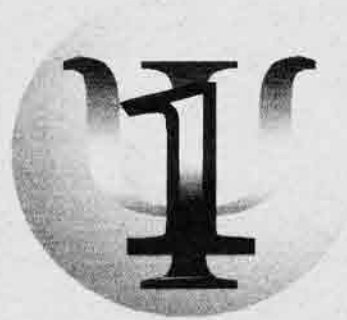


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Inside

Back again at the same old stand! This issue brings again some treasured remnants of last year's convention program along with some newly minted pieces. We have convention talks by Ralph Rosnow and Howard Rachlin, interviews with Phil Zimbardo and Bill Bevan, a very special book review by Bob Perloff, additional psychological trivia, along with some business stuff including a message from our President Lewis Lipsitt. Some late breaking news: you have elected Linda Bartoshuk to be the next President-Elect of the Society, Frank Farley and Wendy Williams to serve as Members-at-Large of the Executive Committee and Greg Kimble to return as Representative to the APA Council. Congratulations to them and to your perspicacity in electing them. On with the show. Enjoy!



Hedgehogs, Foxes, Ethics, and the Evolving Social Contract in Psychology

Ralph L. Rosnow
Temple University

I am deeply grateful to Division 1 for honoring me with the George A. Miller Award. The article designated for this award was published in *Psychological Methods* in 1997. I thank Mark Appelbaum, editor of the journal, who invited me to submit the paper, and Howard Sandler, who patiently shepherded it through the review process. I also thank Temple University for the continuous support that I have received through the Thaddeus Bolton Professorship.

Emerson once wrote: "We want but two or three friends, and these we cannot do without, and they serve us in every thought we think." Mimi Rosnow, Bob Lana, and Bob Rosenthal are for me the embodiment of that sentiment. Bob Rosenthal and I first met when we were young assistant professors at Harvard University and Boston University, respectively. Over 35 years, Bob has improved my thinking about research methods to fill a baker's dozen books together, including our most recent, coauthored with Don Rubin, *Contrasts and Effect Sizes in Behavioral Research: A Correlational Approach*. Later on, I will have a little more to say about this approach. Bob Lana, my Ph.D. adviser at American University from 1960-1962, and my Temple University colleague for 32 years, whetted my interest in the epistemological foundations of psychology, introduced me to the joys and complexities of experimental social psychology, and is the reason I moved to Temple University in 1967, to help him develop a new doctoral program in social psychology. Mimi Rosnow has been my constant intellectual and emotional companion longer than I am permitted to tell you. She is the creative person who conceived the terms "wish rumor," "dread rumor," and "synthetic benevolence," and is the reason why many students find *Writing Papers in Psychology* so accessible. I am profoundly indebted to all three for their generosity of spirit, penetrating insights, and tireless mentoring, which have enriched my life and my work.

Some years ago, George A. Miller wrote that the role of scientific psychology was to promote human welfare,

and urged individual psychologists to instill scientific facts about the potential of human nature in the public consciousness. My article in *Psychological Methods* was addressed in large part to new researchers who, in their efforts to use the special tools of science to produce such facts, felt burdened by an expanding body of bureaucratic rules and regulations. Of course, even old researchers can find themselves caught in a bind between the methodological demands of science and the moral sensitivities of society. The "hedgehogs and foxes" reference in the title goes back to the poet Archilocus, who in 650 B.C. stated, "The fox knows many things, but the hedgehog knows one big thing." I first encountered this reference in a classic essay by Isaiah Berlin, who compared Dante, Plato, Hegel, and Nietzsche's single central focus with Shakespeare, Aristotle, and Goethe's pursuit of many visions on many different levels. My generation of researchers

Address by 1999 winner of the George A. Miller Award of the Society for General Psychology at the APA Convention in Boston, August, 1999. The Award is for an excellent recent paper relevant to General Psychology. Dr. Rosnow's article by the same name appeared in *Psychological Methods*, 1997.

(and earlier ones) belonged to the hedgehogs, embracing a single central vision of science as an "endless frontier," to quote Vannevar Bush's famous phrase. Because researchers today must navigate labyrinthine byways of an evolving social contract of moralistic do's and don'ts, and still comply with technical scientific demands, they must become foxes, that is, move on many levels and pursue many visions.

Broadly speaking, the social contract between psychological science and society can be described as the responsibility not to do psychological or physical harm to any of our research participants and to do beneficent research in a way that will produce valid conclusions. Often, however, this is easier said than done. Confidence in the validity of scientific facts may be jeopardized by the suspicion of subject or experimenter artifacts, but controlling for artifacts calls for a delicate balance between methodological and ethical concerns. Recently, Bob Rosenthal and I revisited this

theme in *People Studying People: Artifacts and Ethics in Behavioral Research*. We began by quoting the social scientist Otto Neurath, one of the founders of the Vienna Circle:

"We are as sailors who are forced to rebuild their ship on the open sea, without ever being able to start fresh from the bottom up. Wherever a beam is taken away, immediately a new one must take its place, and while this is done, the rest of the ship is used as support. In this way, the ship may be completely rebuilt like new with the help of the old beams and driftwood—but only through gradual rebuilding."

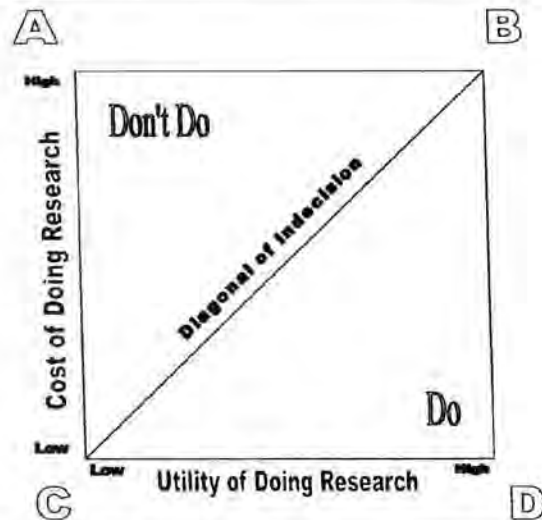
We added that, just as old beams must constantly be replaced by new ones, so must the old sailors be replaced by the new. The new sailors must learn from the old what factors weaken the beams and how they are to be replaced. Because artifacts in research weaken its beams, each generation of sailors must learn to spot them in order to improve the ship of science. Although replacing old beams is essential, it can be dangerous. Passengers can be hurt as the ship is improved, so sailors must learn how to sail safely, harming no one aboard. Understanding the capabilities and limitations of our methodological tools helps keep the ship seaworthy; sensitivity to moral values keeps the sailors worthy and the passengers safe. That there are moral underpinnings of science is now generally taken for granted. This was not always the case, especially during the heyday of the positivist movement. According to the prevailing image, scientists were disinterested seekers after truth, characterized by a "see no evil, hear no evil" impartiality. Science is morally neutral by its very nature, it was said, because the moment it begins sorting facts into "good ones" and "bad ones" it is no longer science. Some theorists drew a distinction between natural and behavioral science, arguing that although moral detachment was conceivable in natural science, it was impossible in behavioral science because of the societal implications of psychological facts. For example, when we speak of behavior as "normative," the implication to a layperson is that we mean the behavior is to be expected, or is morally desirable. When we disseminate facts about prejudice, mental illness, or child abuse, we are touching on societal issues that are highly charged with moralistic implications. Even when we comment on topics using facts that appear to us to be morally neutral (learning behavior, for example), to others those facts may be supercharged with values and conflicts. As the history of science teaches, the assumption that natural science is morally neutral crumbled with the development of atomic physics. Ultimately, unrest surfaced in all disciplines about the motives, purposes, and moral implications of research itself. In psychological science, going back to the 1960s, leading spokespersons had voiced concerns about the status of human values in research. Like the impassioned plea of the Tin Woodman in *The Wizard of Oz*, who prayed to be given a heart, there were ardent pleas to give our science a humanistic heart. In 1966, the APA created a task force which was assigned to compose a code of ethical principles for research with human participants. Out of those deliberations came the ten basic guidelines

which researchers have euphemistically referred to as the "Ten Commandments of APA." A favorite quote from that period was Kenneth Gergen's forewarning:

"Most of us have encountered studies that arouse moral indignation. We do not wish to see such research carried out in the profession. However, the important question is whether the principles we establish to prevent these few experiments from being conducted may not obviate the vast majority of contemporary research. We may be mounting a very dangerous cannon to shoot a mouse."

What Ken labeled a "dangerous cannon" now seems like a popgun in light of pressures, obstacles, and priorities imposed on researchers by daunting arrays of ethical considerations, bureaucracies, formalities, and legalities that did not exist in their present form in the 1960s. As the APA's proposed revision of its ethical guidelines for research implies, virtually every aspect of the scientific agenda may be perceived as value-laden to some degree, from the selection of a research topic, through the conceptualization and implementation of the study, to the statistical analysis, interpretation, and reporting of results.

In his recent book, *Ethical Issues in Behavioral Research*, Allan Kimmel discussed the various codes of ethics that have been adopted by psychological associations in the U.S., Canada, France, Germany, Great Britain, the Netherlands, Poland, and elsewhere. Clearly, psychological research around the globe is bounded by professional rules and guidelines. In the U.S. we have become accustomed to having IRBs play the role of gatekeepers, supervising the flow of institutional research. A few years ago, while serving on the APA's Committee on Standards in Research, Mary Jane Rotheram-Borus, Steve Ceci, Peter Blanck, Gerry Koocher, and I wrote in the *American Psychologist* about the vagaries and limitations of the decision process involved in IRB ethical evaluations. The idealized decision-plane model in the figure, which Bob Rosenthal and I first published some years ago, will help me explain one aspect of this problem. It shows



how the cost of doing research might be simultaneously evaluated against the utility of doing it. Presumably, a study falling in the region labeled "D" (that is, high utility and low cost) would be approved by an IRB, whereas a study in the area labeled "A" (low utility and high cost) would be rejected. On the B-C axis would be studies in which the perception is that the costs and utilities are balanced, thereby changing the decision from easy to difficult. In the case of low-cost, low-utility research, however, an IRB might be unwilling to approve a study that it viewed as harmless but unlikely to yield any substantial benefit.

As most of us know from personal experience, the gatekeeper process is far less reliable. Some years ago, Steve Ceci, Douglas Peters, and Jonathan Plotkin sent sample research proposals to the chairpersons of several hundred IRBs, asking for their candid assessments. The proposals outlined a research investigation of discrimination in the hiring of managerial positions by Fortune 500 corporations. Some proposals, by mentioning instances of discrimination or reverse discrimination, were intended to appear highly sensitive; others, by documenting discrimination based on height or weight, were intended to appear less sensitive. The proposals constructed to seem highly sensitive were twice as likely to be rejected by the IRBs, the primary reason given for rejection being the political impact of the anticipated results. It frequently seems that IRBs ignore utilities and merely use the A-C axis value as their criterion, as in this case. But even when utility is a consideration, the idealized model is insufficient because it ignores the costs of research not done. By concentrating only on the act of "doing research," and ignoring the act of "not doing research," the decision process concedes to a less rigorous standard of accountability than that aspired to by most researchers. Another example would be if an investigation designed to reduce the risk of HIV infection were rejected on the grounds that the proposed methodology could not guarantee the privacy of the participants. Likewise, rejecting a study that involved a deception experiment to reduce violence or prejudice would not solve the ethical problem, but would merely trade one moral issue for another.

I do not mean to imply that researchers should try to circumvent the review process, but (as discussed more fully in my article) I think we need to generate ideas and implement a method to improve this process. The Committee on Standards in Research was dissolved by the APA in 1993, but in our final report we made an appeal for the development of a mechanism to keep IRBs abreast of emerging issues in psychological science and to raise their consciousness about the costs to science and society of sensitive research not done. We also argued for speedy access to appeals when IRBs are subject to political pressures that restrict their judgments.

As Donald Bersoff, Joan Sieber, and other leading psychologists have observed, there are exploitable research opportunities in ethical dilemmas. Twenty-five years ago, Bob Rosenthal and I alluded to

one such opportunity in our book *The Volunteer Subject*. In an early example of meta-analysis (before the term was coined by Gene V Glass), we used pooled empirical data to tease out ethical-enhancing strategies for reducing volunteer bias by stimulating research participation. Daniel E. Koshland, in an editorial in *Science* magazine a few years ago, noted a medical case in which, it would seem, such strategies could be advantageous. Suppose a study of cholesterol in the diet needed volunteer subjects, but because people at high risk were most likely to volunteer, it could be dangerously risky to generalize from this group to a more normal one. On the other hand, if there were ethically acceptable ways to improve the rate of participation by the reluctant nonvolunteers, that should also improve the generalizability of the research sample. In our book, Bob and I recommended a number of such strategies, including making the appeal as interesting as possible to capture people's attention, making it as nonthreatening as possible to avoid eliciting unwarranted fears, and emphasizing the theoretical and practical significance of the research to encourage willingness to participate. Using these empirically justified strategies should not only reduce the amount of volunteer bias, but also make us more thoughtful scientists, because they will improve our relations with participants. Moreover, if we were to tell potential participants as much as possible about the significance of our research — as though they were another granting agency, which in fact they are, granting us time instead of money — we would have to do significant research.

In the remainder of my talk I would like to examine another facet of the ethical dilemma, which I call the "waste not, want not problem," based on an argument that Bob Rosenthal raised in *Psychological Science* a few years ago. He argued that squandering scientific opportunities in poorly designed and weakly analyzed research makes for bad ethics because scientific data are expensive in terms of time, effort, money, and other resources, and he mentioned a number of ways to improve this situation. Consistent with that waste management objective, I will examine four aspects of this problem within the framework of four metaphorical principles: the reader's lament, the scientist's key, Tarzan's leap, and the dayyan's decree.

First, one way that waste can occur is when investigators succumb to what statisticians James O. Berger and Donald A. Berry called "the illusion of objectivity" in statistical analysis, unwittingly work with low power, and conclude there was "no effect" merely because they found p to be on the "unpopular" side of .05. Perhaps we need to remind ourselves and our students of the following conceptual relationship: **significance test = size of effect X size of study**. It shows that significance tests (such as t , F , and chi-square) can be understood as the product of two components, one having to do with the size of the effect (as measured, for example, by Cohen's d , Hedges's g , or the product-moment r between the independent variable and the dependent variable) and the other with the size of the study (i.e., the number of sampling units). The

equation reminds us that it is easier to claim the presence of a phenomenon of any given magnitude at the desired significance level when working with a larger N than with a smaller N . Thus it is crucial to realize that finding "nonsignificance" is not the same thing as finding "no effect," as it is quite possible for a meaningful effect to be present although the significance test lacks sufficient power to detect it at the desired significance level due to modest N or an imprecise design. By the reader's lament, I mean that significance testing with insufficient power is like switching off the light just as you sit down to read a good book. Claiming there is no book would be deceptive, and wasteful. Reporting the magnitude of any obtained effect (and its confidence interval or null-counterfactual interval), or providing the raw ingredients so others can compute these values, allows research consumers to make up their own minds about whether there was any meaningful effect.

Second, another problematic situation concerns the bias against publishing replications, which is further exacerbated by the publish or perish tradition because it encourages researchers to submit single studies. It is a giant leap of faith, however, to assume that because one found a statistically significant result, it means the result is reproducible and generalizable. As practical experience has taught most of us, a psychology experiment conducted with one group of subjects in one place at one time may yield results very different from the "same" experiment conducted with another group of subjects in another place at another time. Even though reproducibility is almost universally accepted as the most important criterion of genuine scientific knowledge, it is precariously rare in some areas of our science. In a study reported in the *International Journal of Research in Marketing* in 1994, Raymond Hubbard and J. Scott Armstrong noted that of 1120 papers they sampled from three influential journals in marketing research, none were replications. Just under 2% were what they called "extensions," of which only 3 papers provided full confirmation of the original results. Without repeating experiments we cannot, as Bill McGuire wisely recommended, continue the discovery process by clarifying and expanding the meaning and limits of hypotheses. Replication can be understood as a waste control strategy because, as Hubbard and Armstrong observed, it protects the literature from the uncritical acceptance and dissemination of erroneous and questionable results. We have a collective responsibility, they concluded, "to ask whether a given result is plausible, reproducible and/or generalizable."

This brings me to the principle I called "the scientist's key," which takes its name from something that James B. Conant, an experimental scientist, once wrote. He said the scientist is like someone who is trying to unlock a door using a hitherto untried key. The role of replication, we might say, is to make the key available to competent others so they can learn for themselves what is behind the door. I do not mean reproducing the identical p value, but observing a similar relationship or seeing the same phenomenon. Suppose you were jogging around Boston Common one morning and

spotted two Martians — not two people disguised as Martians, but real Martians: green skin, antennas poking out of their scalps, etc. You aren't going to whip out your calculator, but you sure are going to ask somebody, "Do you see what I see?"

Third, it would also be prudent to temper our overreliance on omnibus statistical tests, which we were taught as graduate students to regard as protection from the dangers of "data mining." Unfortunately, omnibus tests can trick unsuspecting researchers into abandoning the focused question of interest. To illustrate, suppose a student who was working on his thesis at Seattle University was interested in assessing two theories, A and B , each implying a specific prediction about how many counseling sessions it will take to improve psychological functioning in parents of children with serious illness. Theory A predicts a minimum of four sessions to produce any benefit, and that any fewer than four will be futile. Theory B predicts small benefits as early as the first session, with gradual improvement expected to continue throughout all four sessions. The student designs a randomized experiment consisting of four groups, corresponding to 1, 2, 3, or 4 sessions of counseling. So far, so good. Unfortunately, instead of inspecting his results more closely, the student computes an omnibus F test. To his dismay, he finds the associated p is not significant at .05, assumes it means there was no effect, and ends up sleepless in Seattle worrying that he will never get a degree.

The trouble with omnibus statistical tests is that they seldom tell us anything we really want to know. All the while that a particular predicted pattern may have been evident to the naked eye, the student's reliance on an omnibus F may have led him to miss the forest for the p . Instead of addressing the two focused questions of interest, he addressed a diffuse question of dubious scientific value. Science is not a Simon says game in which researchers must seek permission from the p value associated with an omnibus F before they can look for answers to specific questions. The student's 3 df - F was too nebulous to be of value to him, because the omnibus F would be the same whether he were interested in the prediction implied by Theory A or Theory B . Moreover, informative effect size indices such as Cohen's d , Hedges's g , and the product-moment r cannot be gotten from an omnibus F . Relying on omnibus F tests can be hazardous for many researchers, and may become even more perilous when there are several dependent variables and multiple degrees of freedom for the independent variables. This leads me to the principle I call Tarzan's leap, which refers to something Johnny Weissmuller, who played Tarzan in the movies, said. Asked about the nature of the apelike skills he displayed while swinging through the trees, he replied "the main thing is not to let go of the vine." It is good advice for researchers who let go of their specific hypotheses prematurely, often without ever testing them.

Returning to the insomniac student, what he should have done was to calculate contrasts, which would have allowed him to address the competing predic-

tions in a precise way. To do so, he would begin by expressing the predictions as integer lambda values that sum to zero. Theory A predicted no benefits prior to four sessions, but a substantial benefit after Session 4, which can be stated in terms of lambda weights of -1, -1, +3. Theory B predicted a continuous linear increase of benefits, which can be expressed by lambda weights of -3, -1, +1, +3. Now all that is needed is to plug the weights into the correct formulas, calculate meaningful indices of effect size, construct confidence intervals or null-counter null intervals around those effect size indices, and interpret the results. If you are not familiar with the counter null value, it refers to the nonnull magnitude of the effect size that is supported by the same amount of evidence as is the null value of the effect size. The null-counter null interval implies whether conclusions of "no effect" might be in error, thereby providing some protection against the anguish that may be experienced by those who balance their "accept/reject" decisions on the razor's edge of the 5% p.

Incidentally, an easy way to create appropriate X weights is to estimate the mean outcome in each group, then subtract the overall mean from each group mean to produce weights that sum to zero. In the case of Theory A, suppose we predicted condition means of 0, 0, 0, 4 for sessions 1, 2, 3, 4, respectively. Subtracting the overall mean of 1 from the four means gives us weights of -1, -1, -1, +3. For Theory B, suppose we had predicted condition means of 1, 2, 3, 4 for the four "dosage" levels. Subtracting the overall mean of 2.5 gives us -1.5, -0.5, +0.5, +1.5, which multiplied by 2 gives us lambda weights of -3, -1, +1, +3. A useful post hoc procedure (just as easy to do) is to correlate the weights and the obtained group means. Bob Rosenthal, Don Rubin, and I call this aggregate correlation an "alerting r " because it can alert us to overall trends of interest. Another useful feature of the alerting r is that squaring it reveals the proportion of the overall between-condition sum of squares that can be accounted for by the particular set of weights. With this information, we can easily carve a contrast out of the omnibus F to address the predicted pattern. We can find out whether, given the particular circumstances of the investigation, Theory A was a better predictor than B, or B was better than A, or neither theory did particularly well, or both theories did equally well.

Finally, another cost conscious, waste management strategy would be to allow for the distinct possibility of more than one valid theoretical explanation. You may be familiar with the scientific approach called "strong inference" by the physicist John R. Platt, in which the strategy is to pit one theory against another in a game of empirical jeopardy and eliminate the loser. This approach may have worked in the gladiatorial combat of ancient Rome, and may work well in physics, but it can be wasteful in psychological science because behavior is often "pushed" and "pulled" by more than one determinant. The principle of the dayyan's decree is sensitive to this problem. The name comes from a Yiddish anecdote about a rabbinical judge who was asked by a married couple to settle a conflict in which

they were embroiled. The wife told her side and the dayyan said, "You are right." The husband told his side and the dayyan again said, "You are right." An incredulous talmudic student who overheard the conversation addressed the dayyan: "Rebbe, you really mean they are both right?" The dayyan replied, "You are right, too." Many years ago, Donald Campbell and Julian Stanley advised researchers that when two well-grounded theories disagree, both may be right to some degree. The lesson of the dayyan's decree is not to assume there is only one valid explanation, and lambda weights and contrasts can again serve us exceedingly well by helping us ascertain the degree to which each theory is applicable in a given situation. Likewise, if we were interested in finding out how well the two competing predictions fare together, we could compute a contrast using the combined, Z-scored lambdas.

I began by referring to one facet of the work of George A. Miller, after whom this award is named, and it is fitting that I conclude by mentioning another side of his work. Thirty years ago, writing in the *American Psychologist*, he proposed a code of priorities for assessing the costs and benefits of new technologies before they were introduced to society at large. Another giant of psychology, Gordon Allport, estimated the life of popular concepts in psychology to be about two decades, after which, he said, "they begin to taste as flat as yesterday's beer." Miller's work was far above average; the eleven criteria he proposed in that article remain as vital today as they were three decades ago: validity, intelligibility, reliability, social relevance, safety, accountability, the use of informed consent, avoidance of deception, and emphasis on individuality, availability, and distributability. I would suggest adding a twelfth to that list, *precision in specification*, so that both we and society have an accurate picture of what we are prepared to instill in the public consciousness. Contrasts and effect sizes encourage us to be precise in our thinking and writing, and provide an easy-to-use methodology for doing so. Used wisely and ethically, they will allow us to go a long way in fulfilling the benevolent vision that Miller articulated so eloquently.

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THE PRISONER'S DILEMMA AND SELF-CONTROL

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For the last ten years or so, whenever I present the results of my research on self-control at university colloquia or conferences, I begin by saying that I want to give the audience a phenomenal experience of complex ambivalence by means of playing a game. Index cards are then handed to 10 randomly selected people and the others are asked to imagine that they had gotten one of the cards. They will be asked to choose among hypothetical monetary prizes by writing either **Y** or **X** on the card. The rules of the game (projected on a screen behind me while I talk) are as follows:

1. If you choose **Y** you get \$100 times N .
2. If you choose **X** you get \$100 times N plus a bonus of \$300.
3. N equals the number of people (of the 10) who choose **Y**.

A player in this game (a "multi-person prisoner's dilemma") would always get \$200 more by choosing **X** than by choosing **Y**. Choosing **X** rather than **Y** would decrease N by 1 (rule #3), losing \$100; but by choosing **X** the \$300 bonus would be gained (rule # 2). The resultant is a \$200 gain for choosing **X**. Logic thus says to choose **X** and any lawyer would advise a player to do so. The dilemma (the source of ambivalence) is that if everyone followed the advice of their lawyers and indeed chose **X**, $N = 0$, and each would get \$300; while if everyone ignored the advice of their lawyers and chose **Y**, $N = 10$ and each would get \$1,000. Thus, **Y** choices ("cooperation") benefit the group as a whole while **X** choices ("defection") benefit the individual at the expense of the group.

At this point I ask the 10 people holding cards to make their choices, imagining as best they can what they would choose if the money were real, and letting no one else see what they have chosen. Then I collect the cards

and hold them until I finish my lecture. I have done this demonstration or its equivalent dozens of times with audiences ranging from Japanese psychologists to Italian economists. The result is about an even split between cooperation and defection, indicating that the game does create ambiguity.

Figure 1 (labels in bold typeface) represents the con-

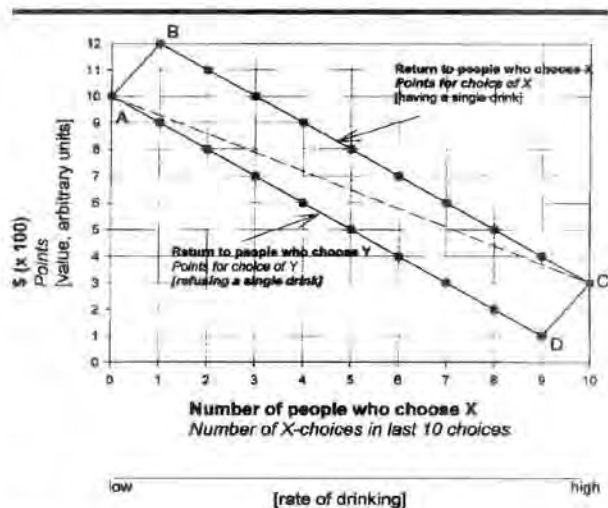


Figure 1.

tingencies of the prisoner's dilemma game that I ask my audience to play. Point **A** represents the condition where everyone cooperates. Point **C** represents the condition where everyone defects. The dashed line represents the average (hypothetical) earnings per person at each value of N . Clearly, the more people who cooperate, the greater the average earnings. But, as is shown by the two lines, **ABC** (representing the return to each player who defects) and **ADC** (representing the

¹This talk is an abbreviated version of a chapter in my forthcoming book, *The Science of Self-Control*, to be published in April, 2000, by Harvard University Press. The research was supported by grants from the National Institute of Mental Health and the National Institute on Drug Abuse.

return to each player who cooperates), an individual always earns more by defecting than cooperating.

Suppose, instead of hypothetically giving money to each player, I instead pooled the money each player earned (still hypothetical) and donated it to the entertainment fund of whatever institution I were lecturing at. Given this common interest it would now pay for every individual to choose Y; a choice of Y by any individual would increase N by 1 for all 10 players, gaining \$1,000 at a cost of the individual player's \$300 bonus, for a net gain to the pool of \$700. A common interest thus tends to reinforce cooperation in prisoner's dilemma games.

Biologists have speculated that the degree of common interest between organisms is fundamentally reflected in their shared genes. The innate tendency of any pair of organisms to cooperate with each other would then depend on the degree to which their genes overlapped. To the degree that closeness of familial relationship correlates with genetic overlap, innate tendency to cooperate should be greatest within families and decrease as overlap decreases in the population. The analogy between social cooperation and self-control may be seen as an the logical conclusion of this argument since each organism shares all of its genes with itself. Figure 2 illustrates the analogy. In Figure 2 a single person at successive moments in time, ranging from past to future is compared to a person in a group of other people. Clearly, these conceptual individuals (Person, P , at t_{-4} , ..., t_{-1} , t_0 , t_1 , t_2 , t_3 , t_4) have a common interest; they all inhabit the same skin. Consistent choice of Y benefits P over a sum of times even though each particular choice of Y imposes a sacrifice at t_0 (now). Consistent choice of X does the reverse. In the language of the prisoner's dilemma, a person who chooses Y in a self-control experiment is cooperating with his or her past and future selves while a person who chooses X is defecting from their interest.

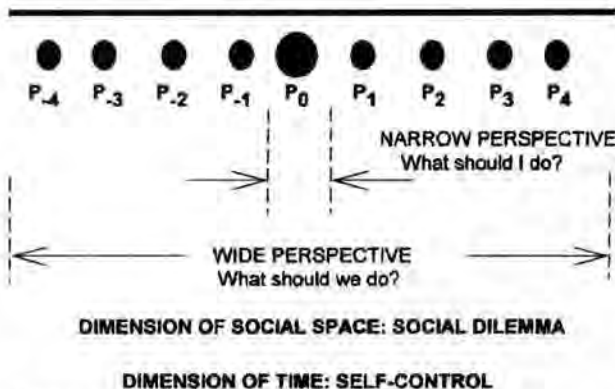


Figure 2

The contingencies of the above demonstration of ambivalence in social cooperation belong in the category of "primrose-path" experiments. In the social cooperation situation illustrated in Figure 1 (bold typeface) many subjects each make a single choice between X and Y. In primrose-path experiments, on the other hand, a single subject makes repeated choices between X and Y. The rules of the primrose path experi-

ment, usually not told to the subjects, parallel those of the social cooperation experiments. A typical set of rules follows:

1. Each choice of Y gains N points (convertible to money at the experiment's end).
2. Each choice of X gains N points plus a bonus of 3 points.
3. N equals the number of Y choices in the last 10 trials.

Figure 1 (italic typeface) illustrates these contingencies in a corresponding way to social cooperation. The reward for choosing X is always greater than that for choosing Y, but overall reward (proportional to the ordinate of the dashed line) would be maximized by repeatedly choosing Y. Ambivalence (reflected in social cooperation dilemmas as nonexclusive choice between X and Y across subjects) would be reflected, in primrose path experiments, as nonexclusive choice by individual subjects across trials. Indeed, in these experiments, subjects generally distribute choices non-exclusively across X and Y.

Complex as it is, Figure 1, is a highly simplified picture of real-world complexity. Lines AD and BC need not be parallel or straight or even monotonic. High rates of consumption, harmful in one context, may be not harmful, or may be beneficial, in others. Nevertheless, the ambivalence represented by Figure 1 is real and captures everyday-life problems of self-control.

The labels in brackets in Figure 1 illustrate the application of this model to alcoholism. Let us say that point A represents a low rate of drinking (one or two glasses of wine with dinner). Dinner would be more enjoyable, however, with three glasses of wine and perhaps a cocktail beforehand (point B). But this much drinking every evening might interfere with sleep, or cause a hangover the next morning, or be slightly damaging to health. That is, notwithstanding the distinct pleasure of the extra drinking, the average value of the drinker's state over time (the dashed line) would be ever so slightly lower one unit to the right of the ordinate than on the ordinate. Further increases in the number of drinks before, during, or after dinner (or instead of dinner) would always be immediately preferable to continuing at the lower rate but, if repeated day after day, would bring average value over time lower and lower (moving to the right on the dashed line). Eventually, at point C, drinking would serve only to prevent the misery of descent to point D. In other words, positive reinforcement, in going from point A to B by the social drinker having an extra drink, would have been replaced by negative reinforcement (avoidance of point D) in staying at point C by the alcoholic continuing to drink at a high rate.

Self-control as an intrapersonal prisoner's dilemma. Consider the contingencies of the 2-person prisoner's dilemma diagramed in Figure 3a. If both players cooperate, both get 3 points; if both defect, both get two points; if one cooperates while the other defects, the cooperator gets 1 point while the defector gets 4 points. Figure 3b (labels in bold typeface) diagrams the game

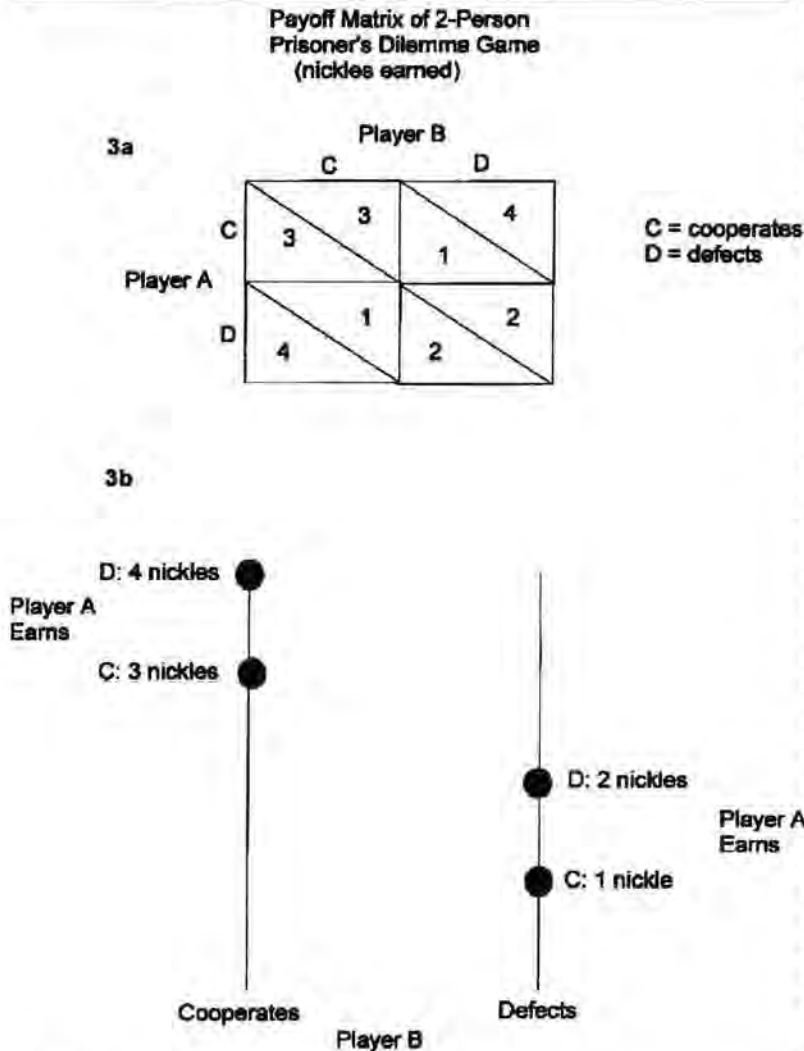


Figure 3

in a corresponding way to Figure 1, revealing the motivational ambivalence. As in Figure 1, defection results in a higher immediate reward and a lower reward in the long run while cooperation results in the reverse. Regardless of the other player's choice, it is always immediately better to defect than to cooperate; if the other player has cooperated then a player will gain 4 points by defecting and 3 points by cooperating; if the other player has defected then a player will gain 2 points by defecting and only 1 point by cooperating. If communication between players is against the rules, if the game is to be played only once (and no similar cooperative tasks are ever expected to be undertaken with the other player), then the motive to defect should predominate. However, if there were some way to get the other player to cooperate, then whatever it takes to do this should predominate over defection because the gain from the right to the left line of the parallelogram in Figure 3b averages 2 points while the gain from the lower to the upper point (from cooperation to defection) averages 1 point. The best set of circumstances would

be to defect while the other player cooperates, earning 4 points. This is an unlikely scenario since the other player would then earn only 1 point. However, if communication were within the rules, it would be possible to compromise by agreeing to mutual cooperation, earning 3 points each (the highest pooled score). Or, if the game were to be played many times, it would be possible to reinforce the other player's cooperation by cooperating, and to punish the other player's defection by defecting. This strategy is called "tit-for-tat." A line from C to D would show average points gained in repeated trials against tit-for-tat with a distribution of choices proportional to the horizontal distance between the vertical lines. For example, alternation of cooperation and defection would yield 4 points and 1 point alternately for an average of 2.5 points per trial against tit-for-tat. The highest long run average (hence the best strategy against tit-for-tat) is to cooperate on all trials.

The crucial variable influencing cooperation in 2-person games seems to be reciprocation. This is also true in games with more than 2 players such as illustrated in Figure 1. The tit-for-tat strategy imposes a strict reciprocation and thus engenders cooperation. Prior communication enhances reciprocation and thus has the same effect. On the other hand, when reciprocation is low or nonexistent, as when the other player plays randomly or always cooperates or always defects, cooperation deteriorates.

Outside of the laboratory, cooperation or defection among genetically similar or different organisms may in turn depend on high or low reciprocation probability. That is, genetic overlap (beyond specific mechanisms governing such innate activities as sexual behavior or parental care) may act fundamentally on reciprocation probability. Genetically similar organisms may have a greater tendency to reciprocate each other's acts of cooperation, and of defection as well, than do genetically dissimilar organisms. If this were true then, at the extreme, an organism should tend to reciprocate its own acts of cooperation and defection over time much more than the cooperation and defection of others. If cooperation depended on reciprocation it would follow that (holding everything else equal) self-control (cooperation with one's future self) should be stronger than social cooperation (cooperation with others). The first series of experiments to be discussed below studies both self-control and social cooperation in a corresponding situation and verifies that cooperation is higher in self-control than in social cooperation situations. The second series

of experiments varies reciprocation probability directly and shows that this variable strongly determines whether subjects cooperate or defect. Reciprocation probability is thus a plausible candidate to be the underlying variable differentiating self-control and social cooperation.

Probability of Reciprocation

Because there seems to be a relationship between the structure of self-control and the structure of social cooperation, it is important to know whether experience in one sphere transfers to the other. Jay Brown of the psychology laboratory at Stony Brook is working on this problem; some of his completed research will now be described.

The object of the experiments was to compare self-control with social cooperation by humans playing a game. The game was played either by a single player

("alone") to study self-control, or by a pair of players ("together") to study social cooperation. The subjects were all female Stony Brook undergraduates.

The game board is diagramed in Figure 4a. It consisted of a rectangular plastic tray divided into 4 compartments ("boxes"). Each box contained 3 items:

A red or green index card with a picture of a door ("red doors" or "green doors");

A red or green key;

1, 2, 3, or 4 nickels as shown in Figure 4. (In subsequent experiments there were 1, 2, 5, or 6 nickels in the boxes.)

The upper boxes both contained red doors; the lower boxes both contained green doors. The left boxes both contained red keys; the right boxes both contained green keys. Note that each right box held 1 more nickel than the box to its left, and each upper box held 2 more nickels than the one below it. All the items in the boxes were visible to the players.

The self-control game ("alone"). Each trial began with the apparatus as pictured in Figure 4. To start, a player was given a red key. The player could use that key to "open" one or the other red door (to choose either the upper left or upper right box). The used key was then surrendered. If the upper left box was chosen, the player was permitted to take the 3 nickels and the red key from that box. If the upper right box was chosen the player was permitted to take the 4 nickels and the green key from that box. Then the nickel(s) and key taken were replaced by the experimenter and the next trial began. If a red key had been received on the previous trial, the player could again choose between the two red doors as before; if a green key had been received on the previous trial, the player could use the key to "open" one or the other green doors (to choose a key and nickels from either the lower left or lower right box). The sequence is shown in Figure 4b.

The alone game is a self-control procedure in the sense that the behavior leading to the higher current reward (choosing the right box with 2 or 4 nickels plus a green key) conflicted with the behavior that maximized overall reward (choosing the left box with 1 or 3 nickels plus a red key). Choosing the right box always earned the player one more nickel than choosing the left box did, but at the cost of obtaining a green key. With the green key the player paid for the 1-nickel gain (for choosing the right box) on the previous trial with an average 2-nickel loss (having to choose between the lower boxes) on the present trial.

The best overall strategy in the alone game is always to choose the left box, always receive a red key, and always earn 3 nickels. Always choosing the right box yields a fixed return of 2 nickels per trial. Alternating between the left and right boxes yields an average return of 2.5 nickels $[(3 + 2)/2 \text{ or } (4 + 1)/2]$ per trial. Only on the very last trial does it pay to choose the right box but the subjects did not know when the experiment would end.

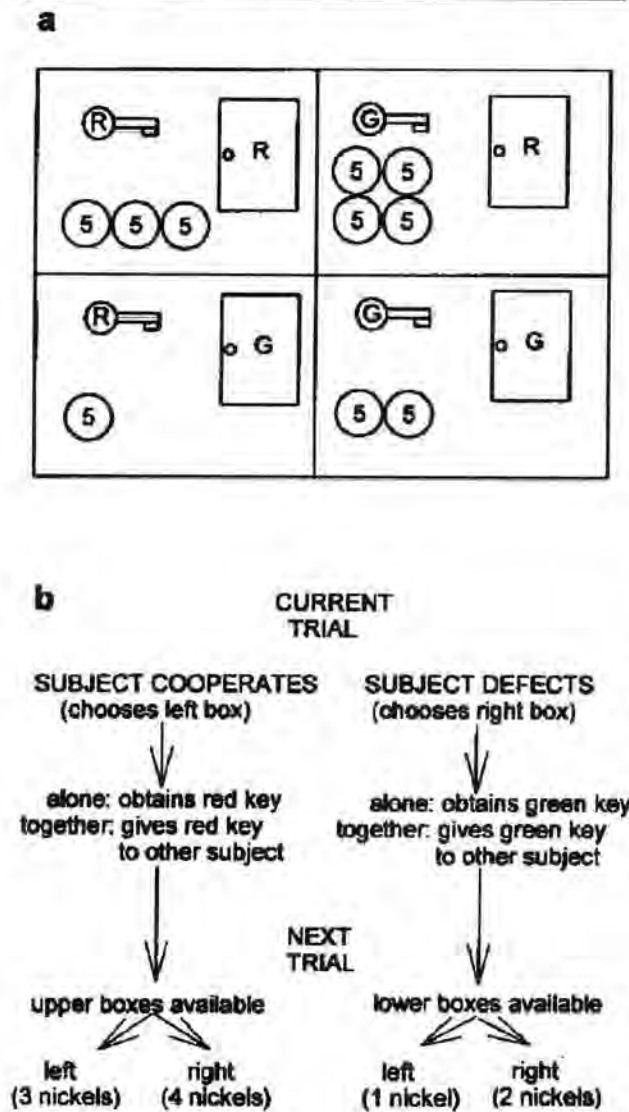


Figure 4

The alone version of the game duplicates the contingencies of a prisoner's dilemma game against an opponent playing tit-for-tat. Tit-for-tat says, cooperate on the first trial and from then on cooperate if your opponent cooperated on the previous trial and defect if your opponent defected on the previous trial. Consider what it would be like to play a repeated prisoner's dilemma game against an opponent who plays tit-for-tat. If you were to cooperate on the present trial you would be able to choose next time between the higher rewards (because the other player will have cooperated). If you were to defect on the present trial you would be forced to choose next time between the lower two rewards (because the other player will have defected). These are the very contingencies set up by the keys and doors of the alone condition.

Because current choice of the lower available reward always leads to a higher next-trial reward, current choice in the alone game depends on the degree to which the (higher) next-trial reward is discounted. Because it cannot be obtained until the next trial, the higher future reward may be discounted by delay. But another possible source of discounting is probabilistic discounting. A player may currently discount higher future reward by the probability that she herself will fail to choose the lower reward on subsequent trials. Suppose a player has repeatedly chosen the higher current-trial reward in the past, earning 2 nickels per trial. If she chooses the lower current-trial reward on this trial only, and the higher current-trial reward on all subsequent trials, she will earn 1 nickel on this trial, 4 nickels on the next trial (for an average of 2.5 nickels), then return to 2 nickels per trial. This might not be enough incentive to choose the 1 nickel on the current trial. But if she chooses the lower current reward on this trial and continues to do so, she will eventually earn 3 nickels per trial, a 50% increase. This may well be a sufficient incentive to choose the 1 nickel now. However, if by past experience a player believes it unlikely that she will choose the lower current-trial reward in the future, there is little incentive to do so in the present. Probabilistic discounting may apply as well in everyday-life self-control situations. If by past experience a dieter believes it highly improbable that high calorie foods will be resisted tomorrow and on subsequent days, there is no reason to resist them today.

The social cooperation task ("together"). The game, as played by two players together was the same as when played alone, except the two players, playing on a single game board, made choices on alternate trials. After using her key to open a box, each player took the nickels in the box for herself but then handed the key to the other player. Thus, after the first trial, whether a player was permitted to choose between the upper boxes (3 or 4 nickels) or between the lower boxes (1 or 2 nickels) depended on the other player's choice on the previous trial. The sequence is shown in Figure 4b. The players were not allowed to discuss the game. Their only means of communication was through the choices they made.

Playing this game together, income would be maxi-

mized (at 3 nickels per trial) for each player if both players repeatedly chose the left box (cooperated). However, the individual player would always gain more on the present trial by choosing the right box (defecting). The penalty for defecting, of having to choose between the lower boxes, is suffered not by the player who defects but by the other player, who inherits the green key.

There is an ambivalence in the together game as there is in the alone game. A player wants to choose the right box with the higher number of nickels (2 rather than 1 or 4 rather than 3). That is, she wants to defect. But she also wants to have a red key (to be able to choose between 4 and 3 nickels rather than between 2 and 1). She will only have a red key if her partner had chosen the left box (if her partner had cooperated) on the previous trial. Since her partner has the very same motives, one way for a player to get her partner to cooperate on the next trial might be to cooperate herself on the present trial. Thus, each player has a reason to defect and a reason to cooperate.

But cooperating is the very worst strategy in this game,

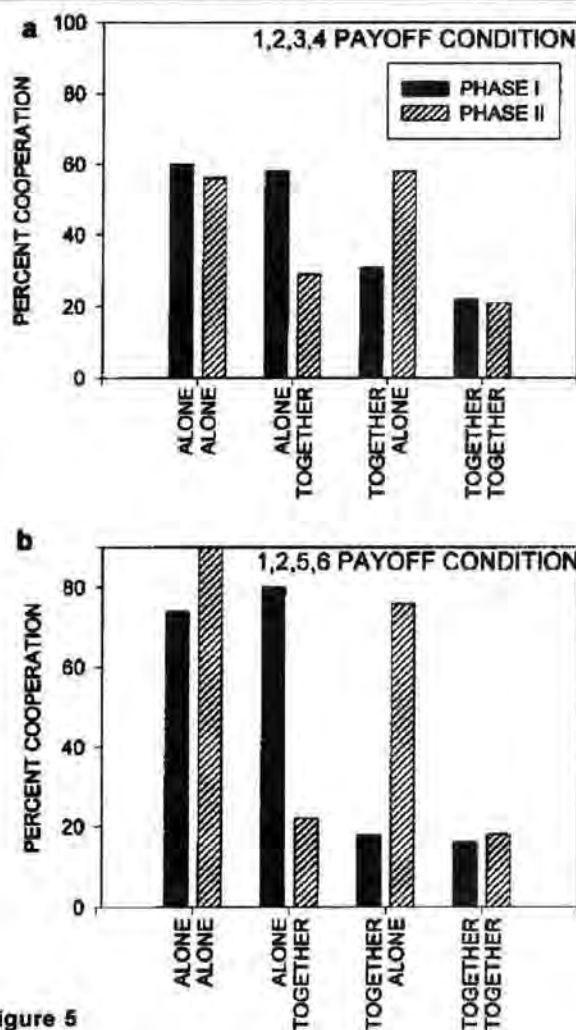


Figure 5

unless the other player also cooperates. Therefore the only reason to cooperate (within the demands of the game) is to influence the other player to cooperate subsequently. The reward for cooperating in the social cooperation version of the game must be discounted not only by the delay to the player's next turn but also by the probability that the other player will reciprocate.

People's estimation of the probability of other people's future cooperation, however, might be expected to be lower than their estimation of the probability of their own future cooperation. For this reason, a player who cooperates with her own future self in the alone game (who consistently chooses the lower current-trial reward) may defect from the interests of her partner in the together game. Figure 5 shows what we found.

Four groups of subjects were tested in a standard "transfer" design. One group played the game alone for 40 trials. Another group played the game together for 40 trials. The third group played alone for 20 trials (first phase) and then together for 20 trials (second phase). The fourth group played together for 20 trials (first phase) and then alone for 20 trials (second phase). Figure 5a shows the results averaged over 4-trial blocks. Subjects playing alone came to cooperate on about 60% of the trials while subjects playing together cooperated on about 20% of the trials. When subjects were switched from playing alone to playing together cooperation decreased. When subjects were switched from playing together to playing alone cooperation increased. Experience in the first phase with one condition seemed to have no effect on behavior on the second phase with the other condition.

Then, with new subjects, Brown redid the entire experiment as before except he increased the number of nickels in the two upper boxes (with the red doors) to 5 and 6 (rather than the 3 and 4 shown in Figure 4). This manipulation maintained the self-control (and prisoner's dilemma) contingencies but increased the larger delayed reward (the reward for cooperation). The reason for increasing the larger reward was to make sure that the alone condition presented a true conflict of motives rather than just a cognitive problem - a problem in practical arithmetic. If the alone condition were a true self-control problem, increasing the amount of the larger-later reward should increase self-control as it has done in many self-control experiments. If the alone condition is just a problem in practical arithmetic, increasing the amount of the larger-later reward should have no effect. In a practical arithmetic problem correct answers are not increased by increasing the amounts. (If Johnny goes to the store with \$10 to buy groceries, just as many children will get the right answer as if he goes to the store with \$20.) The results of Brown's second experiment, with the 5 and 6 nickels in the upper boxes, are shown in Figure 5b. Now subjects in the alone condition cooperated on about 80% of the trials, rather than the 60% cooperation found with the smaller next-trial rewards - evidence that the alone condition does indeed test self-control.

But what about the together condition? Increasing the amount of the next-trial rewards had no effect on coop-

eration of subjects playing together; they still cooperated on about 20% of the trials. Recall that the benefit of cooperation in the together condition is realized only if the other player reciprocates. A crucial variable in the together game is a player's subjective estimation of the probability that, if she cooperates, the other player will cooperate too. It is not surprising that increasing the amount of the next-trial reward (the reward to the other player) did not increase this subjective probability.

In summary, the results of these experiments imply that a crucial variable distinguishing self-control from social cooperation is the probability of reciprocation of cooperation and defection. In self-control situations, where reciprocation is under the control of a single person, this probability may be high. Most people perceive a common interest between themselves today and themselves tomorrow. But in prisoner's dilemma situations where the players have no common interest, this probability is inherently lower. An experiment by Forest Baker was designed to test the effects of reciprocation probability directly. We will describe it shortly. But first it is necessary to describe (briefly) a third experiment by Jay Brown that further tests the analogy between self-control and social cooperation.

The crucial variable in this experiment was patterning of trials. Patterning of trials ("soft commitment") has been shown to increase self-control of pigeons and humans under conditions of ambivalence. The effect of patterning is to broaden temporal scope - to cause decisions to be based on more abstract rather than more particular contingencies. If social cooperation is a consequence of correspondingly abstract choices, then patterning should also increase social cooperation. In this third experiment with the game board, four new groups of subjects all played the game together at all times. The game board illustrated in Figure 4 was used except, as in Brown's second experiment, there were 5 and 6 nickels rather than 3 and 4 nickels in the left and right upper boxes. Instead of alone versus together, the conditions of the game were patterned versus *unpatterned* trials. The *unpatterned* trials condition was the same as the together condition of Brown's second experiment. In the *patterned* trials condition both subjects playing together made 4 decisions at once. They indicated on a piece of paper out of the other subject's view whether, on each of the next 4 trials, they would choose the left box or the right box. Then, the four trials were played out one by one. The player might have a red or green key on any of the 4 trials, but her choice on each trial was predetermined before the 4-trial sequence began. The experimental design was again the standard transfer design with one group playing 40 patterned trials, one group playing 40 *unpatterned* trials, and the other two groups switched in the middle. The results are shown in Figure 6:

Although the increase in cooperation due to patterning was not nearly as strong as that due to playing alone, patterning did suffice to significantly increase cooperative behavior as it increased self-control in prior experiments (note the elevation of the two patterning

FOUR CHOICES AT A TIME VERSUS ONE CHOICE AT A TIME (all pairs playing together)

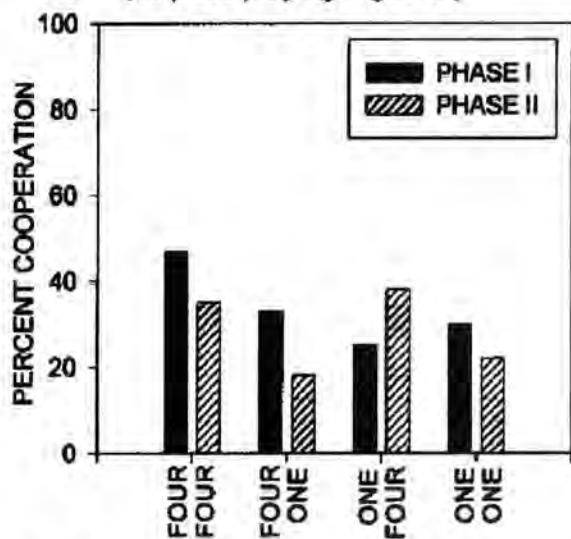


Figure 6

groups at the beginning of the first phase and at the end of the second phase). This is more evidence that self-control and social cooperation are corresponding processes.

The object of the experiment by Forest Baker, to be discussed next, was to directly test the theory that the crucial variable differentiating self-control from social cooperation is the probability of reciprocation. According to the theory, subjects playing alone in Brown's experiments cooperated more than subjects playing together because the alone player's estimation of the probability that if she cooperated on this trial *she herself* would cooperate on the next trial was higher than the together player's estimation that if she cooperated on this trial *her partner* would cooperate on the next trial. In Baker's experiment, probability of reciprocation was explicitly varied. The game was played together, not on a game board with another subject, but on a computer screen with the computer taking the place of the other subject. On the screen was a diagram with 4 boxes like the game board of Figure 6 but without keys and doors and with 5 and 6 nickels depicted in the upper boxes (as in Brown's second and third experiments). A bar on the screen grew in size proportional to the money earned. As in the alone condition of Brown's second experiment, subjects chose between the left box with 1 or 5 nickels and the right box with 2 or 6 nickels. Whether they could choose between the upper two boxes (5 versus 6 nickels) or the lower two boxes (1 versus 2 nickels) depended on the computer. Between the subject's choices the computer would decide to highlight either the two upper boxes or the two lower boxes. The subject could choose only between the highlighted boxes. The subject's decision determined the computer's decision with a probability, p . This

probability, the probability of reciprocation by the computer, was the crucial experimental variable. Figure 7 illustrates the contingencies.

On the screen, in addition to the 4 boxes and the bar

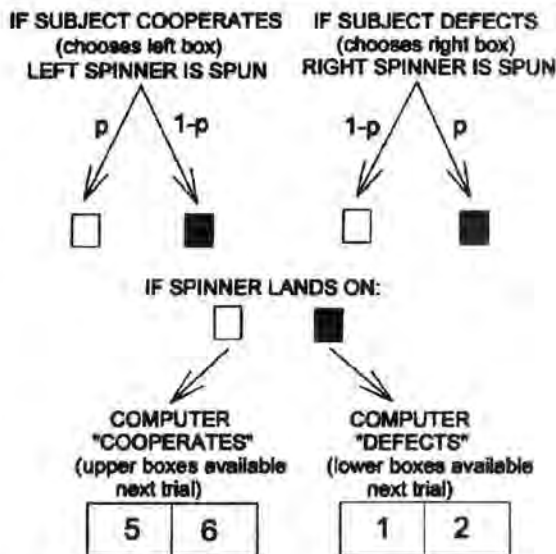


Figure 7

indicating winnings, were two spinners with white and black sectors. The proportion of the left spinner that was white was equal to p and the proportion of the right spinner that was white was equal to $1 - p$. That is, the right spinner was the inverse of the left. On a given trial, if the subject chose the left box with the lower number of nickels (if the subject cooperated) the left spinner was spun. If the spinner stopped with the white sector under the arrow, the upper boxes were highlighted on the next trial (that is, the computer cooperated); if it stopped with the black sector under the arrow, the lower boxes were highlighted on the next trial (that is, the computer defected). On the other hand, if the subject chose the right box with the higher number of nickels (if the subject defected), the right spinner was spun. Again, if the spinner landed on white, the upper boxes were highlighted on the next trial (the computer cooperated) while if the spinner landed on black, the lower boxes were highlighted on the next trial (the computer defected).

The probability, p , was the probability of reciprocation. If the subject cooperated the computer would cooperate with a probability equal to p and defect with a probability equal to $1 - p$; if the subject defected the computer defected with a probability equal to p and cooperated with a probability equal to $1 - p$. Thus, the probability of reciprocation, implicit in Brown's experiments, was explicit and signaled by a discriminative stimulus in Baker's experiment.

There were 5 groups of subjects, each with a given probability of reciprocation: $p = 1.0, .75, .50, .25, 0.0$. With $p = 1.0$, the computer was playing tit-for-tat. If the subject cooperated on a given trial, the computer

would surely cooperate; if the subject defected the computer would surely defect. The $p = 1.0$ condition was equivalent to the alone condition in Brown's experiments; subjects would maximize earnings by always cooperating. With $p = .50$, the computer essentially flipped a coin to decide whether to cooperate or defect. Subjects would maximize earnings by always defecting (choosing the higher current-trial number of nickels) since cooperation with a computer would earn them nothing — not even gratitude. With $p = 0.0$, the computer cooperates only if the subject defects. That is, the computer is playing *tat-for-tit*. The subject should surely defect in this case since defection would both maximize reward on the current trial and get the computer to cooperate thus maximizing reward on the next trial as well.

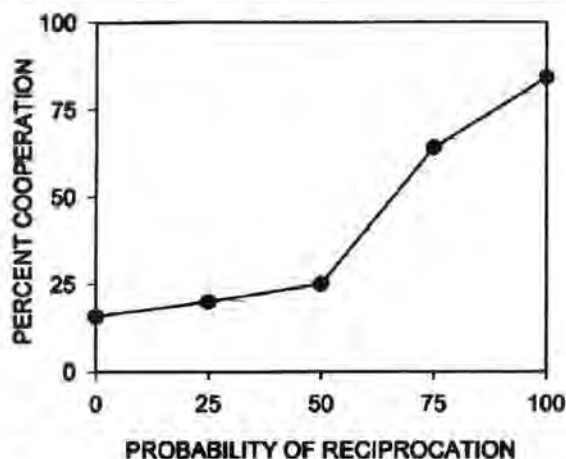


Figure 8

Figure 8 shows Baker's results. The average degree of cooperation of each group at the end of 100 trials was strongly affected by the probability of reciprocation. This probability, explicitly varied in Baker's experiment, must have implicitly varied in Brown's experiments and been largely instrumental in determining the difference between the alone and together conditions. Even though the actual reciprocation probabilities for the alone subjects in Brown's experiments were 1.0, the lack of a clear discriminative stimulus and the complexity of the key and door contingencies may have reduced the subjective reciprocation probability for these subjects below 1.0.

What counts for cooperation is not the absolute probability of future cooperation or even the subject's subjective estimation of that probability. It is in the nature of the prisoner's dilemma that it pays to defect regardless of the *absolute* probability that others will cooperate (or, in self-control situations, the absolute probability of an individual cooperating with his or her own future interests). The above experiments show that what counts is the *relative*, or *conditional*, probability, the probability of reciprocation over a series of opportunities to cooperate. The important question is not, "Will others cooperate (or will I cooperate) in the future?" but, "If I cooperate now, will others cooperate

(or will I cooperate) in the future?" When self-control breaks down, as in cases of addiction, the probability of reciprocation plummets. The alcoholic who has quit a thousand times, must also have started up again (defected) at least 999 times. For an alcoholic there may be no reason to cooperate today with the person that the alcoholic himself will be tomorrow. The probability that that future person, that incarnation of today's decision maker, will reciprocate today's drink refusal may well be low. It is not possible to change that probability at a single moment any more than it is possible, in Aristotle's analogy, for a single retreating soldier to turn and stop an enemy advance. But, it is particular acts that have to be done. This is why the establishment of self-control, as well as social cooperation, when none was there before, requires a sort of faith. This faith is embodied in an act of imagination — acting as if something were true that is not in fact true — acting as if the probability of reciprocation is high when it has been low in the past. Enough such acts will create the very thing imagined. Such acts are part of a still wider pattern of behavior, the pattern we refer to when we talk about the concept of "self." We now turn to the question, why would a person ever perform an act of self-control or social cooperation when the probability of reciprocation is low.

Self and Self-Control

Reciprocation probability is crucial in differentiating between self-control and social cooperation, but it is not the only determinant of cooperative behavior. What about cases where the reciprocation probability is apparently zero, yet still people cooperate with each other? In other words, what about altruism in social situations? The social cooperation game of Figure 1, the one that introduced this talk, was played only once. As was shown, it did not matter what other subjects chose, defection always paid more than cooperation. There was no chance for reciprocation, yet half of the people in the audience usually say they would cooperate if the situation were real. Those who defected seemed to profit. What keeps us from defecting in similar situations in life? The usual answer is: altruism. But, in the analogy between social cooperation and self-control, altruism corresponds to internal willpower, and internal willpower is a concept that itself needs explanation. The same goes for altruism. Why are we ever altruistic? To approach this question, let us return to the analogy between self-control and social cooperation.

The problem of self-control may be seen as a conflict between particular acts such as eating a caloric dessert, taking an alcoholic drink, or getting high on drugs, and abstract patterns of acts strung out in time such as living a healthy life, functioning in a family, or getting along with friend.

A corresponding problem arises in everyday-life situations requiring social cooperation (ranging from littering to international arms control). Conflicts often exist between acts benefitting an individual (or a relatively small group) and acts benefitting a (larger) group. But despite the closeness of the analogy between

self-control and social cooperation, it is far from clear that choice of more abstract alternatives in self-control situations (where delays are long) will correlate with or generalize to choice of more abstract alternatives in social choice situations (where probability of reciprocation is low). Social cooperation demands a more complex perception of individual self-interest than does self-control. In social cooperation situations the benefits (to the individual) of cooperating are often very highly abstract (having a better self-image, an easy conscience, a sense of moral rightness) to the point of complete insubstantiality (going to heaven). The discrimination of such benefits from their lack is what we mean when we say that a person has a wide concept of self.

Biological evolution has arranged matters so that for most species, most of the time, and for humans, some of the time, behavior adaptive for the moment is also adaptive beyond the moment. The squirrel saves nuts not because its self-concept extends beyond the autumn and into the winter but rather because it wakes up one morning and suddenly finds burying nuts to be valuable in itself. As previously said, the temporal breadth of a nonhuman animal's interest can, most of the time, remain narrow while Mother Nature takes care of the long view. A squirrel does not have and does not need a broad self concept. In human life, however, a conflict frequently arises between the long and short runs. An alcoholic may strongly prefer having a scotch to having a soft drink but also prefer strict sobriety to alcoholism. The preferred long term pattern (strict sobriety) is inconsistent with the preferred short term act (having a scotch). It is only when such inconsistencies arise that conformity to the preferred long-term pattern is labeled "self-control." If such inconsistencies rarely or never arose (as in the life of the squirrel) there would be no need for self-control, hence no need for a self concept extending beyond the moment.

Self-control and social control. It is customary to distinguish self-interested behavior, consistent with the goals of an individual, from altruistic behavior, consistent with the goals of someone else. Where these interests are in harmony (as in normal economic interchange) apparent altruism is normally explained in terms of self-interest. It is not considered altruistic to pay the grocer if he gives you something you want in return. But behavior where nothing is apparently received in return (such as volunteering at a hospital, anonymously donating to charity, rushing into a burning building to save a child) is considered altruistic because there is presumably no personal benefit, no benefit to the self, but only social benefit involved. The personal satisfaction that we may derive from such acts is considered to be dependent not on fundamental self-interest but on socially imposed conscience (or superego), or an innate altruistic motive built into us and, by its very nature, distinct from our selfish motives.

Failures to cooperate are common in real life prisoner's dilemmas. Fishermen in New Bedford fishermen each profited maximally by buying and sending

out as many boats as possible but ultimately, when all the fishermen began to do this, the common resource (the available fish) became depleted and the group suffered as a whole (the industry was devastated). The fishermen did have a common interest (in restraining their fishing) but that interest was not clear to them (the few surviving fishermen still deny it). It is thus of crucial social importance to understand how social situations may be manipulated to cause people to perceive a common interest with others.

Altruism Reconsidered

We have seen in Brown's third experiment that patterning increases self-control. Why should this be?

On an individual level, people who commit themselves to a sanitarium (where eating or drinking will be rigidly controlled) are attempting to break a pattern of repeated defection in their normal environment by choosing in advance a weekly or monthly (self-cooperative) pattern. They expect that once they have begun to cooperate they will keep cooperating outside of the institution. On a social level, such commitment processes are not generally available. It is not feasible for instance to move to an authoritarian society (like Singapore) where social cooperation is rigidly enforced, as a sort of sanitarium, and then back to a permissive society (like ours). The best alternative of course would be to consistently cooperate within the permissive society. As I have argued above, this alternative is not just best for society but also best for the individual for purely selfish reasons — *provided* we perceive our selfish selves broadly rather than narrowly in time.

Among cognitive decision theorists it is thought to be a puzzle why people habitually leave tips at restaurants (say on a highway) where they will never eat again. Leaving a tip at a restaurant where you know you will never eat again is clearly irrational when doing so is considered as an individual, isolated act. In fact, leaving a tip even at a restaurant where you do intend to eat again is irrational from the point of view of your self considered wholly at the present moment; you benefit now; it is only that *other* guy, the one who will inhabit your body at a later time, who may suffer. Richard Price's book, *Clockers*, vividly illustrates the devastating effects of a ghetto environment where short-term social interactions dominate long-term ones. There, powerful short-term rewards (drugs, money, daily survival) overwhelm vague and abstract long-term rewards (health, family, friends). Consequently, everyone's self-concept is narrow in time, controlled by the clock rather than by the calendar.

Leaving tips is a pattern that (usually) serves us well in our society, a pattern in aid of individual self-control. It may seem as though there is no connection between the benefit our tip confers on the next person who wanders into the restaurant where we will never return and the benefit conferred on us by the previous person's tip. But there is a connection — in the pattern we maintain in our behavior. It would be bad for us *personally, selfishly*, if we decided individually each

time we went into a restaurant whether or not we should leave a tip. Decisions on a case-by-case basis are exactly the sort of (pseudo-rational) behavior (the lawyer's prisoner's dilemma advice) that would get us defecting all of the time. It is not possible to tease apart the individual and social benefits of such acts. Most altruistic acts are, like tipping, personally profitable a lot of the time. Giving to charity is often observed and frequently rewarded by society. But the relation between generosity and its rewards is vague and indistinct. Generosity for most of us (like sobriety for the alcoholic) is not profitable and would not be chosen considering only its case-by-case (its narrow) reinforcement. Consequently the way for most of us to profit from generosity (and the way for an alcoholic to profit from sobriety) is to pattern our behavior abstractly — to choose to be a generous (or a sober) person. It is in aid of making such choices that narrative — biographical and autobiographical — derives its function. The internal *mechanism* by which a personal narrative may gain control of a person's behavior is a subject for neuroscience. Here we are concerned with the *function*, the purpose of that mechanism in human life. Saying, "I am a generous (or a sober) person," provides a discriminative stimulus that functions to group generous (or sober) behavior into positively (if vaguely and indistinctly) reinforced patterns — to classify behavior into acts consistent with one's self — acts reinforced in the long run and as a group.

But in order to pattern our behavior in this way (and reap the rewards for so doing) we must forego making decisions on a case-by-case basis. Once we abandon case-by-case decisions, there will come times in choosing between selfishness and generosity when we will be generous even when generosity is explicitly *unreinforced* or even punished. In other words, we will behave altruistically. But such altruism is not only compatible with a wider selfishness, it is a *necessary* component of a wider selfishness.

In an environment (like at least some of ours) where tit-for-tat behavior (do unto others as they do unto you) is more or less the norm, altruism is clearly selfish. Those of us who live in such environments can come to perceive the social good as our good in the same way as we can come to perceive our long range good as our immediate good — by developing good habits. But good habits require a certain kind of faith in the future. If you were born yesterday and were going to die tomorrow (a case approached by some of the characters in *Clockers*) there would be no advantage in behaving well — such behavior would have no function. But our larger society demands a wider self. The mother who, without thinking twice, runs into the burning building to save someone else's child is behaving altruistically but she is also behaving selfishly. By her behavior she has teased out of her booming, buzzing environment an abstract reflection of herself. ΨΨΨ

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A Generalist Looks at His Career in Teaching: An Interview with Dr. Phil Zimbardo¹

INTERVIEWER: Could you give an overview of how you got interested in psychology?

ZIMBARDO: I think I was nurtured into psychology by my early life experiences. I grew up in the South Bronx ghetto. My parents didn't go past the eighth grade and nobody in my extended family ever finished high school, if that far, so education was not a value, or rather it was a luxury we could not afford. Life was about surviving, not reflecting.

For poor kids living in the ghetto, several things happen. First, if you're poor, the only important factor you have is people. You don't have material things. You don't have commercial games. You don't have paid entertainment, and back then you didn't have TV or electronic video games, or home entertainment centers. The main thing you had was people and dealing with other people in the streets—that was our entertainment center.

In those days, the world of children excluded adults. You didn't have parents supervising kid's play. Essentially parents told you to get out of the apartment, because they were small and cramped and they wanted quiet time without a bunch of noisy kids around. So you lived and played in the street in front of your tenement. What that meant was you had to learn very quickly to be a personality diagnostician. Who are the people you can trust? Who are the big guys you should ingratiate yourself with? When should you be compliant and conforming? When should you try to be unique, independent and stand out from the gang? What people call "street smarts" is really a blend of this kind of intuitive personality analysis mixed with primitive situational analyses. I was a skinny, sickly kid growing up with really tough kids on East 151st Street. To survive, it was incumbent upon me to learn those skills very quickly and make them operationally effective. And ultimately I did learn those lessons so well that I went from being a lowly follower, tag-a-long, to the ring leader, head of the gang, president of the class, captain of the team. That was one set of experiences that got me focused on people, on observing people, trying to understand people, trying to manage impressions and form them of others, and ultimately working to persuade, influence, and manipulate others.

A related phenomenon pushed me toward becoming a social psychologist. Poor people are all situationalists. Rich people are dispositionalists. When you're poor, you look around and all you see is failure. The question is, to what are you going to attribute the failure? The failure of your father to have a good job or any job? The

failure of your mother to be dressed well or be well educated? The natural thing is to say, maybe it all depends on the situation—I bet things would have been very different if we had more resources, more privileges, more breaks, better luck. Whereas if you're rich, you look around and see success, and want to take credit for the success—it's in people, it's in their genes, their family line, their IQ, their personality traits.

I think these notions fuel these two very different approaches to understanding the causes of behavior we observe. Social psychologists say the social context controls more of what we think, feel, and do than most people realize. The dispositionalists say no, we really have to understand personality traits, like the morality, character, intelligence, the organismic variables that are person-centered. In psychology, the dispositional has really been the mainstream approach in our individualistically focused culture. It has been the unquestioned assumption as the causal core of human nature in all of our institutions, like law, religion, medicine, business. Those two forces coming out of my childhood, pushed me in the direction of psychology. The one restraining force against becoming a child psychologist is that I took introductory psychology my freshman year at Brooklyn College and hated it! I got the only C grade in my life. It was a bad book, a bad teacher, no coherence to the lectures or the text (Munn), and nothing of relevance to real people that I had been observing and being. A class of 300 taking multiple-choice tests where you could not explain what you know, turned me off my early love. I didn't take another psych. course until my senior year. It was an experimental psychology course, and I realized how much I liked to do research and answer causal questions with precision of experimental methods and analysis. I had been majoring in sociology/anthropology, where they asked the big, interesting questions but never had a satisfying answer, and they were dealing with institutions, when I wanted the people level of analysis.

INTERVIEWER: How do you look at yourself now? You're in a position to look back, essentially to have been a participant in much of the history of modern psychology. How has psychology changed in the last 45 years?

ZIMBARDO: Although I'm a social psychologist at heart, around that heart I think is a well-crafted gener-

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alist. During my training at Yale, I worked very hard to become a psychologist, to take courses across the board in learning, in personality, in sensation, in abnormal psychology, as well as in social psychology, and did research with a range of faculty from different disciplines - Neal Miller, Frank Beach, Seymour Sarason, Carl Hovland, Bob Cohen, Jack Brehm, Irving Saroff, among others. In a sense, being labeled a generalist can be almost disparaging, meaning that you don't know enough or aren't good enough to be an expert in a particular area that identifies you.

But for me, it's a compliment. My focus has always been on the broadest possible perspectives of psychology. As an undergraduate, majoring in sociology and anthropology gave me that broader context in which to put my interests in social psychology. And my Yale training as an experimental behaviorist enabled me to feel comfortable using paradigms from classical or operant conditioning, from classic motivational research, or to use consummatory behaviors and psychophysiological reactions in my social psychology investigations of dissonance theory or studies of unexplained arousal, for example.

From the beginning, I also loved to teach as much as I love research. I want to show how, in my career, teaching and research have always been two sides of the same coin, two ventricles of the same heart. I've always loved to teach introductory psychology from my first course as a graduate student in 1957 to my 40^x year of teaching it last term. You can't teach introductory psychology well without being a generalist, interested, or rather fascinated by most areas of psychology so that you can bring out the excitement of them to your students. Nor can you write a good psychology text without loving every different domain of psychology beyond your narrow field of specialization.

What is remarkable to me in looking back on my career is the many influential leaders of psychology that I knew personally, studied with, worked with, talked with informally. It was indeed a golden age because most of the second generation of psychologists was still alive and kicking. In addition to those I mentioned earlier, I have known the following psychological colleagues: Hal Proshansky, Irving Janis, Mark May, Don Campbell, Muzifer Sherif, Leon Festinger, Stanley Schachter, Hank Reichen, Stanley Milgram, Solomon Asch, Hal Kelley, John Darley, Bibb Latane, Morton Deutsch, Hal Gerard, Bill McGuire, Elliot Aronson, to name a few and not counting my great colleagues at Stanford. The field was smaller then, younger then; It was a time of Main Effects undiluted by higher order interactions that we can never remember, of behavioral experiments with real people observed in interesting contrived situations, not respondents telling us how they imagine they might behave in hypothetical situations on questionnaires.

It was the simple era then before the Information Dam broke loose and we became inundated with data, with endless new journals churning out more articles and edited volumes churning out more chapters than any-

one could possibly read in a lifetime. So we knew less than students know now, but I would like to believe we knew less in a better way.

On the other hand, so many new fields have been opened to us and new ways of thinking and knowing, that there is more excitement now about the broad range of psychological inquiry and at the same time the greater depth of the analyses we can make using new technologies. For example, when I was a student, one of the most boring areas of research was that of memory, modeled by Ben Underwood's relentless pursuit of every conceivable independent variable related to nonsense syllable memory paradigms. Now there are few areas more interesting than the study of memory, with its many different memory systems, memory processes, measurement techniques, creative experimental tasks, range of populations studied, and levels of analysis.

INTERVIEWER: You've been teaching introductory psychology for 40 years and writing an intro textbook for 30. How do you keep up to write a new chapter on learning, or moral development, or whatever?

ZIMBARDO: I can't imagine a more challenging, difficult job than to teach an introductory psychology class or write an introductory psychology text at this time. Psychology now spans the universe! It's hard to think of what is not psychology. It goes from cell to culture. In my text, *Psychology and Life* (in its 15th edition, now with Richard Gerrig as co-author) there are sections on what is happening inside an ion channel inside of a single neuron. On the other end, in a new social psychology applications chapter we're talking about genocide, ethnic cleansing and peace psychology—and everything in between. There's no other field in all of academia that has that breadth and depth at the same time.

But that makes it very hard to keep up with the information explosion in our field. I think that is one reason there are fewer and fewer generalists and more people specializing. Many of these specialists soon become microspecialists, some not even interested in being a social psychologist, but interested only in a small part of the field and intentionally ignorant of the rest of the field, or earlier work. Then there are new fields that are emerging that were not around when I was a kid. Health psychology, cultural psychology, behavioral gerontology, biology of memory, are new to this time. Cognitive neuroscience, with its functional neuro imaging, is a totally new field that did not exist two editions ago. It is great that cognitive psychologists gave us back our minds that my Behaviorist brethren had set aside, and further that these Cognitive Neuroscientists are giving mind a place in the brain. And that Social psychologists are revealing that what matters in understanding behavior is context as well as content, situations as well as inner determinants, and cultural diversity as well as pure bred subjects. The way a textbook author has to handle this explosion of knowledge is to read all the time, as much as you can, as broadly as you can. Nothing of human nature can be

alien to a psychology textbook author. The moment the text is published, you're already starting a file for the next edition, where you're clipping, cutting and pasting the articles you read and things from the media as possible items for this or that chapter.

Also, before starting a new edition, I talk with a lot of colleagues whose expertise and opinions I respect. We talk about what's new and valuable, what findings are no longer important in a field, and what are the new things that they are excited about. In a sense they give me more than just new information, they give me the sense of passion they have for their discipline, and then as an author I try to imitate it to my readers. The big challenge for a text author is between presenting the classics, the oldies but goodies, updating them with new research that qualifies their implications, and also working in contemporary research in entirely new areas. What to omit is as hard a decision as is what to include, maybe tougher, and especially as the information mass grows, so does the size of our texts. They would become unwieldy without judicious editing, that often feels like slashing and cutting when done by your editor's vicious "delete" key.

It is enormously time and energy consuming, an overwhelming task, to generate a 700-page book with 18 chapters, with thousands of references (our current edition has 36 pages of references in small font). Not to mention additional work on brief editions, instructor's manuals, student study guides, and other ancillaries. When I look back, I can't imagine now how I ever did it edition after edition, and still found time to have a life as a researcher and a person. However, as you get older, you have to selectively optimize your time and energy, according to my colleague, Laura Carstensen, an expert on aging. That's the time the old generation calls on the new generation to begin to share the load. In my case, Richard Gerrig, a cognitive psychologist and master teacher of introductory psychology, comes to my assistance. First he adds a lot of sophistication in many areas in which I am no longer particularly strong, with the goal of ultimately replacing me, just as I replaced Floyd Ruch back in 1968. Then, if things go right and our book continues to be adopted, in 20 years or so, old Dr. Gerrig will be replaced by one of today's students of psychology, as long as she is a sound generalist.

INTERVIEWER: What advice would you give a generalist-somebody who felt it was worthwhile to teach introductory psychology and not be a narrow specialist?

ZIMBARDO: Let me reframe that question. Would I advise a young person who wants to teach at a top school to be a generalist rather than a specialist? The answer is no. Departments don't hire generalists. Every psychology department wants the outstanding person in a very narrow area, the specialist super star. But that's how you get your foot in their door. I think what you have to do is then be willing to wear those two hats. Your career is going to be advanced by being the world's leading expert on the psychology of widgets.

Yet, what will turn you on most day after day is the high you will get from being able to teach introductory psychology in all its richness and glory. That is where you can make a big contribution by being able to have access to those young, inquiring minds who are not necessarily going to become psychologists, but are going to rule the world some day. You have an opportunity to shape the way they think about psychology, to focus and broaden their vision. By being a generalist in that arena you can more fully enjoy the diversity of modern psychology and transmit that enthusiasm and passion to your students. There's nothing like it. Research is exciting when you get a new idea, get significant predicted effects, and have your published article in hand, a process that now takes years with little fun in between those few highs. Good teaching gives you those highs steadily on a fixed interval schedule. Given that there are so few people who are able, willing, and competent to teach introductory psychology, if you can do that well, that will help you to get good jobs and be a feather in your tenure cap, assuming that you continue to work your butt off on the side churning out research that your colleagues respect.

INTERVIEWER: What about people at junior colleges or teaching-oriented four-year colleges where they don't do research, but like to keep active intellectually?

ZIMBARDO: If you're not doing research, that should mean you have more time to devote to your students, to advising and counseling, as well as spending more time preparing classes. However, in many instances these teachers are forced to carry a heavy workload without the help of teaching assistants, secretaries or other resources that are essential for teaching professionals. But many of them are involved in clinical practices or community work that helps to give them real world experiences, which hopefully they can incorporate into their teaching. Where resources are slim, creativity must take over to save the day and the class. However, the key in all of this is whether you really love to teach or are doing it to pay the bills, just a job. If you love it you will find a way to make it work for you and your students; if you don't all the resources in the world won't make a difference. Teaching is a perfectible skill, not an inborn trait, and it takes learning, practice and feedback from students and colleagues to perfect. And it takes years of doing it to learn how to do it really well. As I said before, when teaching is going well, it gives you day-to-day reinforcement that you never get in the same way from research. One virtue of not doing much research is not having to face the humiliation of dealing with journal editors, who do have an inborn talent for making anyone feel incompetent. When I give teaching workshops to community college or junior college teachers they report that it is not possible to do as good a job as they want to because of their heavy teaching load. I am sympathetic to that position because when I started my first full time job at NYU in the Bronx in 1960, I taught 13 semester courses each year, 5 each term, 2 each summer (and the 13th was off campus), to make extra money.

Most of them were large lectures, including introduc-

tory psychology three times a year. Of course it was tough to carry such a load, especially since I knew that I wanted to do research because I loved to do it and had to do it to get promoted, or to escape to a better school. So you make a lot of sacrifices, give up vacations, entertainment and entertaining, and live your life around your students, teaching and research. That is not a life that many academics want to live, but for me at that time it was better than working on an assembly line or doing the menial jobs that members of my family did.

Of course now I teach many fewer courses, but put more time into the preparation and execution of any one of them with the new goal of making each one memorable for my students and me, like a Michelin 3-star meal. I just finished teaching the best course I have ever taught in my life after 40 years at the podium. My Introductory Psychology course last term at Stanford was a magical class in every way. I had tears in my eyes when it ended because I didn't want it to end, I wanted to keep it going on forever. It was just a wonderful experience; it's doing something totally different within the same context, same teaching structure, like Haiku. I prepared new lectures and demonstrations, had exciting guest lecturers, used new ways of testing, encouraged students to do actual experiments for their term papers, and more. I started a Web site which included detailed class notes, so students could pay better attention to the lecture knowing they could download the details later, and a bulletin board that enabled them to post generic questions that I answered each day or had expert colleagues reply to.

The last point is that if you're in a place where teaching is relatively more important than research, then being a generalist really means understanding the relationship between psychology and philosophy, and psychology and literature, and psychology and art. And that makes one a generalist of an even broader scope than I mentioned earlier, a generalist across domains of psychological knowledge. To be an intellectual generalist, you have to read novels, poetry, go to plays, keep up on current events—so that you can work them all into your lectures and class exercises. For example, my research on deindividuation, the effects of anonymity on increasing antisocial behavior, came from reading Golding's *Lord of the Flies* for a course on group dynamics, and experimentally testing the novelist's assumption that changing one's appearance can disinhibit aggressive behavior.

In Oxford University, at High Table, you are not permitted to talk about your subject area. The assumption is that you should be sufficiently well-educated and learned that you can carry on conversations outside your narrow domain of specialization. For most people I know that would be an impossible constraint on their speech.

INTERVIEWER: Since our journal is taking a generalist approach, which you've called "unpopular," maybe you could make a few other comments on the wedding of teaching and research for you.

ZIMBARDO: I believe that the introductory psychology course is the most important course we teach in any psychology department for two reasons. First, it attracts those students who will major in psychology and go on to become professionals, and ideally your course started them on a path where one day they will replace you, be better than you, and move the field ahead in new directions. Secondly, many students take this course who do not major in psychology or take any other of our courses, but who will go on to influential positions in the world. They need to appreciate the value of the scientific method, the value of informed, open-minded skepticism, how to answer questions with data, not with authority or dogma, and how to apply the many valuable lessons of psychology to improve the human condition. Psychology teaches the need for tolerance and acceptance of diversity, of individual differences, of analysis without prejudgments. I want the psychology majors to put more life in out psychology and these other students to put better psychology in our lives.

I see that as a challenge and a burden. I know there are some universities where beginning graduate students teach introductory psychology. That's a sin. It should not be allowed. It's almost immoral. You simply don't know enough at that level. I'm not sure I know enough after 40 years of teaching, after writing many editions of introductory texts. To think that a new graduate student can teach all of introductory psychology is just wrong, given the amount of knowledge required these days about so many areas. It is just an abdication of responsibility of faculty because they're essentially saying we're going to dump it on graduate students because we don't want the burden, or can't teach it well ourselves without doing too much preparation. And then, of course, they justify this action by arguing that the introductory course is not so important anyway. It is a tough course to teach, no doubt about it. We got to a point at Stanford where nobody wanted to teach introductory psychology besides me and a very small band of other faculty because of the work it took to be knowledgeable about so many areas beyond one's own specialty. So in order to attract other faculty to teach the course, we changed the structure and required each course instructor to utilize expert lecturers from within or outside the department in those areas the instructor did not feel especially competent. What this does is (a) reduce the instructor's preparation of those lectures while guaranteeing that students are going to get expertise across five or six domains in psychology, and (b) it introduces the students to the professors who will be teaching the second-level courses in developmental, in cognitive, social, personality, abnormal, and so forth. Of course, the primary professor has to be there all the time to do the integration and synthesizing across the different domains. It's worked really well at Stanford and I encourage other departments to try this procedure.

In my teaching, I always also bring in outside guests because my goal in teaching is to break down the boundaries between academia and the real world. I try to bring people from the community into the classroom

who have something important to say of psychological relevance. Some are non-traditional college lecturers, such as prostitutes, football coaches, debaters who are pro and anti abortion, former mental patients, ex convicts, concentration camp survivors, cult recruiters and ex cult members, and many others. The goal is to break down the boundaries between purely intellectual academic experts in the classroom and those with street smarts in the world outside.

Now that I think of it, I try to do a similar thing in my research, either in generating ideas for the research or after I do the research, I always search for what are the parallels in the real world. That is, I want my research to be significant not only for other social psychologists, but for the general public, for the ordinary person to read it or hear it and say, "Wow, that's interesting, that makes me understand something in my life."

For me, the notion of giving psychology away to the public has always been a central part of everything I do, whether it's my teaching or research. I was pleased that George Miller, when he was president of APA, gave that as his invocation as something that psychologists should do, and we still don't do enough of it. He actually cited the Milgram experiment and the Stanford prison experiment as instances of research that had public appeal. Incidentally, Stanley Milgram and I were homeroom classmates in our senior year at James Monroe High School, the Bronx, in 1954.

INTERVIEWER: In your own life as a psychologist, you've done well with a textbook, you have done well with your career and visibility. Have you thought about business applications of psychology?

ZIMBARDO: The world of work is, for most people, one of the most important things in their life. Psychologists have to make an important contribution to understanding psychological aspects of work. After saying that I have to confess that my research and my focus has not been in that area. I don't know why, but I was never interested in economics, or making money, perhaps because it had been such a preoccupation with my parents at a survival level. I never had an interest in business, indeed education seemed to be the escape route from real work and the world of business. I did work as a kid, was a shoe shine boy in the Bronx, delivered laundry, worked for years as a concession boy in the St. James Theater on Broadway checking hats and coats, selling programs, candy and drinks. But your question reminds me that for poor folks work was always something to be avoided. Work was always ugly and trivial and demeaning. There was no role model who came home and said, "God, I had a wonderful day at the office!" Instead, the mantra I heard as a kid was "another day over, how many days to go to payday?" My father's side of the family were barbers, my mother's side were shoemakers, and hated doing what they were doing, like service people, or waiting on other people. I hadn't thought about it until now. Yes I probably work as hard or harder and longer than most people I know, but it is a work of love, I do it for me, not for a boss and not for the money.

When I wanted to go to college and later to graduate school, my father tried to dissuade me, and finally concluded that: "You don't want to go to work, so stay in school." I was an incorrigible. He thought of going to school as avoiding work. And it was, to avoid the kind of demeaning work that I had always perceived as an awful necessity of life. Interestingly, my wife, Christina Maslach, a UC Berkeley professor, is an expert on job stress and the pioneering researcher on the psychology of burnout. So by having a family colleague who is contributing to making the workplace a better place, I am absolved of not doing my share in that domain.

INTERVIEWER: But you seem to have enjoyed what you've done...

ZIMBARDO: Yes, very much so. The difference between my father's work and mine is that I have the privilege of working on things that I love. The problem comes from loving too much, too many different things. I love teaching most, but I also love many phases of exciting research, and I love writing articles, chapters, text and trade books, I love applying psychology to real world situations, I love being able to counsel and advise people on issues where I am considered an expert, and I love the little bit of therapy that I have done with shy people. I even like some professional activities, such as being President and Board member of the Western Psychological Association. It is also this range of activities that keeps any one of them from becoming boring by it having to sustain me all the time. So it is this total package that has kept me going all these years and hopefully for many more even better ones ahead.

I think that overall I work harder now than I ever have before. I don't teach more courses than I did when I was a kid teacher at NYU, but I put more time into preparation of each course and of each lecture and demonstration than I did previously. In part it is because I have built a reputation that I feel obliged to measure up to. I mean I'm coming near retirement age, yet I still teach more than most professors of my rank and tenure, four or five courses a year, always a large introductory psychology course of about 300 students, freshman and sophomore seminars (on topics like shyness and time perspective), graduate courses, this past term on collective violence, and teaching practicums, as well as individual studies, honors and thesis student advising. I still read term papers and always give extensive feedback, and am available to students through office hours that I enjoy several times a week, and of course on electronic mail.

What I struggle most with is achieving a balance between work and play, between all my professional activities which are typically my Future-oriented self and my non-professional activities, that I like to group under Present-oriented Hedonism. It is always difficult to find time to be a good psychologist and also a good husband, father, friend, active citizen, and then good to one's self by indulging in "pleasures of the flesh," such as getting massages, going on vacations, or making love often enough. The problem is the business side

of life always has deadlines to meet and somebody monitoring them. The personal doesn't fit, so it gets put into the "when I get time to do" bin, and then of course may not get activated. A new problem for me is electronic mail that consumes time like a starving piranha, since I spend two hours a day answering and sending mail, never less than one hour, and my record last week was five consecutive hours.

Where does that new time eater come from, since it has to take it from other activities that I used to do before this wonderful, terrible technology took over part of my life? From reading the morning newspaper, or magazines, or watching TV, or taking a walk, or talking to friends, and so forth. The balance of life gets skewed more toward the professional and less toward the personal, so it is a new time drain that I am trying to cope with. All things considered, I would say that I work at being a psychologist about 60 hours a week, including many weekends. But now that old age is starting to take its toll with various ailments starting to crop up, I have to spend more time taking care of bodily and spiritual needs than I have in the past, rather than living the totally mental-professional life.

INTERVIEWER: To follow this thought, what are your goals to be working so hard at this point in an extremely successful career?

ZIMBARDO: This may sound strange, but I am someone who never has specific, explicit goals that guide my behavior. I mostly slip into situations that have work agendas that I follow and generally succeed with, and then move on to the next item on my "Life to Do" list. I should organize my life better, just as I should organize the chaos in my home and school offices, but can't find time to do. My big goals are I want to be liked, to be loved, to be respected for what I do, to put new life into psychology, and better psychology into our lives, and to make all those I come into contact with to feel better about themselves in some way. Beyond those meta-intentions are not specific goals, but rather getting drawn into some proposal or other, giving in to some temptation or other. One of my weaknesses is the inability to say "no;" that personal failure gets me into doing things I should not, but may lead to unexpected joys.

For instance, I give too many guest lectures and colloquia because it is easier to say "yes" than "no" at the time of the cordial request, then I may regret it when it comes to having to prepare them and traveling long distances to the university or convention. The other side of going with flow of invitations is the invitation to be the creator of the Discovering Psychology PBS-TV series for Annenberg CPB. Somebody contacted me from Boston station WGBH, vaguely described the project, which sounded interesting, but might take a lot of time. Yet it could reach a lot of people beyond my classrooms. So, "Why not?" said I. It is a new teaching challenge, it could be fun. They paid me my Stanford release time salary for a year, but it took three years to complete the project. It was as much hard, deadline-dictated work as I have ever done, and forced

me to put my text writing, research writing, and new research projects on the back burner. It actually cost me a lot of money to do this project, since it delayed publication of my textbook by a year, lowered my annual raises at Stanford since they depend on publications (in high citation, refereed journals), and I could not engage in any other supplementary income-making activities.

Because I did not have any long-term goals, I also did not negotiate for royalties or residuals, so have not made a penny from this enormously successful venture. The series is a 26-program introduction covering all of psychology made back in 1989. It has been showing for a decade to PBS audiences throughout the US and Canada, in ten other nations, in most colleges, and now in virtually all high schools. More than 70,000 students in Telecourses have gotten college credit for Introductory Psychology without a teacher, by watching my series and reading a textbook and passing a standardized test. Discovering Psychology series might be my most important contribution to psychology because of its broad and sustained impact on such a wide range of the public in so many countries. I like to believe it is attracting new students to the field of psychology by its appealing presentation of scientific and applied psychology. I hope to persuade the folks who run the show to make two new programs soon, one on cognitive neuroscience and another on cultural psychology, two new realms that psychology has entered in the past decade.

Not having clearly articulated goals has also meant that my research does not have long term focused agendas, but rather flows from topic to topic according to what comes out of a given study, or some idea that I have or challenge a student poses in class. So I end up having done research in a large number of totally unrelated areas because I have gone with what excited me at that moment in time, for example, affiliation, dissonance, persuasion, shyness, time perspective, cults, hypnosis, deindividuation, aggression, psychology of imprisonment, political psychology, prosocial and antisocial behavior, and more.

INTERVIEWER: That's interesting to hear, because I think that most of us probably do react to life more than we like to say. Maybe as Westerners, when we reconstruct it, we say, "I had a plan." We rationalize what we felt like doing at the time. I don't think you're unusual, although you clearly had sub-goals. Once something caught your eye, your goal was to uncover it or discover it, or do a program of research. You clearly had goals that took you beyond a few days or even a few years. You've been in a cultural mix and a university mix that has provided you with plenty of stimulation. And you've had a broad intellect, so you've been interested in too many things. To what degree do you think that psychology has laws like physics or chemistry, as opposed to sociology or cultural anthropology where there is still a lot of subjectivity?

ZIMBARDO: That's a tough question. "Laws" seem so pretentious. There's no question in my mind that psy-

chology has a great many principles, demonstrated proofs, reliable generalizations that have real-world applicability in many domains, such as health, business, education, sports, law, and more. Our dedication to the scientific method and also to using our knowledge to improve the Human Condition, has made psychologists focus their attention on many vital problems that could be understood from experimental analysis, that benefited from the creative use of new technologies, and that had an eye toward application. Much of psychology is in fact, so much a part of our every day life that we take it for granted. We now not only have the dramatic demonstration experiments like Milgram's obedience studies or the Stanford Prison Experiment, but there are lots of psychologists who are doing parametric studies carefully extracting second and third order interactions, developing standard paradigms, tasks, measurement procedures that can be used to cumulate knowledge. That has been missing in the past, our work was too isolated, without the sense of building on earlier work. That is changing, and the best example can be found in the new approaches in the field of memory research. I think psychology has something to say in any domain where it is important to change the behavior of the individual or a small group by changing the context.

Elliot Aronson revealed in his elegant jigsaw classroom studies that it is possible to reduce prejudice in the classroom, enhance self-esteem in students, and also improve academic performance simply by allocating to students part of the information that they had to present as a group to the rest of the class, with each member's grade dependent on the contributions of all members of the group, including the minority kids. This approach has had powerfully positive effects in school systems all over the country.

The real thing psychologists have to offer is that we are sensitive to the dynamics of behavior. We know where to look, we know how to ask the right questions, and we know how to answer them with experimental, correlational, or other methodologies better than any other social science.

Let me mention just a few of the ways in which psychology has had an impact on everyday life, so much so that many of our concepts are embedded into daily practices and the way we see the world. For example, the importance of reinforcing childrens' appropriate correct behavior versus punishing undesirable behavior is now well accepted by most parents. That was a revolutionary idea provided by behaviorist psychologists. Its correlate is never punish the child, you only punish negative, undesirable behaviors. The concept of stress did not exist when I was a graduate student. The importance of stress in influencing cognitive functioning, physical functioning, quality of life has been shown in our research. Then we have gone on to specify how to minimize stress, not only in terms of self-management, but also changing the situation and learning more effective coping styles. That's but one important contribution of psychology to individual and national health.

There is less stigma about seeking out professional, clinical help for mental problems than there used to be. Psychologists have worked very hard to reduce such stigma. We have made inroads in the treatment of many forms of mental illness, improving the quality of life in the workplace, in athletic performance, to mention a few. We have recognized the vital importance of social support networks for sustaining health, and the devastating consequences of social isolation as precursors to pathologies of mind, body and spirit. There are many things that we have done that we can be rightly proud of, but instead we tend to be overly modest about the accomplishments of our field. In our teaching, many of us encourage students how to become more critical consumers of media persuasion tactics, and in the process I think we have helped make our society more sophisticated about sources of social influence.

One big problem, however, in analyzing the impact of psychology is on its effect in changing national institutions, such as the legal system or prisons. They have a very resistant inner structure to external criticism or new ways of doing things. For example, Scott Frazier, a social psychologist, head of Applied Research Associates in Los Angeles, does experimental research demonstrating the invalidity or impossibility of certain kinds of eyewitness testimony done under conditions of low illumination. He does psychometric analysis of the light source, and the distance from target to witness, and what is humanly possible for the sensory system to detect, thereby illustrating that a given witness could not have made his or her identification accurately. Research like that is beginning to change the attitude of some judges and lawyers, but many still resist and do not accept this scientific evidence. Beth Loftus has single-handedly had an enormous impact on the legal system in undercutting the previously alleged belief in the reliability of all eyewitness testimony using her experimental demonstration of the unreliability of memory recall of observed events when post-event information is suggested to the witness. But I and my colleague, Craig Haney, have had little success in changing any aspect of the correctional system from our research, writing, or advocacy. In fact, corrections in the U.S. is in much worse shape now than it was 25 years ago when we did our prison research (as we described in detail in our recent *American Psychologist* article, August, 1998).

INTERVIEWER: Any predictions about where the field will be in 25 years'?

ZIMBARDO: It's so hard to predict the future of the field. Ten years ago, in the last of the *Discovering Psychology* programs, we asked all the eminent psychologists we interviewed, "What do you think about the future of psychology." Half said it was going to become more fragmented with greater specialization; the other half said it was going to become more holistic and unified.

I'm just not sure where our field is headed. One part is clearly on a path to be more directed toward new

technologies of imaging the functioning brain, and will attract more medical students, more post doctoral students, and fewer graduate students. In some departments cognitive neuroscience, or its variants, have split off from the rest of the psychology department. I deplore that change in the field because their students will not be fully educated as they become more narrowly focused on their specialization, and also the rest of the field loses access to these colleagues. As some of us go more molecular, others are moving more molar, for example, with cultural psychology becoming a new influential area of research.

For me, one of the most interesting changes happening right now is that gender reversal in psychology, with fewer males and more females being attracted to our field than ever before. Concurrent with that change is the increase in minorities majoring in psychology, and the reduction of white males (who may be attracted to newly emerging technology fields). It is interesting to consider what kinds of issues this new breed of psychologists will focus on, what kinds of research paradigms they will use, and how the field will be modified from what it was when I started, which was almost totally male-dominated? Surely greater emphasis on the positives and less on negatives, more on relationships, health, prosocial behavior, and less on aggression, illness, antisocial behavior. But perhaps also more correlational and applied than experimental and purely scientific, and surely less use of deception in research.

It seems to me that over time, psychology will become increasingly influenced by women psychologists as they move up into positions of power, especially as the old guard of white guys retire, or die in the next decade. I'm curious about how social psychology in particular, and psychology in general, will change as more and more women come to influence the agenda of research, teaching and publishing. There may be less interest in control as one of the main goals of psychology than on understanding, or finding meaning. I am hoping that as more white boys go into the specialized fields of psychology, like computational neuroscience, more women from diverse backgrounds will take up the Generalists' banner and champion the search for broad patterns of behavior. These patterns can lead to better ways of applying psychological knowledge to enhance the quality of everyday life.

INTERVIEWER: Thank you for your time and insight, Dr. Zimbardo. ΨΨΨ

Book Review

Kessel, F. (Ed.). (1995). *Psychology, science, and human affairs: Essays in honor of William Bevan*. Boulder, Co.: Westview Press.

Review by Robert Perloff

Introduction. Sixteen original essays, plus an introductory chapter by the editor, Frank Kessel, pay tribute to an extraordinarily productive and influential psychologist, scientist, scholar, university leader, and a wise and gifted official of many organizations, including executive officer of the American Association for the Advancement of Science and president of the American Psychological Association. Another section discloses his numerous contributions and affiliations and the text of the "Gold Medal Award — William Bevan," — which includes a selected bibliography.

These 16 original essays, over and beyond their accolades for Bevan, offer thoughtful and significant discourses covering a variety of substantive areas in psychology as well as observations about the organization of psychology, research enterprises, and public policy.

Additionally, this remarkable book, whose dust jacket bears a memorable photograph of Bevan; has an afterword by Bevan himself; a useful set of descriptions of the 16 contributors and the editor, Frank Kessel; a fine biography of Bevan; and one of Bevan's most celebrated essays, "Contemporary psychology: A tour inside the onion," which appeared originally in the *American Psychologist* (1991, 46, 475-483).

Of particular interest to general psychologists, aka members of APA Division One, The Society for General Psychology, for which Bevan served as President (1979-1980) is the transcendent theme of Bevan's underlying philosophy, that is, 1) the importance of achieving coherence in psychology, the need to rise above needless specialization, divisiveness, and fractionation, 2) the importance of achieving, so far as this is possible and desirable, unity among the various branches and specializations in psychology, and 3) the importance of identifying and reinforcing the threads that are common with respect to professional psychology and research/academic psychology as well as to applied psychology and so-called basic or fundamental psychology.

Although Bevan's roots are in experimental and laboratory psychology, he is no snob, no holier-than-thou personage peering down upon us commoners in psychology from an exalted perch on Mount Olympus. As a matter of fact, his catholicity is manifest dramatically in one of the giant strides he took on behalf of psychology before he was forced into retirement for illness-induced reasons. That is the visionary health, including mental health, research networks that he persuaded his then employer, the John D. and Catherine T. MacArthur Foundation, to undertake, an activity described in rich detail by Norman Garnezy (1995) in his chapter in this book, "Development and adaptation: the contributions of the MacArthur Foundation and William Bevan." It should be noted here that the Health Program of the John D. and Catherine T. MacArthur Foundation embodies the largest supporter of research in mental health outside of the United States government.

This foray into mental health was nurtured, mind you, by someone (Bevan) whose early scientific moorings were amid the likes of activities including limens of visual form

perception, thresholds of visual form, scotopic sensitivity in human vision, psychophysical judgments, and other solid stuff that bench scientists muck around in, the point being that Bill Bevan's interests ran the gamut from the highly scientific to the pressing problems of mental illness and the leanings of humanistic psychologists. Speaking of Michelangelo! Yes indeed, William Bevan is an authentic Renaissance person, a general psychologist's general psychologist, a *primus inter pares* psychologist of the most imitable variety. We Division One (Society) types should be honored that he has cast his lot with our passion for a general psychology, a coherent psychology, a psychology which enshrines the basic verities inherent in all of psychology's variegated and disparate venues. Bill Bevan is our guy! And Frank Kessel's book tells us why and documents Bevan's initiatives in every nook and cranny of psychology, including his magnificent efforts to influence and shape public policy based, in part, on the theory, research, and findings indigenous to all of psychology.

On the other hand: To introduce some balance in this review and to establish this reviewer's credibility for being reasonably objective, here are some ways that the book might have been improved. To begin with, a subject and name index would have been useful.

The book's chapters are organized into four parts: "Philosophy and History," "Development, Mind, and Body," "Applications and Public Policy," and "Psychology Evolving?" A preface to each of these four sections, and possibly a summary for each, might have enabled the reader to better appreciate and savor the themes each section was hoping to delineate, the orthogonality, if you will, of each unique part of the book.

Penultimately, while this might well have been unduly burdensome for Bevan, I would have liked to learn about his views as to how we might endeavor to grapple more effectively with the problems that his writings have identified. In other words, where do we go from here and how do we get there? A more or less comfortable way to have developed such a retrospective — minimizing the burden on Bevan — would have been to have a series of questions, by say the editor, and answers given by Bevan.

Finally, it would have been revealing for us to read something from people who do not share Bevan's laments, to see why they disagree with him, and for them to articulate why they believe their "world view" of psychology is superior to his.

The Themes and Issues Bevan has Addressed. William Bevan, the scientist and scholar, as well as William Bevan, university statesperson and association official, has written, spoken about, and maybe even bled over the social and human dimensions of science, the nature-nurture controversy, cognition and psychodynamics, applications (for clinical, forensic, and organizational psychology), behavioral science and the political process, and the problem of specialization and fermentation within psychology;

About the university: (from Kessel, 1995, pp. 2-3, quoting Bevan): "The central mission of the university is 'the advancement of learning through the transmission and enlargement of knowledge' ... The university neither is, nor should it be mistaken for, a social service or social action agency ... it cannot be expected to assume responsibility for the quick and dirty solution of every everyday community problem of a local or even a broader nature that comes down the pike. But neither can it get away with a splendid ivory-towered isola-

tionism. It is an institution embedded in, and many unbreakable ties to, the larger society. Above all, it is the recipient of the latter's tolerance as well as its largesse. And to affirm the university's obligation to fundamental scholarship in no way warrants a denial of the real world ... " **About science and technology** (from Kessel, 1995, p. 4, quoting Bevan): "Scientists must take science and, as one great poet of an earlier day (Shelley) urged as a task for the artist, 'assimilate it to human needs, color it with human passions, transform it into the blood and bone of human nature.' For its part, the lay public must in turn be willing to seek a better grasp of both the logic and the intuitive character of science and technology — no matter how complex and difficult to understand in the individual instance they may be — as first of all companion approaches to solving problems that combine logic with observation and that, in the general case, are simple and straightforward and even beautiful in their conceptualization ... " **The central conceptual problem for behavioral science:** Bevan (1995, p. 435) "arrived gradually at the conviction that the context problem, when properly defined, is the central conceptual problem for behavioral science." **Psychology has many axioms, but few indisputable data:** "Much of what I [Bevan] have said ... adds up to one thing: the need for constantly maintaining an attitude of self-criticism to balance against our imagination. Einstein, when once asked how he came upon the principle of relativity, is said to have answered: 'By challenging an axiom.' Psychology has many axioms, but few indisputable data. Challenging axioms is a habit we can afford to cultivate" (Bevan, 1995, p. 24).

Summaries of the Book's 17 Chapters

Introduction and Overview of William Bevan's career. "A journey at the dangerous edge of things: some reflections on William Bevan's Legacy," by Frank Kessel. Kessel provides a copious reprise of Bevan's writings, particularly those that are in the public policy and "axiom-challenging" domains. Anyone interested in obtaining a relatively quick, though precise and coherent, perspective on Bevan's wisdom and observations, will find that Kessel's chapter does the job. Of the 23 references provided, 18 are those of Bevan.

Part One. Philosophy and History

1. "The human agency of science," by Sigmund Koch. Koch lionizes a mentor that both he and Bevan admired on the Duke faculty, where both received their doctoral degrees. That mentor was Karl E. Zener, "a man who, though he published little, was, in the opinion of those who experienced his thinking, one of the most profound psychologists in the century" (Koch, 1995, pp. 17-18).

Koch summarizes the attributes and values that he and Bevan share: the belief that psychology is not a coherent science, indeed that many of its subdivisions are not "science" as conventionally defined; that much of the research literature in psychology is mindless and trivial; and that the distinction between "applied" and "pure" psychology is an oversimplification and not warranted. They both yearn for a "fundamental" psychology which addresses important human, individual, and societal needs.

Koch applauds Bevan for establishing the MacArthur Foundation's health, including mental health, networks, remarking that "... Bevan, who via his conceptualization of the MacArthur networks and his dedicated husbanding of them, has done something concrete, large and bold to reorient psychology toward its human context — and responsibility" (p. 19).

Part 1 of the Koch chapter dwells in fascinating detail on the thinking and musings underlying his magnum opus, *Psychology: a study of a science* (Koch, 1958). Part 2 probes and extends his (and Bevan's, too) rejection of the "value-neutrality" of science, and their concern with how "fact" and "value" are related one to the other.

2. "Taking human nature seriously: psychology and the polity," by Daniel N. Robinson. In Part 1 of his iconoclastic essay, Robinson challenges the validity of four so-called "protected assumptions" which have exerted, in his light, a pernicious effect on psychology: 1) An acceptable theory of human nature must fundamentally be evolutionary or Darwinian in nature. Not so, avers Robinson. [This reviewer dissents from Robinson on the grounds that, simply put, we have all evolved from genetic/environmental antecedents and therefore it is unthinkable that a theory of human nature should exclude evolution or Darwin.] 2) "Motivation ... is to be understood within the context of challenges to survival" (Robinson, 1995, p. 35). 3) "Understanding human actions requires an examination of the pattern of motivations to which these actions are beholden" (p. 35). 4) "Although ... the accounts of human actions may be presented as something of a folk-tale or narrative, a genuinely scientific account shadows such talks and is attained finally through appropriate reductive models and arguments" (p. 35).

In Part 2 of his article, Robinson avers "... a sketch of arguments favoring the conclusion that these four assumptions are either false or inconsequential or logically incoherent" (p. 36) is a discourse on culture and politics. In my view the highlight of Robinson's Part 3 is his conviction that "... a reductionistic psychology or one tied to a deterministic metaphysics ... must be self-defeating and finally self-destructing: "p. 44) In Part 4 Robinson declares that the central subject of psychology "... is the person in culture, the person as at once the agent and the creature of culture, and in both cases entirely aware of the fact. The last thing we need now is a psychology of sleepwalkers or robots or assembly-line social units conforming to the tyranny of linguistic or other conventions" (p. 44).

3. "Metatheory in psychology and a psychology of metatheory: the case of William James," by John B. Conway. Conway explores "the possibility that our metatheoretical beliefs about what psychology is, or what it should be, are influenced, in some part, by who we are, by personal experiences and personal qualities. The idea that personal influences are at work in shaping one's intellectual views about the nature of the discipline is not an idea that has had much currency among psychologists. In the first part of this essay, I make a case for the value of this idea. Then ... I consider how some of the personal experiences and personal qualities of William James may be seen as having had their influence on James' philosophical views on the nature of psychology" (Conway, 1995, p. 46).

Conway concludes his article by acknowledging "That there are other views to be had but I can do no better than to summon Bevan's (1991, p. 482) words, pleading that 'our reality is formidably complex, dominated by asymmetries, and forever challenged by the unpredictable.'"

4. "Stargazing: James McKeen Cattell, *American Men of Science*, and the reward structure of the American Scientific Community, 1906-1944," by Michael M. Sokal. Among other things, in this instructive intrusion into the history of science, Sokal addresses Bevan's concern with and abhorrence for the modern-day scientist's quest for stardom, for salary

increases, to gain promotion, and to obtain status among one's peers. Sokal says it was always thus, giving a number of examples, including in the 1740s Benjamin Franklin's seeking Royal Society Fellowship for his electrical theories. In his chapter Sokal "seeks to detail the history of American scientists' preoccupation, from 1906 through the mid-20th century, with another kind of star, one more closely related to Bevan's than to Galileo's. This historical exercise seeks to respond to many of Bevan's continuing concerns by shedding light on the origins and evolution of America's current scientific reward structure" (Sokal, 1995, p. 65).

Sokal then goes on to shed light on the similarities and differences between James McKeen Cattell and William Bevan, both of whom held leadership positions with the American Association for the Advancement of Science and with the American Psychological Association. Cattell, however, was notorious for his inability to get along with others, while "... many of Bevan's students and colleagues continue to think of, and profit from, his kind and generous support for their efforts. No wonder this festschrift has attracted so many eminent contributors!" (p. 66). Cattell's reward structure centers upon his editions of *American Men of Science* and the asterisks he attached to their names, identifying the "most eminent" American scientists of the day. Much of this chapter is saturated with the pros and cons, yeas and nays, advantages and disadvantages of the Cattell asterisk/star system of labeling different strata of scientific distinction. The Cattells — James McKeen and his son Jaques — were resistant to change in their classification systems for recognizing scientific levels of stardom. "Thankfully," Sokal assures us, "American scientists have not always responded with such resistance to changing contexts and their consequential pressures. And in this respect Cattell could have learned much from William Bevan. His career and breadth of interest and his adaptive responses to changes in his world could serve as a model for late-20th century scientists and scholars, whom he honors by accepting the essays in this volume. And if these help at all to promote the goals to which Bevan has devoted his professional, life, they will have served their purpose admirably" (pp. 79-80).

Part Two: Development, Mind, Body

5. "The nature-nurture controversy in social and historical perspective," by Frances Degan Horowitz. Her review of the nature-nurture controversy "has been fashioned so that the main theme ... is considered in the context of three factors or conditions at different periods of time over the last 150 years: 1) the nature and growth of our empirical knowledge about the development of children; 2) the character of the prevailing developmental theories; and 3) the social and political currents of the time" (Horowitz, 1995, p. 89). Horowitz's lucid and instructive discourse is organized into these sections: early formulations, Gesell and Watson's contributions to the debate, the era of political environmentalism, and Horowitz's "current reflections," including her belief that "the simple assignment of percentages of influence to genes will become a scientifically obsolete practice" (p. 96).

Among several emerging formulations Horowitz points to dynamic systems theory and the structural behavioral model. "If properly used and developed, these formulations may be able to transform a persistent and ill-advised debate into a scientifically fruitful enterprise that will, at the same time and perhaps more importantly, contribute to instructive discussions of the problems in our society" (p. 97). [Fran

Horowitz's demanding responsibilities as president of the Graduate School and University Center of the City University of New York have not dampened a whit her sharpness as a scientist and as one of developmental psychology's most preeminent thinkers. Good for you, Fran!]

6. "Bevan's wisdoms," by Jerome Kagan. Kagan argues persuasively, on rational grounds as well as in terms of empiricism illustrated through his own copious and creative research, that the quest for symmetry in understanding behavior is a snare and delusion, toward which he makes a strong case for the presence of asymmetries in nature, which is also a pervasive belief of Bevan's over the years.

Kagan's second major point in this chapter stresses the need to be prepared for surprises. His "A preparedness for surprises" is introduced thus: "In a review of a biography of Niels Bohr, Wilczek (1992) commented on Bohr's appreciation of the inevitability and importance of unexpected observations. Perhaps that is why Einstein described Bohr presenting a scientific talk as someone perpetually groping for, but never in possession of, the truth" (Kagan, 1995, p. 103). Kagan then goes on to convincingly make the case for surprises through walking us through a good bit of his solid research. In his "Concluding Comments," Kagan characterizes Bevan as "a realist, but not a skeptic; careful, but not timid; brilliant, but not an egoist" (p. 107).

7. "Development and adaptation: the contributions of the MacArthur Foundation and William Bevan," by Norman Garnezy. This chapter describes the Health Program of the John D. and Catherine T. MacArthur Foundation, which in its beginnings was directed and led by William Bevan. When Bevan formulated the Health Program he had these goals in mind: "continuities in healthy development over time; the significance of deprivation and disadvantage as potential disrupters of positive health status; the integration of scientists of varied disciplinary backgrounds who shared a developmental focus and supported collaboration in developing an integrative, innovative, and representative set of core contents" (Garnezy, 1995, p. 110).

Garnezy goes on to describe the taxonomy of the MacArthur research networks, a crucial feature of which is Bevan's unwavering interdisciplinary vision.

8 "Context defines psychology," by Gregory R. Lockhead. Lockhead parses the classical psychophysical formulations of Wever and Zener (Bevan's legendary mentor at Duke), particularly their method of absolute judgment and shows that the intrinsic meaning of absolute judgment is not antithetical to the emphasis on the importance of context which is the hallmark of Bevan's psychological credo over the years. This point is explicated by Lockhead in his section on "Why 'Absolute' Isn't Absolute." This is followed by an incisive examination of Hany Helson's Adaptation Level Theory, which Lockhead discusses under "The Prototypical View."

Lockhead asserts that Bevan's conceptual metaphors "are all consistent with the fact that, our beliefs or wishes notwithstanding, we really do not know with certainty what belongs to Psychology and what does not. Because of this ... it may be more efficient to leave Psychology undefined and continue blindly measuring what seems to be important ... This might allow the discipline(s) to gradually define (themselves) based on acquired knowledge rather than on preconceptions of what the facts are, or what the discipline is or should be" (Lockhead, 1995, pp. 133-134).

To me, however, after struggling — probably unsuccessfully — to keep up with the appetizing abstractions concocted by Lockhead, the real payoff from his dizzying twists and turns conceptually and philosophically is seen in the last few lines of his summary, which for me puts context in, well, in its proper context: "What an attribute is perceived to be depends on its environment [aka context]. The same is true for words in sentences and sentences in paragraphs [and, I would add, for paragraphs in chapters and for chapters in books]. We only know what a stimulus or a word or anything else is in terms of its relation to other things. Psychology must similarly be defined by converging arguments that relate differences or relations. Instead of models of elements or words we need models of differences, of context" (pp. 136-137). Is there a psychologist who exists who would be brazen (and senseless) enough to quarrel with the underlying truth and good sense inherent in this paean of Lockhead (and of Bevan, too, to be sure) to the essentiality of context? I don't think so. From this point forward I shall be an unreconstructed believer in the role of context in psychology. Thank you, Gregory R. Lockhead and William Bevan! You've made a true believe of me.

9. "Cognition, Psychodynamics, and Control of Experience," by Mardi J. Horowitz. In this chapter Horowitz offers up a feast of intellectual delectables or what he calls a "convergence between psychodynamics and cognitive science" (Horowitz, 1995, p. 138). Three aspects of this convergence, make his chapter a veritable page-turner, these aspects, as formulated by Horowitz, being states of mind, schemas of relationship between the self and significant others [so here is where the phrase, "significant others," originated], and control processes.

An instructive and appealing way that Horowitz might have ushered in his titillating essay would have been to place his "summary" at the beginning of the chapter: "The merger of psychodynamic and cognitive points of view is part of the revitalization of an intrapsychic and psychological approach in psychiatry and clinical psychology. It uses a language that applies to conscious experiences, and accepts variations of personality across states of mind and structural arrangements of schematized information. The processing of ideas and emotions is considered in relation to processes of control that can, for defensive reasons, alter concepts, forms of expression, and schematic activation. The convergence of theory provides a route to study of emotional conflicts and contradictions in personality structure, and usefully follows a path of interdisciplinary convergence illuminated and encouraged by Bill Bevan" (p. 146). Now that's what I call heady stuff, a Mardi Gras that would make New Orleans proud!

Part Three: Applications and Public Policy

10: "Expert psychological testimony about eyewitnesses: An update," by Howard Egeth. Egeth is grateful to Bevan for showing "that a person can have interests in broad social and policy issues and yet still do serious research" (Egeth, 1995, p. 151), and this chapter is an exposition of just such an area of research which has great impact on an important social issue, specifically, according to Egeth, when it comes down to the proposition that "much expert testimony about eyewitness identification is not yet scientifically justified" (p. 151). Hence, scientific research on eyewitness testimony that informs testimony given by witnesses in courts of law represents an important contribution by psychology that is clearly in the public interest.

Following a discussion of the rationales for the use of expert

testimony, Egeth explicates the various factors affecting eyewitness testimonies. Such work, Egeth stresses, "... is complex and very difficult but the potential benefits to society are commensurately great ... and the kind of work Bill Bevan has encouraged throughout his career" (p. 163).

11. "DSM-IV Process and Outcomes," by Peter E. Nathan. Nathan, who spent much time working closely with William Bevan at the John D. and Catherine T. MacArthur Foundation, before Nathan went to the University of Iowa as provost and Foundation Professor of Psychology, in this chapter outlines the achievements of the DSM-IV (Diagnostic and Statistical Manual of Mental Disorders) process. After walking the reader through the steps involved in developing DSM-IV, Nathan apprises us of an important semantic aspect of DSM-IV, and that is the definition of mental disorder, which is sufficiently important, I believe, to reproduce here, word for word as Nathan puts the definition together: "... the term 'mental disorder' unfortunately implies a distinction between 'mental' disorders and 'physical' disorders that is a reductionistic anachronism of mind-body dualism With these words the drafters of DSM-IV reject a definition of mental disorder that would view it as exclusively biological, or social, or psychological, in favor of a definition that emphasizes the interaction of all three factors in the etiology and course of most psychopathology," reflecting "a growing consensus ... that no single factor can by itself explain the variance associated with virtually every psychopathologic condition described by the instrument" (Nathan, 1995, p. 173).

12. "Organizational Management: A point of view" by Milton Grodsky. Grodsky's essay suggests "some antecedent conditions relating to the development of management processes which have contributed to our current difficulties" and offers up the "reforms necessary to avoid unwieldy, unproductive responses to a changing economy" (Grodsky, 1995, p. 179). In this chapter Grodsky describes how William Bevan has helped him think about important issues of organization and management.

Grodsky [amusingly, in typing his name I often come up with "Godsky," implying that he is some kind of Russian deity, but that is not really how I view Grodsky!] then goes on to present his version of what an integrated management system should look like. This system embraces four interactive factors: leadership, vision, strategies, and culture.

13. "Scientific Freedom and Responsibility," by Richard Trumbull. Part 1 of this paper delves into the work of the American Association for the Advancement of Science's (AAAS) Committee on Scientific Freedom, which touches upon "the scientist and the public," "scientist versus scientist"—"Whether attributed to absent-mindedness, unconcern, arrogance, or indifference, the scientist's disregard for his public image is most evident in his relationships with other scientists" (Trumbull, 1995, p. 198), and "young scientist versus old." (Trumbull was Deputy Executive Officer of AAAS when Bevan was the AAAS Executive Officer.)

In Part 2, Trumbull discusses terms of employment, processes of science, products, users, society, and personal conduct. Part 3 of this paper focuses upon the AAAS Committee's charge "to recommend mechanisms to enable the Association to review specific instances in which scientific freedom is alleged to have been abridged or otherwise endangered or responsible scientific conduct is alleged to have been violated."

14. "Why Behavioral Scientists Must Take Root in the Politi-

cal Forest, and How They Can Find Their Way There," by David Johnson. Johnson describes very readably and convincingly the political process in which laws are made and the personalities and skills of the lawmakers. His discourse captures much of the essence of public policymaking. Scientists must, Johnson declares — and no one can really quarrel with him, while each of us should take him very seriously if we are to earn the resources to carry on research and policymaking activities aimed at improving the common weal through adherence to sound behavioral science theory and methodology — that scientists should become involved in the political process, for three perfectly good reasons: 1) mere survival, 2) scientists have a civil obligation to serve, and 3) participation will lead to better science. Amen. Reverend Johnson!

Part Four: Psychology Evolving?

15. "Psychology has a rosy past, present and future," by Wendell R. Garner. "A major theme of this essay concerns interactions that exist between different areas of psychology and between different disciplines. It therefore seems appropriate that, since this is an essay written in honor of William Bevan, I should start with some background on the interactions between the author and the man being honored" (Garner, 1995, p. 231).

Bevan and Garner, have in common not only the fact that each at their names consists of two syllables, but they also attended Franklin and Marshall College in Lancaster, Pennsylvania. There they both admired unstintingly their mentor, psychology professor Paul L. Whitely. [In his "Afterword" Bevan (pp. 261-262) recalls wistfully that "During our years in Chicago my wife, Dottie, and I looked forward to spending a weekend each summer with Dr. Whitely at Lone Elm, the family farm near Fairmount, Indiana. There, while sitting on the back porch in the evening, we would enjoy Dr. Whitely's home-baked apple pie and discussion of some of the big questions in psychology. The affection and respect in which we held PLW is reflected in the fact that neither Dottie nor I could ever call him by his first name, as he always urged us to do." (p. 261) [reminding me (Perloff) that I could never bring myself to call my Ohio State mentor, Robert J. Wherry, "Bob," as he urged me to do].

Continuing on the good old ship "nostalgia," Garner recalls that "There was one other interaction that did not last as long as I had hoped. In 1966, Bevan came to Johns Hopkins as Vice President and Provost, and I anticipated many years of close interaction. However, in that same year I left Hopkins for a year's sabbatical in England, and while there decided to move to Yale University. So that anticipated long interaction never came to fruition, to my regret" (p. 232).

Garner goes on to document his and Bevan's discomfort with and disapproval of the extant dichotomy between applied and basic research, reminding us that Bevan, in his infinite wisdom and unbounded belief that psychology's tent should proudly house both applied and basic research and researchers. Both he and Bevan agreed "that basic science does indeed need to justify itself, and that no science is above being judged by the society that supports it" (p. 233). Bevan had argued earlier "that scientists must try to make explicit the implications of their research, for if society as a whole is to support basic research, that society has a

right to expect something in return" (Garner, p. 233).

Garner's next section is on "The psychological generalist," in which, he enlightens us, it is a myth to assume that in the good old days the prominent psychologists were generalists and that the beast of specialization is only a latter-day sin. Not so, Garner reminds us by reviewing each of the "seven psychologies" portrayed by Edna Heidebreder in her classic *Seven Psychologies* (1933). Garner rubs our noses in the fact that each of these "seven psychologies"—those of 1) William James, 2) dynamic psychology and Columbia University (R. S. Woodworth and particularly E. L. Thorndike), 3) functionalism, 4) behaviorism, 5) Titchener and structuralism, 6) gestalt psychology, and 7) Freud and the psychoanalytic movement—were general neither in their methodology nor in their subject matter. For example, take behaviorism, whose primary subject was "learning, especially animal learning, and there was little concern for perception, thinking, and other more cognitive processes" (p. 235). So the good old days were not so good after all.

Garner then introduces us to "The New Generalist," generalism in the form of interdisciplinary approaches. "These may well provide more generality than the purported generalist of the past" (p. 239-240). Now, what about the generalist of the future? Garner feels "that there will and should be generalists in the future.. But they will be teams of individuals representing different specialties" (p. 242).

Garner is upbeat about the future, thus: "The past, present, and especially the future of psychology do look rosy. Boundaries of all sorts are breaking down. Basic scientists interact with applied scientists, and that fact in itself means that scientists from different disciplines will collaborate more The generalist of the future will not be an individual, but a team of individuals, and this team will most definitely be a generalist" (p. 235). One more thing needs to be said and that is that psychology's future will be assured if it is populated by the likes of Garner, Bevan, and the author of the following and last essay in this remarkable little volume, the one and only Anne Anastasi. .

16. "Psychology Evolving: Linkages, Hierarchies, and Dimensions," by Anne Anastasi. Anastasi pays tribute to William Bevan, in the beginning of this exquisite essay by asserting that "whenever an administrative leadership role became especially complex or challenging, people called upon Bill to see it through. I have personally witnessed Bill charring, with his characteristic wisdom, forbearance, and fortitude, some of the most difficult meetings I have ever attended." (Anastasi, 1995, p. 245).

In this chapter Anastasi highlights its theme from Bevan's substantive writings on contemporary psychology" whose effect on her is "disquieting, with occasional waves of pessimism. But in their final impact, optimism wins out. The overall effect is energizing, impelling me to act and to do a lot of hard thinking," urging her to "want to pull together some of the ideas I [Anastasi] expressed in these papers and toward which I have been moving over many years" (p. 245).

In her section entitled "A search for unifying linkages in modern psychology," Anastasi asks us, amid the swirl of ever more narrow fields of subspecialization,

many of which are necessary "in order to attain sufficient depth of knowledge to make an effective contribution to either research or practice . . . " " . . . [we should] remain cognizant of a common core of methodology and concepts in psychology and should maintain communication across relevant specialties" (p. 242). And this latter admonition, to "maintain communication across relevant specialties" is precisely the mantra, the sermon, the importuning of our friend and colleague, Peter Salovey, editor of our Society's Review of General Psychology. [Peter, why not ask Anne and the book's publisher to reproduce Anne's chapter in a forthcoming issue of the Review?]

In her next section, Anastasi leads us through, happily and optimistically, a mosaic of unifying trends that would appear to brighten the hearts of those of us who worship at the shrine of generalism, the big picture, of pulling things together. These trends embrace theory and empirical research, basic research and practical applications, emergence of integrating fields, the new look in ["in," mind you, not "at" — and there is a difference] behavioral genetics, merging trends in psychotherapy, and bridging islands of research within a topic [which she breaks down into "trait hierarchies" and "trait formation"].

And heed ye all this bombshell of a concluding paragraph — wow! "Undoubtedly, there are many more specialists than generalists in psychology today — and that is as it should be. More specialists than generalists are needed to carry on the work of psychology. There are specialists of different degrees of specialization and generalists of different degrees of generality. It takes only a few generalists to identify and develop the broad, overarching linkages [and such generalists are urged to publish their observations in Peter's journal]. But even the most specialized specialist could — and should — identify and promote the mini-linkages at the base of the structure, out of which the more inclusive linkages can be built. The diversity of current linking efforts sampled in this essay provide grounds for optimism about the future of psychology. There are signs that the discipline is responding to the challenges so vigorously posed by Bill Bevan, the most general generalist I know" (p. 242).

Concluding Remarks

One of the main reasons I'm glad that I decided to invest the time, the blood, the sweat, and the tears into this review is that, sadly, I understand that the book is out of print and it would be a tragedy for such a monumental work, about a monumental person, and peppered with brilliant and creative essays, to go unnoticed. Westview Press should be taken to the woodshed for keeping this trailblazing book as one of the best kept secrets of our time.

This book should be read not only by dues-paying members of the Society for General Psychology, but by every member of the American Psychological Association and of the American Psychological Society. It should be in the library of each of our institutions, offices, clinics, hospitals, departments, wherever, everywhere. It contains an illuminating blueprint as to where psychology should be headed and where it came from, identifying psychology's warts and beauty marks.

To show that I'm not alone in this glowing assessment, I reproduce, below, blurbs from the dust jacket, blurbs which reinforce my own limitless adulation for Bevan and high regard for the book's editor, Frank Kessel, and of course for its 16 superb chapter contributors.

Richard Jessor: "Wide-ranging and provocative, these essays capture — and celebrate — the vision, the wisdom, and the social commitment of a leading American psychologist. A refreshing and stimulating read — there is something here for everyone concerned with the making of psychological science and with its contribution to society."

Mortimer Herbert Appley: "Sixteen distinguished scholars review developments in psychology, science, and academe over a turbulent half-century to celebrate Bill Bevan's multifaceted leadership roles, as worrier and warrior, in all three areas. This is an excellent tribute and rewarding reading."

Gregory A. Kimble: "This book is dramatically successful in demonstrating Bill Bevan's influence on psychologists and psychology. The authors are all experts in a substantive field of scholarship and present lucid summaries of both research and theory. The relationship between scientific knowledge and human welfare is treated with honesty and candor. This book deserves the thoughtful attention of every psychologist."

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Here is the citation accompanying the Gold Medal Award for Life Contribution by a Psychologist in the Public Interest that was presented to Dr. Bevan by the American Psychological Foundation in 1991 (American Psychological Association, (1991). Gold Medal Award — William Bevan. *American Psychologist*, 46, 275-281):

"In recognition of a lifetime of service as a far-sighted and demanding yet humane and compassionate academic administrator at three of the nation's great universities; as a foundation officer and program director who fostered development of an innovative, comprehensive behavioral science research program of the highest quality; as the executive officer of one of nation's leading broadbased scientific organizations and the publisher of its distinguished journal; as a generous contributor to his profession in its leading organizational roles; and as one of the early, most influential leaders in research on human cognitive functioning; Throughout an extraordinarily distinguished career, William Bevan has met a single standard: excellence in everything he has undertaken" (p. 778).

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[The reader may be interested in knowing that Perloff and Kessel are attempting to find a way to have the currently out-of-print book reprinted. Stay tuned.]



An Interview with Dr. William Bevan

The Editor asked old friend and fellow Dukie Dr. Bevan to supply us with an autobiographical account to accompany Dr. Perloff's review just preceding. Bill complied with the following:

I completed my high school career by graduation from Staunton Military Academy, Staunton, Virginia in the spring of 1939. I had gone there in the hope of obtaining an appointment to the United States Military Academy, West Point, New York. The appointment was not forthcoming. Thus I faced the task of selecting a college that summer. I was interested in Franklin and Marshall College in Lancaster, Pennsylvania and I entered there that fall.

I enrolled in my first psychology course in the spring of my sophomore year. The professor was Dr. Paul L. Whitely, who was the finest teacher I have ever known. Dr. Whitely had taken his Ph.D. at the University of Chicago under Harvey Carr. Over the long years of our friendship, he encouraged my wife, Dottie, and me to address him by his first name. This was something we could not bring ourselves to do because of our great admiration and affection for him.

After three years at Franklin and Marshall, I found that I had accumulated enough credits to graduate with the A. B. degree with honors. A member of my class was Wendell R. Garner. Dr. Whitely encouraged both of us to apply for graduate study at the University of Iowa in Iowa City, where his longtime friend, Dr. John A. McGeogh, was a senior professor. His death precipitated a change in my plans. I chose to undertake graduate study at Duke University. Duke was a young institution in North Carolina that had only assumed a place among the universities of the nation. Duke had formerly been Trinity College, a Methodist institution and had for some years received the broad support of the Duke family.

My reason for choosing Duke was that I had acquired an interest in the work of Professor Karl E. Zener who had come to the University with William MacDougall, the highly respected British psychologist as William Preston Few, then Duke's President, sought to add distinction to the faculty ranks. Dr. Zener held a bachelor's degree from the University of Chicago, a doctorate from Harvard, and a period as a post doctoral fellow in Germany with the group that were the founders of Gestalt Psychology. Zener had over a period of years, developed a research program that had viewed in cognitive terms the phenomenon of the Pavlovian experiment. In the early 1940's, psychology at Duke was housed in the science building on the East Campus, which at that time was the home of Duke's Women's College. Dr. Zener's dog lab was in the basement of this building. Psychology occupied faculty offices on the first two floors of the science building;

graduate students were assigned offices on the third floor or top floor.

During my first year at Duke I completed a Master of Arts degree under Dr. Zener, and the topic was the role of past experience in the perception of visual form. A manuscript based on this study appeared some years later in the *American Journal of Psychology* under our joint authorship. Two other professors at Duke influenced my intellectual development. Dr. Donald K. Adams and Zener had introduced the work of Kurt Levin to American psychology through a translation of a number of Levin's papers in a book published by McGraw-Hill. The other professor was Sigmund Koch who had taken his Ph.D. in 1942 with Adams.

In my second and third years at Duke, I busied myself with three projects. The first involved working as a research assistant with Dr. Zener on an activity, the goal of which was to develop procedures for training aerial observers in aircraft recognition. This study was carried on in cooperation with the U.S. Navy preflight school based at the University of North Carolina in Chapel Hill. The second project was carried out with Dr. Zener under the general direction of Dr. Paul M. Gross, then Duke's provost. The sponsor of this project was the United States Army Air Corps. Duke scientists were charged with developing a frangible bullet, a projectile that was designed to disintegrate on impact. The goal of the project was to improve the accuracy of fire by waist gunners in bombers.

The third project was directed to carrying out a series of experiments on figural after effect, a perceptual phenomenon first described in Wolfgang Köhler's little book, *Dynamics in Psychology*, and dealt with in greater detail in a monograph published by the American Philosophical Society.

The time was the summer of 1944 and I had been notified by my Pennsylvania draft board to report for military induction. I had come to the conclusion that I would fare better in the Navy and asked the draft board to assign me to the Durham, North Carolina Board.

Meanwhile, I learned that the Navy had an office in Raleigh where they were recruiting officer personnel. I went to Raleigh and applied for a Navy commission. Ultimately, I was called up for duty and taken by bus to Fort Bragg, North Carolina and processed for induction. I had learned that the pastor of my home church was serving as an Army chaplain at Fort Bragg who interceded on my behalf and arranged for me to be assigned to the Navy. The Navy ordered me to Princeton University for indoctrination training in the fall of 1944.

As this training program period approached its conclusion, I was interviewed by a senior officer from the

Bureau of Naval Personnel and selected for language training at the Naval Oriental Language School located on the campus of the University of Colorado in Boulder. I was assigned to the curriculum in Chinese. The school taught Mandarin, the national language of China and three other dialects (Cantonese, Amoy, and Fuchow) and my assignment was to Cantonese. The school also taught Russian, Japanese, and Malayan. I completed my language training in the spring of 1946 and was sent to the Office of the Chief of Naval Operations in Washington, D.C. After several weeks of service there, I was released to inactive duty. Later on I spent 26 years as a Naval Air Intelligence officer in the Naval Reserve. I might add that the Office of Naval Research supported my research program over a long time.

In the fall of 1946, I joined the faculty of Heidelberg College in Tiffin, Ohio as an instructor in psychology; I was the psychology department. Dr. Zener and my father had advised me to accept this job because they felt that a period of teaching would provide the experience needed to work myself back into the discipline. After some 18 months in Ohio, I returned to Duke to complete my residence requirement for the Ph.D., which I received in the spring of 1948.

Prior to graduation I accepted an invitation to associate with the department of psychology at Emory University and to teach courses in physiological psychology, perception, and cognitive psychology. It was Emory's ambition to build its psychology department into one of major distinction for the southeastern United States.

While at Emory, I began a highly productive research collaboration with William F. Dukes, a Berkeley Ph.D. As part of our intellectual work together, Dukes introduced me to the research of Harry Helson on Adaptation Level Theory. The concept of this theory allowed me to make sense of psychophysics. During my time at Emory, I spent the 1952-53 academic term in Norway at the University of Oslo as a Fulbright scholar.

I left Emory in 1959 to become chairman of psychology at Kansas State University in Manhattan, Kansas. As soon as circumstances allowed, we brought Harry Helson to Kansas State as the first distinguished professor in the University's history. Helson was the second best teacher I have ever known. After three years at K-State, I became Dean of Arts and Sciences and a year later, Vice President for Academic Affairs.

In 1965-66, I spent my sabbatical year at the Center for Advanced Study in the Behavioral Sciences at Stanford University in Palo Alto, California. While the fellowship year was in progress, Dr. Milton Eisenhower, President of The John Hopkins University in Baltimore, invited me to come to Hopkins as Vice

President and Provost. After four years at Hopkins, I was asked to come to Washington, D.C. as Executive Officer of the American Association for the Advancement of Science and publisher of the magazine, *Science*. I was on leave from Hopkins and continuing a research program there. After four years affiliation with AAAS, Duke invited me to return to my Alma Mater as the William Preston Few Professor of Psychology and as founding director of the Duke Round Table on Science and Public Affairs (1974-83). I next accepted the invitation of Duke's President, Terry Sanford, to serve as Duke's Provost. While in this position, I took an opportunity to establish the Duke Talent Identification Program (TIP). It is an activity directed toward research on gifted young people and bringing a significant number of them to Duke each summer for study in several regular college courses.

In the fall of 1982, President Sanford granted me a leave of indefinite duration to join the staff of the John D. and Catherine T. MacArthur Foundation of Chicago as Vice President and Director of the Health Program. The goal was to develop a series of research networks in the field of mental health. I spent almost a decade pursuing this goal. In the summer of 1988, I suffered a severe embolic stroke that has left me hemiplegic on my right side. I retired from the MacArthur Foundation in September 1991 and returned to Duke in May 1992 only to be retired since I had turned 70.

I have always taken special pleasure in organizing activities in a collegial setting. Serving on numerous committees e.g., those of the American Psychological Association, the National Academy of Science, the National Science Foundation, and the Institute of Medicine I took my responsibilities very seriously. I found that the best position in academic administration was that of the provost or vice president for academic affairs because that officer keeps his or her fingers on the academic budget and by his or her decisions influences the quality of the faculty and, therefore, the reputation of the college or university. In that position, I was the final candidate for several presidencies but did not accept the invitations.

In looking back on my career, I found a real pleasure in having established the Duke Round Table on Science and Public Affairs (1975-83). It was a postdoctoral program for training new behavioral science PhDs for careers as legislative aides to members of Congress. This program was planned as a sequel to the Congressional Science Fellows Program, which was initiated during my term as executive officer of the AAAS. I have also taken great satisfaction in the tremendous success of the Talent Identification Program (TIP) which I started at Duke in 1980 and continue to be involved with it. ΨΨΨ



Psychology Trivia II: Stamps

Gary Brucato, Jr. & John D. Hogan
St. John's University, NY

A recent "trivia question" published in *The General Psychologist* (Spring, 2000, Vol. 35, Number 1, p. 23) asked: Who was the first and only psychologist commemorated on a US Postage stamp? We've seen this question posed several times before in other publications and we disagree with the usual answer.

Lillian M. Gilbreth (1878-1972), psychologist and engineer, did indeed appear on a \$.40 US stamp issued in February 1984 (Scott #1868)¹. Gilbreth received a Ph.D. in Industrial Psychology from Brown University in 1915, probably the first degree of its kind in the US. When she died, at the age of 94, she had become something of a legend, both personally and professionally. The story of her life — and her 12 children — found its way into two popular books and movies.

But there was another psychologist who appeared on a US stamp earlier than Gilbreth. In 1966, a \$.30 US stamp was issued honoring John Dewey (1859-1952) (Scott #1291). It is true that later in his career Dewey was known less for his contributions to psychology and more as an educator and philosopher. Nonetheless, he was a founding member of the American Psychological Association, its president in 1899, and a critical influence on psychology in its early decades. In fact, there are many people who believe that his ideas continue to influence psychology profoundly today. (Biographies of both Gilbreth and Dewey appear in the Division 1 sponsored *Portraits of Pioneers in Psychology, Vol. II*, published by the APA in 1996).

Although the postal record in psychology for the rest of the world is not exactly ample, there are some other psychologists and "psychology-friendly" people who have been commemorated. Wilder Penfield (1891-1976), a neurosurgeon, appeared on a Canadian stamp issued in 1991 (#1303). Penfield is best known for his brain research that explored the physical basis for memory. Although he was sometimes referred to as the most famous Canadian of his generation, he was born in Spokane, WA, and did not migrate to Canada until after his medical training was completed. Another Canadian, James (Hans) Selye (1907-1982), known for his extensive work on stress, was honored with a Hungarian stamp in 1997 (#3578). A World Congress on stress was held in Budapest that year.

Sigmund Freud (1856-1939) appeared on an Austrian stamp in 1981 (#1175) and on a Mali stamp in 1979 (#345), honoring the 40th anniversary of his death. Russia commemorated Vladimir Bekhterev (1857-1927) in 1952 (#1655), Charles Darwin (1809-1882) in 1959 (#2166), and Ivan Petrovich Pavlov (1849-1936) three times: in 1949 (#s 1390-1391), in 1969 (#3649), and again in 1991 (#5999), as part of a Nobel Prize series. Switzer-

land honored Jean Piaget (1896-1980) with a set of five stamps in 1996 (#965-969), the 100th anniversary of Piaget's birth. They depict Piaget in an armchair with children playing around him. These stamps are the only ones in the entire Scott catalogue that use the caption "psychologist," something of a curiosity since Piaget claimed he had never taken a psychology course in his life.

Germany issued two stamps in honor of Hermann von Helmholtz (1821-1894), the first in 1971 (#9N314) and the second, in 1994, on the centennial of his death (#1867). In 1982, Great Britain issued a 4-stamp set on the centennial of the death of Charles Darwin (#965-968). The stamps showed Darwin with tortoises, iguanas, finches, and skulls. In 1959, Poland issued a "Scientists Series" that also included Charles Darwin (#880) on the 100th anniversary of the publication of *Origin of Species*.

Ireland honored Bishop George Berkeley (1685-1753) with a stamp in 1985 (#623) on the 300th anniversary of his birth, and Vatican City celebrated the contributions of Gregor Johann Mendel (1822-1884) in 1984 (#s 729-730) on the 100th anniversary of his death. Rene Descartes (1596-1650) was depicted on French stamps in 1937 (#s330-331) to commemorate the third centenary of the publication of his *Discours de la Methode* and again in 1996 (#2512) to celebrate the 400th anniversary of his birth. Monaco also honored Descartes with a stamp in 1996 (#2015).

These are the only stamps we could find that we thought were related to psychology or its history. A few other European stamps included physicians or psychiatrists but we decided that their contributions were not central enough to be noted here. The Scott Catalogue, which was our principal source, is used by all stamp collectors. It contains the standard listing of every stamp issued worldwide — the only complete listing as far as we know. Without it, collectors would have no way to communicate with one another.

The scant worldwide postal record for psychology reminds us that the American Psychological Association lobbied for a stamp to celebrate its centennial in 1992 but it was not successful. APA had proposed putting William James and G. Stanley Hall on a single US stamp to celebrate American psychology. That proposal still sounds like a good idea to us. And getting back to Lillian Gilbreth — she may not be the only psychologist to appear on a US stamp but she does emerge as the only woman psychologist to appear on a stamp in the entire world. And that's not exactly trivial! ΨΨΨ

¹ Numbers refer to the listing in *Scott 2000 Standard Postage Stamp Catalogue: 156th Edition* in 6 Volumes, Copyright 1999, Scott Publishing Co., Sydney, OH.

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A Message from the Society President

NOT SO WILD A DREAM

One might expect something interesting to happen when a group of intelligent, creative, forward-thinking, and socially conscious psychologists are enjoying a repast and one another's company after a busy Division 1 executive committee meeting. In contrast with the more formal daytime meeting where administrative matters are debated, annual convention details discussed, warnings of nominations deadlines heeded, and awardees chosen, the men and women of the meeting-table now lighten up a bit as the lounge-lamps dim. They toast one another's witty little things said today, and soon fall into discussion of what else Division 1 might do to uphold its reputation as one of the more spirited divisions of APA — the division that honors *General Psychology*.

We are the division that tries to look at the commonalities underlying diverse psychological pursuits. We welcome, integrate, and unify ideas relating to all facets of our great discipline. We seek reciprocating relationships and thought-provocation among scholars and practitioners, learning theorists and cognitive scientists, test developers and test users, quantitative and qualitatively oriented psychologists, clinicians and academicians (even individuals who are both!), and teachers who don't do research and researchers who don't teach. We like to put people together who don't often enough speak with one another, psychologists whom you wouldn't think have much to say to one another.

The spirit of our discussions about promoting *general psychology* has been inspired in large part by some articles that have appeared over the past two years in our divisional newsletter, aptly named *The General Psychologist*. We had an issue dedicated to the phenomenon (indubitably psychological, of *passion*. Generalists spoke of their lives' passionate pursuits, from parenting to acquiring knowledge to having good sex.

Eventually we heard from Bob Perloff. He is an extraordinarily prolific and stimulating writer, as well as a Past President of APA. His article was published in the newsletter's Fall issue of 1999, and was entitled "Three Cheers for Coherence (aka 'Giving Psychology Away')." He asks Division 1, the Society for General Psychology, to give itself some unifying tasks. Bring people together, he urges, to discuss and write integrative, action-oriented papers, particularly with a view toward making Psychology work for the public good. Not surprisingly, Bob is the latest winner of the American Psychological Foundation's Gold Medal Award for Achievements in the Public Interest.



Our executive committee has decided, unanimously, to take up the challenge of Bob Perloff. An initiating committee, which will set the course by creating several task forces, will meet in the near future. Bob will be a member of that committee and will chair one of the task forces. Other members of the initiating committee are Steve Ceci, Wendy Will-

iams, Alan Boneau, Frank Farley, Peter Salovey, Lyle Bourne, and myself.

Stay tuned and we will keep you informed. Also, please step forward if you wish to participate in this exciting venture. We have not decided upon the specific topics we will address, but you can count on them being issue-oriented and sometimes conflict-laden. All of them will involve lively debate, the opportunity for resolution and, most importantly, the vision of diverse facets of Psychology working for the public good. We are passionate about this. It is not so wild a dream. ΨΨΨ

Lewis P. Lipsitt

President, Division 1
The Society for General Psychology

Getting Down to BUSINESS

Executive Committee Minutes

The Division One Executive Committee (EC) meeting was called to order by Dr. Lewis Lipsitt at 8:04 PM on March 26, 2000 in the General Manager's Meeting Room in the Omni Inner Harbor Hotel in Baltimore, MD.

Dr. Lipsitt gave a brief President's report, and discussed various issues within the specific topics as they were scheduled on the agenda. He thanked the EC members present for coming and those who were absent for their work. The minutes of the previous EC meeting, as amended, were approved.

The Secretary's report noted that The Handbook and By-Laws have been entered on computer, so that the revisions started previously can be updated and sent to the members of the EC. After discussion by various EC members regarding deadlines for activities, it was decided that the Divisional Calendar, which is part of the Handbook is the most critical need for the division. Dr. Matthews will make that his top priority. As that work is completed the documents will be added to our website. You have seen our Website at APA.ORG haven't you?

The Treasurer's report mentioned that APA is sending the 1999 IRS statement on March 31, 2000 for review and completion. The current budget as of December 31, 1999 was presented and reviewed, and the Society is in good financial shape.

An overview of the written Past President's report (in Dr. Kurt Salzinger's absence) was given by Dr. Lipsitt. Dr. Matthews mentioned that the 1999 Annual Report of Divisional Activities for APA was sent in.

Dr. Lipsitt also made a presentation to Dr. Farley from Dr. Salzinger and the EC for all of his hard work, covering a variety of positions, as well as being President, both during Dr. Farley's year and in assisting Dr. Salzinger during his year as President.

Dr. Lipsitt and the EC unanimously thanked Dr. Salzinger for his outstanding service to Division One as President last year.

The Year 2000 Convention program planning is underway. The members of the EC who will be chairing the various award sessions (the list of winners was in a previous TGP) was discussed.

The Review of *General Psychology* under Dr. Peter Salovey's direction as Editor continues to be productive. In Dr. Savoy's absence, Dr. Lipsitt reviewed his written report. There have been 51 new submissions since the last report. The second issue for 2000 has been published. Currently there is only a 9.5 week editorial lag time, with the articles appearing in print in approximately 6 months. However, we continue to need a dramatic increase in the number of library journal subscriptions. One issue to be discussed at the August

EC meeting is the need for a new Editor beginning in 2002. Various options regarding the Journal publication were discussed. These included ideas such as production of the journal electronically rather than/or in addition to paper production. Dr. Lyle Bourne indicated that he would be interested in exploring such options about electronic print and will talk with APA staff about this issue. Dr. Salovey and Dr. Matthews met with APA Journal Committee staff on Sunday, August 22 to review the progress of the journal.

Dr. Gregory Kimble sent a written report about the APA Council meeting. He noted that the Blue Ribbon Committee's recommendations regarding the elimination of many functions and boards within APA was not well received. The EC had some discussion regarding the implications of this report and as Council takes up this issue, more information will become available.

Dr. Alan Boneau, as Editor, reviewed the status of *The General Psychologist*. Three issues of the newsletter have been completed so far. A list of deadlines and responsibilities for issues related to the publication will be included in the revision of the Handbook. Dr. Boneau noted that he was seeking input from both the EC and members at large regarding the philosophy of the type of material that should be included in TGP. Ok, members, if you have some ideas, please communicate them to Dr. Boneau. His address and e-mail address are inside the front cover. Dr. Lipsitt and the EC members thanked Dr. Boneau for all of his diligent work.

Dr. Steve Ceci reported on the progress of the Master Lecture series.

Dr. Lipsitt gave the report for the Nominations and Election Committee. The committee consists of the President, Past-President and President-Elect. The nominations for the 2001 division officers' positions for President, Council Representative and the two Member-At-Large positions were finalized and slate will appear in the newsletter.

Dr. Boneau gave the Awards Committee report and provided a handout to the EC members. The Year 2001 Award determinations are in progress, with this year's deadline having been March 15 for nominations. On issue as a result of this change is that publication will need to go out sooner in the future. Issues regarding the number of winners for awards and what should happen when there is a tie for an award, such as splitting the set amount of money for the award or giving two checks, each for the full amount was discussed. Dr. Boneau suggested that consideration be given to a second award (to be named) in place of the Gardner Lindzey award. The criteria for all of the awards and the written procedures for the awards also need to be reviewed. Following some comments from other EC members of these topics, it was decided to refer the matter to Dr. Boneau who was thanked for all of his work on reorganizing and facilitating the activities of this committee.

Dr. Lipsitt encouraged the EC members to think about and assist in naming possible nominations for various APA Divisional awards. In addition, individuals who might be encouraged to apply for APF awards were discussed.

The Fellows Committee report was sent by e-mail from Dr. Morton Ann Gernsbacher. Although she had previously resigned her position of Chair of the committee, she agreed to remain Chair through the current Fellows process. The President, prior to the APA convention will appoint her replacement this summer. Several names for the Chair position were discussed. The EC voted to only consider current fellows and then acting as the Fellows Review Committee generated a list of names of Division one members who are currently fellows in other divisions. The list was then reviewed, and the individuals approved as Fellows of the Division. The names will be sent to Dr. Gernsbacher for notification. A list was also generated of Division One members who are not currently fellows of other divisions and these names will be placed on a list for consideration and contact as possible New Fellows, after the appointment of the new Chair for this committee and the August APA meeting. Dr. Lipsitt expressed the thanks of the entire EC for Dr. Gernsbacher staying in the Chair's position for this current application period.

The need to recruit more generalists and students to the Division was discussed. Drs. Newcombe and Bonecu are to investigate possible recruitment techniques to increase membership. The EC voted to allocate funds to cover mailing and additional printed materials for this activity. Various possible methods were discussed at the meeting, as the Division needs to continue to work on the recruitment of students and women members.

The Divisional Leadership meeting which Dr. Lipsitt attended was also discussed. The President-Elect usually attends this meeting and will in the future, but because of circumstances both last year and this year, the rotation was out of sequence.

Recent Divisional initiatives were briefly reviewed. The Program Chair for the 2000 convention is Dr. Lipsitt. Dr. Don Bersoff has offered to help with the arrangements. Other divisional members are encouraged to volunteer as part of these preparations. The election results for 2000 have Dr. Lyle E. Bourne as President-Elect, Drs. Lynn A. Hasher and R. Duncan Luce as Members-At-Large.

Dr. Donald Dewsbury gave the Historian's report and noted that Dr. Wade E. Pickren has been appointed as the Director of Archives and Library Services at APA and that space has been allocated for storage of materials. Dr. Dewsbury has visited that site. He recommended that a decision be made to place all of our materials in the APA archives. A motion was placed as a new business item on the agenda and subsequently approved later in the meeting. Members of the EC were encouraged to send him documents for inclusion from this past year.

Dr. Michael Wertheimer presented the report on the work that Dr. Kimble and he have done on the Pioneers Series. Dr. Kimble also sent an e-mail review. Volume IV will be in chapter proof form to the authors shortly and the Volume will be published by the end of the calendar year.

Consideration for authors for Volume V is progressing.

President-Elect Dr. Lyle Bourne reported that he is coordinating with the Science Directorate for additional ideas for the 2001 Convention program. The Awards Committee Chairs were appointed for 2001.

Old business items included discussion of the website, with thanks to the work of Dennis Ba Nguyen and Marc Carter, our Webmasters, from the EC. Recent changes at APA Central Office, including Dr. Ray Fowler's leave of absence for 6 months were noted.

Several new business items were discussed. These included the date for the Executive Committee Meeting for Midwinter 2001 to be scheduled in Washington, DC. The specific date will be set at the August EC meeting by Dr. Bourne. Affiliated Organization dues were reviewed and the EC approved membership in the Association for Scientist-Practitioner Psychologists. (A Caucus of the APA Council), and renewal of membership in the Federation. Dr. Lipsitt raised questions about how people are selected to represent APA at public presentations. As an example, he mentioned Dr. Ron Levant's recent meeting at the White House regarding an issue of child development, given his limited background in the research aspects of children. This event brought up a larger matter, regarding expertise as opposed to political office when such situations arise. The EC requested that Dr. Salzinger discuss this matter with the APA Board of Directors. The Interdivisional grant program was also discussed.

The major new business item was a discussion of splitting the duties of the current Secretary/Treasurer position into two positions. Following discussion Dr. Lipsitt moved and it was seconded by that the position be split into two positions. Dr. Frank Farley and other members discussed the advantages of this change. The EC voted to approve the motion. Since such an action requires a change in the By-Laws, it was decided to make this a formal proposal in the next newsletter. The item will be discussed and voted on at the Division One Business Meeting.

Dr. Lipsitt adjourned the EC meeting at 6:15 on March 27, 2000.

Respectfully submitted and I hope to see all of you at the Division Business meeting in Washington, DC. ΨΨΨ

Lee H. Matthews,

Secretary/Treasurer

[As noted above the Executive Committee voted to amend the Bylaws in order to split the function and position of Secretary-Treasurer and create two positions, those of Secretary and Treasurer. The formal amendment, appropriately worded, will be presented to the members for vote at the Business Meeting of the Society on Saturday, August 4, during the APA Convention in Washington, DC.]

Division One Convention Program

All sessions will be held in the
Washington Convention Center
Friday, August 4 - Sunday, August 6

Friday, 9-9:50 am. Invited Lecture:

"Understanding and Preventing the
Development of Physical Aggression."
Richard Tremblay. **Rooms 10 & 11**

Friday, 10-10:50 am. Invited Lecture: "The
Behavioral Mechanism: How do we get there
from here?" Kurt Salzinger. **Room 29**

Friday 11-11:50 am. Invited Lecture:

"Individual Types: Subcultural or
transcultural? Data from Germany, Japan, &
Russia." Magorah Maruyama. **Room 6**

Friday, 12-1:50 pm. Symposium:

"Experiments in Social Psychology: Science
or Self-deception?" Michael Wertheimer,
Chair. **Room 15**

Participants: Lise Wallach & Michael
Wallach

Discussants:

Christian S. Crandall, David Rosenhan,
Gregory A. Kimble, Brewster Smith,
Robert T. Hogan

Friday, 3-3:50 pm. Symposium: "Growing
where You are Planted: Prospering at small,
isolated universities." Linda Montgomery,
Chair. **Room 37**

Participants:

"Preventing Professional Isolation: Don't
Get Lost in the Outback," Kay E.
Ketzenberger

"Students Successfully Applying to
Graduate School: Establishing a
Tradition," Robert A. Bartsch

"Life Before and After Tenure," Linda M.
Montgomery

Saturday 9-9:50 am. William James Book
Award Address I: "Reliability and
Credibility of Young Children's Reports."
Steven Ceci & Maggie Bruch. **Room 33**

Saturday, 10-10:50 am. George A. Miller
Award Address. "Psychophysics and
Remembering." John Wixted & K.G. White.
Room 33

Saturday, 1-1:50 pm. Ernest Hilgard Award
Address: "On the Joys of Being a Generalist:
Following a boustrophedonical career
path." Philip Zimbardo. **Room 30**

Saturday, 2-2:50 pm. Division 1 Presidential
Address: "The Pleasures and Aggravations
of Babies: Origins and Life-Span
Implications." Lewis Lipsitt. **Room 15**

Saturday, 3-3:50 pm. Business Meeting, &
Award Announcements Lewis P. Lipsitt
Chair. **Room 15**

Sunday, 8-9:50 am. Symposium: "The Hidden
Unity and Problematic of the Social Sciences:
Hedonism." Brent Slife, Chair. **Room 29**

Participants:

"Privileging Hedonism: Confounds and
Consequences," Edwin E. Gantt

"Nonhedonistic Altruism: Does It Truly Exist
Within an Organizational Framework?"

Jeffrey S. Reber

"Limitations in the Psychotherapeutic

Relationship: Psychology's Implicit

Commitment to Hedonism," Justin Calapp

"Naturalism in Psychology: Necessitating
Hedonistic Altruism," Amy Fisher

"Contradiction in Terms: Hedonistic

Altruism," Melinda Petersen & Marissa S.
Beyers

Sunday, 10-11:50 am. Symposium:

"Developmental Assets and Asset-building
Communities: Implications for research, policy,
and practice." Richard Lerner, Chair. **Room 29**

Participant: Peter Benson, Maya Carlson,
Tony Earls, Karen Hein, Raymond P. Lorion,
Jean Rhodes

Discussant: Richard Weinberg

Sunday 12-12:50 pm. Invited Lecture: "What We
Know about Growing a Smart Child." Wendy
Williams. **Rooms 4 & 5**

Sunday, 12-12:50 pm. Multi-Division Symposium:
"Understanding Values in our Work at Home
and Abroad." Steven Handwerker, Chair.

Sunday, 1-2:50 pm. Symposium: "22nd Annual
Symposium on Eminent Women in Psychology:
Historical and personal perspectives." Agnes
O'Connell, Chair. **Room 31**

Participants: Linda Bartoshuk, Jeanne
Brooks-Gunn, Janet S. Hyde, Norine G.
Johnson.

Discussant: Agnes N. O'Connell

Sunday, 3-3:50 pm. The Arthur Staats Award
Address: "Positive Psychology." Martin
Seligman.

Sunday, 4-4:50 pm. Symposium: "Reflections in
the Mirror of Psychology's Past: Honoring John
Popplesstone and Marion W. McPherson."
Thomas Dalton, Chair. **Room 32**

Participants: "Reinventing the Past Through
Reinterpretation," John A. Popplesstone

"New Growth From Phantom Limbs: Tenuous
Attributions to Our Predecessors," Rand
Evans

"Psychological Classics: Historical
Significance, Conceptual Change, and
Myths of Origin," Robert H. Wozniak, PhD

Sunday, 5-5:50 pm. William James Award
Address II: "Genius, Creativity, and Darwin:
Inspiration and exemplar." Dean Simonton.
Rooms 13 & 14

The **GENERAL** *Psychologist*

C o n t e n t s

Rosnow: *Foxes, Hedgehogs, & Ethics*33
Rachlin: *Self-Control* 39
Zimbardo: *Interview* 49
Perloff: *Book Review* 56
Bevan: *Interview* 63
Brucato & Hogan: *More Trivia: Stamps*65
Lipsitt: *Message from the President* 66
***Executive Committee Minutes* 67**
***Division Convention Program* IBC**

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