on to give up research altogether on the grounds that he never has any ideas that someone else didn't already have.

His graduate program is your best ally in this respect. Everything about it is geared toward giving him the idea that before you work on an idea, indeed, before you even know what your idea is, you begin reading widely on the general topic of your interest. Then, after you have become suitably knowledgeable, you are allowed to design your own research. Your patient is not likely ever to hear that Kurt Lewin positively forbade his students to keep up with the literature (while encouraging them to read and reread the classics). We have been successful in preventing publication of Lewin's opinions on the matter of reading habits. Really, though, by now I don't think it would matter if they were widely known. The opposite practices are so well entrenched in graduate programs that any student who heard Lewin's opinions would think he was joking or that he was just a silly old codger. And how would they know differently, inasmuch as we have succeeded in preventing almost all students from reading Lewin or indeed almost anyone who is no longer around?!
Your affectionate uncle,
Snidely

(2)
My dear Slump:
Your point is well taken that sneerers can be hard to find these days. I readily grant that your work is made more difficult because of the cultural trend away from competitiveness and superciliousness. I was able to use the culture of the 1950s to create the poisonous atmosphere at that well-known east-coast department that admitted such a high fraction of the talented young aspiring psychologists of recent decades. Every student lived in terror of the faculty's critical skills. Each member of the faculty could find six flaws in every design, 12 artifactual explanations for every finding, and 24 predecessors for every alleged original idea. Every student adopted that same stance toward his or her fellows. And, most important, that critical stance became part of the scientific conscience of every student. As a result, hardly one of those talented students has lived up to the early promise shown. Each of them can be counted on to shoot on sight any glimmer of an idea the moment it makes an appearance on the horizon of the mind.

But such an achievement would be hard to duplicate now. In the age of "Have a nice day" you may have to make do with steering your patient toward the most agreeable folks he can find. These people will give him no criticism whatever. Indeed, they will avoid talking about ideas altogether because that might leave someone feeling left out. This is not as destructive as sneering, and it will not prevent real talent from flowering, but it is devastating to the intellectually lazy ones who don't want to work hard anyway.
Your affectionate uncle,
Snidely

(3)
My dear Slump:
I have received a report that your patient now has as his adviser a highly interesting psychologist, a woman who is doing very original and important work, and that he has begun to design research with her. (I had to rely on our field office for this be-

cause somehow your letters failed to mention this tiny detail. I will have more to say about this both to you and to the divisional secretary. For the time being, a word to the wise: You're better off if I hear it from you.)

This is a severe setback, but all is not lost by any means. If I had a promotion for every graduate student I have rendered incapable of learning from a talented adviser, I would be in contention for the job of the Anti-Muse herself. (That last is, of course, mere exaggeration for effect. There never has been, and never will be, a more talented temptress than Our Mother Below.)

The tried and true ways are best at this difficult juncture. Your most important advantage is that nearly all young graduate students in the social sciences think that learning how to do research is like learning how to write a novel. That is, they think they have to come up with an idea, which they then work on more or less in solitude, with occasional criticism and advice from a professional. That, after all, is the way they functioned with their senior honors theses. (It never occurs to them that custodial care is all that most faculty are willing to give to undergraduates engaged in research.) So they don't realize that a continuation of that apparently grown-up way of functioning will keep them largely ignorant of how to pursue an idea across a series of investigations, in the face of failure, criticism, and a host of unanticipated difficulties.

We know, of course, that learning how to do research is really like learning how to make movies. There are many things to be learned that are quite invisible to the novice, some highly technical, some grand strategic, and some mundane-seeming but crucial. The only way to do this is by working shoulder to shoulder, trustingly, with someone who already knows how to do it. But you have only to encourage the novelist analogy your patient starts with. Then he can be counted on to sit around trying to come up with the Great Psychological Experiment rather than rolling up his sleeves and going to work on the problems that his adviser and her research team are working on.

Should you fail to prevent him from getting started on research, you can fall back on a variety of stratagems. A useful one is to make him highly concerned about whose idea he is working on. Make him feel that anything he does in collaboration with his adviser is just hewing of wood and drawing of water. He must be prevented from realizing that the idea will become theirs to the extent that he participates enthusiastically in the research and brings his own concerns and intellectual stamp to bear on it.

If you are successful in nipping his collaboration with his adviser in the bud, keep him in his apartment as much as possible, preferably with a pad of paper in front of him. Bertrand Russell wrote in his autobiography that many a time during the writing of *Principia Mathematica* he would start the day with a blank piece of paper in front of him and end the day with the same blank piece of paper in front of him. This is what you want to aim for. Don't let it occur to your patient to put a good book in front of him so that he can alternately think and read. Sitting down with a book has two effects you want to avoid. First, the book may facilitate his thinking, and second, even if it doesn't, he will at least have done some useful reading by the end of the day.

Your affectionate uncle,
Snidely

(4)

My dear Slump,

It has been some time since I have been able to follow your case closely. We have had a near miss with some physicists who were on the trail of the Third Force. (It has ended well. They are now doing some quite mundane astrophysical work.) But I regret not keeping in closer touch. I regret even more what you have reported.

Your patient has had a distinguished graduate career. He has worked on several promising lines of work, with several faculty members, and he has conducted a dissertation of some value and originality. It does no good for you to say he was unusually talented. If he had wasted his time in school, I have the strong suspicion you would have taken the credit for that.

There is nothing for it now but to wheel up the artillery for dealing with the first year on the job. On this field we have many advantages. Your patient will be quite miserable, having been torn away from friends and familiar environments and placed in a new situation for which he is inevitably imperfectly prepared. He will be absolutely alive with doubts about himself. What you want to do is to fan these concerns into a veritable conflagration. In a well-managed case, you can have a patient literally spending most of his time worrying about things he can do nothing about.

It is a good idea to have him concerned about his intelligence. Such concerns are worse than useless for him. They will make him less likely to seek out his new colleagues and students for fear they will find him out. Suitably isolated, he will actually become less intelligent and of course less productive. We know that intelligence is only one ingredient in productive work and that unusually high intelligence is not at all required for unusually valuable work, but humans assign it a quite unreasonable importance. All but the very most brilliant patients are therefore candidates for constant worry about whether they are smart enough to succeed.

Actually, I have had excellent luck even with the very most manifestly brilliant patients. Remember that such people literally have more to lose than to gain by producing something. They know this, and live in constant fear of doing work that is unworthy of them. No idea, of course, however important, seems obviously valuable or even very sensible in its formative stages. The minute a famously smart young person has an idea, you want to encourage speculation about whether it is really earth shaking if true or really true if earth shaking. The patient will almost never be able to give an affirmative answer to both questions, and incipient research can cut off before it's ever started. Give me a patient with an IQ of 170 and I'll produce mental paralysis by the age of 30 more often than not.

But worries about creativity are the best. Regardless of your patient's real or ascribed intelligence, he is bound to have doubts about his creativity. It is hard to believe, but humans actually think there is a *property* of creativity that one can either "have" or "not have," as opposed to a talent for some field—a love of its content that keeps them thinking about it all the time—organization, and a willingness to work. Because your patient hasn't the foggiest idea what the property of creativity might be or how he would know whether or not he has it, you can keep him constantly searching for signs and flinching at chimeras. And, of course, worries about his creativity will have the same salubrious effect on his work output that worries about his potency will have on his love life.

Should you fail in your efforts to preoccupy your patient with his intelligence or his creativity, you might try the opposite tack and get him to imagine he has talents he does not. It is important to remember how social scientists view the Great Chain of Being. They think that sociologists are on the bottom and mathematicians are on the top. A consequence is that the psychologists are always trying to impress the biologists, the biologists the chemists, and the chemists the physicists. This makes most psychologists into rabid reductionists. They think this is good scientific strategy, but it is really just biologist envy. It is easy to play to this sort of weak-

ness. If your patient has a little mathematical skill, get him absorbed with formalizing his phenomenon before he even has a clear idea what it is. If your patient has an interest in physics, encourage him to theorize by grandiose analogy to natural science models. I might remind you of the success that Pedanthrope had with Richard Nisbett early in his career. He persuaded Nisbett that he was a budding physiological psychologist. This for a man who, in his sophomore biology class, rarely saw anything in his microscope but his own eyelashes and once identified a raisin dropped by a careless student as a hypertrophied earthworm heart![1]

Your affectionate uncle,
Snidely

(5)

My dear Slump,

I was sorry to hear that your patient, though anxious and unhappy, is busily engaged in research. But you mustn't be discouraged by this. They almost all do it, early on.

What is troubling about your patient is that the research is something that he is genuinely interested in, something that proceeds from long-time concerns and that builds both on the thinking and the research skills that he developed in his research with his major adviser. What you want is to have him working on some trendy topic he has picked up by reading the journals. You want him to see some piece of moderately interesting work, get him to thinking that it is ridden with alternative explanations, and have him redo the experiments with the major purpose of showing that he is cleverer than the original investigators. Research is least dangerous when it is most reactive. If you play your cards right, your patient won't be doing experiments about nature at all but experiments about experiments.

Always remember that Einstein has written that the basic notion of relativity was obvious to him at age 15. If Gorgebinder had had any sense at all he would have kept Einstein at work on electrical phenomena until he retired at that patent office. Relativity theory could have remained for decades a childish speculation of an obscure Viennese scientist.

Closer to your patient's field, I remind you that John Garcia's work on the neurophysiology of conditioning was based on an experience in childhood that millions of humans have experienced and then harmlessly forgotten. He had a piece of licorice for the first time at the age of 10 and became nauseated from stomach flu a dozen hours later. He ever after avoided licorice. This led him to do the experiments that extended the possible interstimulus conditioning interval from seconds to hours and established forever that organisms are differentially prepared to learn certain associations. I don't mean to slight the great Bilgegarner. He kept almost a whole generation of learning theorists busy as beavers with utter trivia. But only a few more colleagues snickering about "anecdotes" and "introspections" at the right time could have held the line for us with Garcia.

Still, you have to hand it to Bilgegarner. Garcia's manuscripts were kept from publication by editors and reviewers for years on the grounds of rank implausibility of the phenomenon, not to mention multiple artifactual explanations for the experiments. Imagine the magnitude of Bilgegarner's task given that half of the editors and reviewers would have experienced the Garcia phenomenon themselves and would have found it impossible to explain by the contemporary canons of learning theory.

Once you have steered your patient away from research that is meaningful to him and that makes connections with long-established interests, you want to keep him constantly busy. The human who said that idle hands are the devil's workshop never met a modern workaholic. After you have drained most of the pleasure from his research by making it derivative and reactive, you can absolutely suck it dry by making him do it all the time. Of course I don't mean the zealous pursuit of an idea that makes people work joyously and without exhaustion for days and nights on end. I mean the steady drip of seven days a week, 10 or 12 hours a day, regardless of whether he is working on anything of any real interest to him. You want him eventually to wake up every morning viewing his research with all the enthusiasm of a prisoner for the rock pile.

It will help you to achieve this if you understand the relation between grace and worldly success for 16th century Calvinists. The Calvinist believed that salvation was through grace and that no one could know for sure just which humans had grace. On the other hand, all those with grace were blessed with worldly success. Thus worldly success was an indicator at least of salvation, while having some rewards of its own.

There is a direct analogy here for the relation between creativity and sheer production for the modern professional, especially scientific professionals. They know that what counts is creativity and that all creative people are productive in some sense. They also know that it can be hard to tell just what work is genuinely creative. From there it is only a step to get them to focus exclusively on the productivity and to slide into a tacit equation of productivity and creativity. Here you have the academic reward structure massively on your side. Academic decision makers count notably better than they read. We have recently encouraged the rapid promotion and advancement of many psychologists whose work, whatever its value in terms of quality, is wretchedly excessive in terms of numbers of publications. The *ouevre* for most of these people would be better if there existed only a third of it. I don't mean the best third. That would be true of most scientists. I mean a random third. The importance of our success with these people cannot be overestimated. Given the current temper of the times, young people are sure to regard the meteoric rise of these people as having occurred because of their absurd publication lists rather than in spite of them. And they will not be wrong.

I hasten to point out that different times demand different temptations. In my heyday I would have deflected your patient toward work that was socially relevant. I would have had him ignoring what his talents were, what questions he had an intellectual interest in, and what problems were tractable. I would have had him governing his research interests exclusively by calculations about how much human suffering he could alleviate. Many a talented young person of the 1960s is languishing somewhere, having been broken long ago on the wheel of social conscience, courtesy of your uncle. Some of these people could have done important basic work. To add to that irony, the work would inevitably have had applications. As Lewin said, there is nothing so practical as a good theory.

Scientists have little knowledge about scientific history or they would not be so susceptible to our arguments about social relevance. Euclid had more to do with curing disease than any physician who lived between Galen and Pasteur. This is because geometry made possible the science of optics, which made possible the invention of the microscope, which made possible the germ theory of disease. I am told we actually had some Sophists working on Euclid, trying to lure him into working on leprosy, but the ancient Greeks

[1] Naturally, I resent the slur on what I feel are my genuine achievements in the area of physiological psychology. The comments about my performance in biology class are sheer fabrication and raise serious questions about the veridicality of all allegedly factual assertions by Snidely. (Ed.)

placed too much value on intellectual curiosity for this to have had much chance of success.

I have digressed to discuss the topic of different strategies for different eras because there is sometimes an undercurrent to your letters implying that I am out of touch with modern culture and that my advice is out-of-date. Your opinion here is as mistaken as it is unwelcome. I think I read the papers as well as you. In the age of Tom Hayden one plays on social conscience. In the age of Ivan Boesky, one invokes capitalism and the spirit of John Calvin.

But back to work. Your patient's work I mean. Ideally, you want your patient working on research all the time, the better to sterilize it for him. But, realistically, even if he is obsessed with work, he can't do research continually. What you want to do is make any other work he does as barren for him as possible. Get him to take the same driven stance toward preparing his courses that he does toward his research. Put him into an endless round of overpreparing his lectures and tracking down minutiae that won't interest his students in the least. When this is done right, little research is accomplished, the teaching gets steadily worse, and the patient is a bitter, burnt-out case by the time he is denied tenure.

But it must be admitted that getting your patient to spend too much time on teaching can be difficult. Teaching is not rewarded by the academy, and your patient knows this unless he is a fool. It may be more profitable to get him to take the opposite tack and have him avoid teaching as much as possible, and doing it as grudgingly as possible when it is required of him. It is important to remember that teaching in moderation is actually valuable for research. The necessity of explaining one's concerns to others, and of putting them into a broader context, together with the effort to demonstrate why certain topics are interesting, all have the most direct benefits for thinking about research. We have had great success with scientists at the modern research institutes who are relieved of all need for teaching. Most of them are turning out to cease all productive activity within a matter of a few years after arrival.

But the overarching principle here is to prevent the patient from doing things that are intellectually stimulating or pleasurable. After describing Tom Sawyer's famous achievement in getting his friends to whitewash his fence for him, Mark Twain observes that "work is what a body is obliged to do." You want your patient doing only that which he feels obliged to do, thereby thinking of it as work, and regarding everything that is intellectually stimulating as a distraction.

It is particularly important to prevent him from talking with his colleagues. A cup of coffee with a stimulating colleague can be a disaster. It can easily result in a new idea, a new perspective, a new career! And yet it is so easily avoided. Busy, busy. Not today. Nose to the grindstone.

Always remember that few psychologists spend more time just sitting and talking with people than Amos Tversky. I am sorry to mention his name. The topic is as painful to me as it is to you. But we can learn much from anecdotal reminders about our worst failures.

I hate to end on a depressing note, but this letter is beginning to be as long as if it had been written by one of our cherished workaholics.

Your affectionate uncle,
Snidely

(6)

My dear Slump.

I think you are quite mistaken to cackle over the fact that your patient has fallen in love. It is true that love affords innumerable distractions and that he will be quite incapable of work during the inevitable troughs in the relationship. But on balance we have more to lose than we have to gain. Freud pointed out that the primary concerns of the therapist, the domains of life that are most important, are love and work. And no one was more aware than he of the intimate link between these. When a human's love life is going well, work is likely to be carried out with special joy and contentment. These emotions are our deadly enemy.

You may claim that it is mere coincidence, but it is certainly the case that your patient is doing suspiciously interesting work lately. In any event, you are to put a stop to it by any means possible. I hope I make myself abundantly clear.

Your affectionate uncle,
Snidely

(7)

My dear, my very dear Slump, my munchkin, my morsel,

The grim reports I have had to read today are tempered only slightly by amusement at your whimpering about whether my friendly tone and expressions of concern meant nothing from the very beginning. Oh yes, friendship and concern. We will return to those shortly.

But now for the contents of your report. You have let a man get launched on a series of investigations that can only end in disaster. The research is novel and important. Others will be able to make immediate use of the findings and the theory to enrich their own work. The research has obvious applications to significant social questions. His experience with this work will undoubtedly lead him to other important topics and will give him invaluable confidence to sustain him over slack periods. Almost the worst of it, he is having the time of his life. All of us down here feel the agony of the fun he is having, from the divisional secretary to the distraction agents. You will soon feel a particularly pungent form of this emotion.

We have nothing really left at our disposal for this man but a last ditch effort to convince him that he did the work only to get tenure. This has been known to turn around several careers that were particularly threatening.

But you, of course, will not be entrusted with this task. Or with any other task of its sort. My unhappiness at your failure is mitigated only by my amusement at contemplating your reassignment. My friendship and concern will be conveyed most convincingly, I trust, by the letter that awaits you here describing your new duties. As a hint, I shall point out that you will soon be yearning for the days of your previous career as an assistant boredom clerk.

Your affectionate, your friendly and most deeply concerned uncle,
Snidely

Reaction to "The Creation and Destruction of Value"

Theodore C. Kent
San Diego, CA

The article on value (Schwartz, January 1990) gives readers a needed prodding to think about the broad, philosophically oriented question of creating versus destroying value in our human interactions. Writings on questions of values, ethics, and social lifestyles accompanied by Schwartz's innovative and challenging insights deserve applause. My objection to the article is that it may raise false hopes.

Schwartz's perception of a decent society and his recommendations for achieving it are similar to what has been proposed at various times throughout history. For example, in 1749, Rousseau won a prize for his essay contending that humankind had been corrupted by civilization. Later, when the "noble savages" he envisioned became better known, his thesis of the grandeur of primitive life versus civilization was repudiated. Within a dif-