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The word "Inside" is written in a large, stylized, 3D font with a striped texture. It is enclosed within a thick black rectangular border.

A year ago, TGP went online with a pdf version available on the Society's web page. Members with email addresses were notified and more than 90% of these began to receive their issues in this fashion. This meant that some 60% of the members were able to save the Society the cost of printing, postage and handling the three issues from last year, a savings of several thousand dollars and a big stack of paper. All those who have been participating in this experiment are hereby thanked.

This is the election issue with bios and blurbs from candidates for the office of President-Elect of the Society contained within.



Messages from Society President Overmier

One of the challenges that faces our Society of General Psychology is to know who we are in terms of our specific interest areas as well as our interest in "General Psychology". This led me to lookup the degree to which our members are also members of other divisions of APA and the degree to which other divisions' members are members of Society of General Psychology. I thought the results were of sufficient interest that I want to share them with you. Approximately 10% of our members also belong to each of the Divisions 2 (Teaching), 3 (Experimental), 8 (Personality and Social), 12 (Clinical), 26 (History), 29 (Psychotherapy), and 42 (Independent Practice). Reciprocally, 10% (or more) of the members of Divisions 2 (Teaching), 3 (Experimental), 6 (Behavioral Neuroscience & Comparative), 10 (Arts), 21 (Applied Experimental & Engineering), 24 (Theoretical & Philosophical), 26 (History), 34 (Population & Environmental), and 52 (International) belong to the Society of General Psychology. Clearly, the members of Division 1, the Society of General Psychology, are diverse in focal interests while maintaining their commitment to one psychology as a whole. Should we be surprised by this? No, I think not; consider that we all went to school together and were trained in a common epistemology.

One thing that makes overlap of members especially interesting is that there are many who are always trying to divide psychology and psychologists into "camps". For example, Carol Tavis recently wrote in the *Chronicle of Higher Education* that groups within APA are at war with each other. I think that is false. It is true that those who are in salaried positions—as epitomized by academic researchers—and those who are entrepreneurial—as epitomized by those in independent practice—do want and expect different things from APA, their national organization, in helping them hold onto and advance their psychological activities. So they do have conflicts about how APA should use its resources to advance the public image of psychologists and psychology, to whom these efforts should be directed, and even how training should be construed. But this is different from being at war with each other, and the root is in

economics and not in psychology. Indeed, the war is between psychologists offering psychological services and non-psychologists purporting to offer psychological services, for examples, in treatment of anxiety disorders it is clinical psychologist versus aroma therapists (or others who may or may not be degreed but are not psychologists), or in setting up behavioral screens for potential anxiety drugs it is between experimental psychologists and neuroscientists not trained in behavioral methods. In both cases, the inexpert are intruding upon the domains of the expert—and nobody likes that.

Not only are practitioner and experimental psychologists not in different camps, but they complement each other. NIH funds university researchers primarily in hopes of eventually finding new principles for developing better diagnoses and treatments. The practicing clinician finds phenomena that need principled understanding and pose challenges to research; for example, they may find that some unusual treatment works (and can in fact be empirically validated such as EMDR) but the principle remains elusive and awaits experimental discovery (probably 'exposure' as I read the current literature: e.g., Shepherd, Stein, Milne, 2000, *Psychol Med*, 30, 863-871). It is this interplay that has advanced our field in clinical practice, in human factors, in testing and evaluation, in education, etc., and was foundational in the commitment to the Boulder model of training. Commitment to that model may be waning and perhaps for good reasons; I cannot be certain, and, thus, I retain my commitment to the Boulder model. My recently deceased colleague, Paul Meehl, was the perfect example the Boulder-type psychologist and the mutual enrichment and real gains for psychology as a whole that arise from the interplay among real world phenomena, experimental data, and theory. Would that I could emulate him more closely.

One very difficult (indeed "taboo") area of disagreement that is important has to do with persons trained in our Psychology Departments who terminate their training with a Master's degree. While APA struggles with maintaining its membership,

there are some 270,000 MA persons from Psychology Departments that are employed in the USA (federal data). But these Masters level (MA) persons are not afforded full membership in APA, nor encouraged to join, nor served well— while at the same time APA now solicits college and even high school students as affiliates. This seems strange to me! The reasons behind this are those economic ones I have mentioned above. Many worry that Masters level psychologists compete with the PhD level psychologists in the providing of services. To reduce the competition, CAPP proposed and APA has long endorsed efforts to prevent Master level persons from (1) becoming full members of APA and (2) from being licensed as “*psychologists*”. This is called the protection-of-title. Oddly, APA has not pursued the protection of scope-of-practice, which one would presume could be reasonably divined from the differences in training and levels of expertise. [Scope-of-practice protection is what physicians have, and it is this they use to differentiate themselves from physician assistants, among others.] I certainly agree and support differentiating practices of PhDs and MAs, and I accept the PhD level as that required for full independent practice. However, somehow, membership in APA and licensing have become conflated issues. They need not be, and indeed there are advantages to all in separating these issues. While some of APA’s members may see advantages to current policy, others—primarily the academic and science members—perceive disadvantages. While the federal tables are organized in less than perfect ways, it appears that about 50% of the MA psychologists (and perhaps two thirds—depending on how one reads the tables) are not involved in providing clinical services, but rather employed in management, industry, R&D, consulting, educational institutions, and government. And, these are valued colleagues in using psychology to solve real world problems. The real threat to psychology and its quality practice is from *other* professions (e.g., Social Work, among others) whose members purport to offer services (at the MA or even lower levels!) that can substitute for those provided from a psychologists’ skill and knowledge. And, *this* is what we should strive to prevent. Can we—trained and degreed in psychology— not become one family to work together to defend **PSYCHOLOGY** (written large) and all it has to offer across its full spectrum to increase human success and functionality, health, mental health, education, happiness, and general welfare. Together, respecting our differences, we could be stronger and do more for *all*.

Dear Colleagues in Division 1.

Alan Boneau has informed me that he wishes after many years of service to the Division to give up his responsibilities and pass them on to another. Alan is the Editor of our division newsletter, *THE GENERAL PSYCHOLOGIST*, which appears 3 times per year carrying to our members news of the division and its activities, news of the division’s annual program, and—to my reading—some important, insightful, and fun papers by members (and sometimes about them—if you read the interview with Corsini). I have saved some of the individual papers for my reference for years; I found Greg Kimble’s discussions of general psychology especially insightful. This newsletter is an important part of the division’s services to members and key to retaining our members. It is also our division’s voice to carry messages about the state of psychology. This job is both a responsibility and an opportunity.

We need to identify from among our members, candidates for the editorship of *THE GENERAL PSYCHOLOGIST*. We want someone whose vision and enthusiasm for general psychology can be a lens for focusing on our activities and for selecting (and soliciting) papers to appear in *THE GENERAL PSYCHOLOGIST*. Nominations are now in order (including self nominations).

Please take this call very seriously and give some thought to who might serve as the new editor. If you have a person in mind, you might also give them a call to assess their willingness.

Ideally, Al would like to have the person on board so that they could work together on the second issue of this year, with transfer by the final issue. But other transition phasing may be possible if we beg a bit.

Thank you for your help on this.

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CAN PSYCHOLOGICAL RESEARCH SURVIVE THE IRBs?

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Every American university now maintains Human Subjects Committees (called, for obscure reasons, "Institutional Review Boards" or IRBs) that must give prior approval before any faculty research involving human subjects can proceed. This practice began in response to abuses in which medical researchers put experimental subjects at risk without first obtaining their free and informed consent. I remember as a graduate student witnessing a brain surgery in which, for reasons unrelated to the patient's epilepsy, an electrode was inserted three times deep into an unaffected brain region, each insertion doubtless destroying many brain cells and their interconnections. A neurologist and the neurosurgeon were doing a bit of informal experimenting, as physicians had done since Galen's time, usually with no lasting harm to their unsuspecting patients. But the practice plainly was wrong and the IRBs have put an end to most of it. They are also threatening to put an end to much harmless and useful psychological research as well. Indeed, much of the work reviewed in my *The Antisocial Personalities* (1995) could not have been done under the mindless, pettifogging scrutiny of an IRB, at least not the one that I was dealing with fairly recently.

I wanted to ask inmates at Oak Park Heights maximum-security prison to fill out the same personality inventory (Tellegen's MPQ) that we had administered to thousands of twins of all ages. I was particularly interested in testing my prediction that these inmates, once they have had six months or so to adjust to prison routine, are probably as happy as they were before their last arrest, due to the remarkable adaptability of *Homo sapiens*. The warden and the State Corrections people had approved the plan and the prison psychologist had volunteered to collaborate. It was agreed that neither the individual scores nor who had or had not been willing to participate would become part of any inmate's record. As the only incentive to participation, I would provide each volunteer with

a computer-generated report of how his scores compared with those of men in general, the same sort of feedback that we have always used with twins and other research subjects. It was a simple, innocent, inexpensive little project that seemed to me worth doing.

But my Human Subjects Committee were scandalized! Prison inmates are *vulnerable* and must be handled gently if at all. I will mention only one of the loony "stipulations" demanded by the IRB before this project would be allowed to proceed. I was informed by this committee of faculty colleagues, professors of English, Architecture, Law, etc., and chaired by a professor of Occupational Therapy, that it would put these men "at risk" to inform them of their scores "outside the context of individual counseling." That is, the only way I could provide my inmate volunteers with the promised incentive feedback would be to hire a licensed "counselor" to meet with each felon privately to explain his scores, to answer any questions, and to ensure that learning he had scored higher on Aggression than 76% of men in general did not cause him lasting psychological injury. The consequence of this fatuity, of course, was that the project was stillborn.

We have known for some time that long prison sentences do not act as a deterrent. (I must exclude from that "we" those legislators who enacted bills requiring long sentences for drug dealers, the only effect of which upon the citizenry has been the forced early-release of dangerous criminals to make room for the dealers who are at once replaced on the street.) Most of us have also known that prisons do not function as "reformatories." Reasonable people would have been interested to learn that imprisonment beyond a year or so in a well-run institution does not actually punish the offender either—his human adaptability brings his subjective well-being back to his genetic set-point within about six months—and that the only func-

tion imprisonment does serve is to keep dangerous criminals off the street.

Even those professors on the IRB would have found this interesting were it presented to them in a different context. They are all intelligent, highly-educated, well-intentioned people yet — given the responsibility, and the authority, to regulate an activity, which most of them know nothing about, in a bureaucratic system where they are accountable (to higher bureaucrats) for their errors of omission, but not accountable at all to the persons regulated for their errors of commission — they predictably behave in a mindless, arbitrary, bureaucratic way. If I were a younger man and realized what an encumbrance and real threat this was to my chosen profession and to me personally, I would feel obliged to take up arms on this issue. I would organize other victimized researchers, demand audience with higher authority, and point out some of the easy and obvious ways to remediate the situation. For example, we should require every IRB to be composed of faculty at least half of whom actually do research with human subjects and it would be particularly helpful if each proposal had to be discussed *ab initio* in the presence of the interested investigator.

Getting back to Oak Park Heights prison, Kenneth Carlson, the prison psychologist, proceeded on his own to collect MPQs from 67 men whose mean age was 32 years. The median expected release date for these inmates is the year 2030 and the average length of time already served was 37 months. We had previously collected MPQs also from more than 850 male twins aged 30-33 and we used them as a normal control group. The men in our inmate sample had been convicted of serious crimes, 31 of them for murder. Because the MPQ is a self-administered inventory and requires high school reading skills, a considerable proportion of the inmate population could not be sampled but there is no reason to think that the participants differed temperamentally from the nonreaders.

Anyone familiar with the realities of prison life knows that some inmates are predictably violent and dangerous while some are predictably passive or tractable. Figure 1 shows the profiles of the 22 inmates scoring highest and the 22 scoring lowest on the Aggression scale of the MPQ, plotted using the data from the noncriminal male twins as norms. The most aggressive inmates are deviant also on most of the other MPQ scales. They are more than one SD *below* the normal mean on Well Being, Achievement, and Social Closeness, the traits that, with Social Potency, comprise the Posi-

tive Emotionality super-factor of the MPQ. The aggressive inmates are more than one SD *above* the mean on Stress Reaction (neuroticism) and on Alienation, which, with Aggression, comprise the Negative

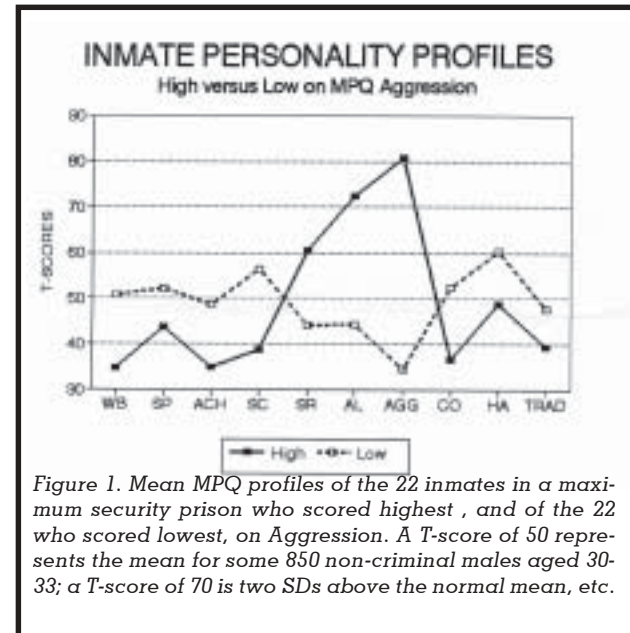


Figure 1. Mean MPQ profiles of the 22 inmates in a maximum security prison who scored highest, and of the 22 who scored lowest, on Aggression. A T-score of 50 represents the mean for some 850 non-criminal males aged 30-33; a T-score of 70 is two SDs above the normal mean, etc.

Emotionality super-factor. And they are more than one SD *below* the normal mean on Control (vs. impulsiveness), and on Traditionalism, two of the traits that comprise the Constraint super-factor.

The non-aggressive inmates, on the other hand, yield essentially normal profiles except for that low score on Aggression and an elevation on Harm Avoidance (fearfulness). In spite of their confinement in the same prison environment, these men show great variability, one from another, not only in personality but also in their tendencies to make or to stay out of trouble in that environment.

Modern Prisons are not "Places of Unremitting Pain"

More than 25 years ago, Haney, Banks, and Zimbardo (1973) transformed the basement hallway of Stanford University's Psychology Department into a make-believe prison block where a group of male student volunteers posed either as inmates or as guards. Some of the "guards" behaved badly and some of the students "begged to be released from the intense pains of less than a week of merely simulated imprisonment" (Haney & Zimbardo, 1998, pp.709.) The experiment was therefore aborted after just six days and nights. Apparently many who read about the Stanford Prison Experiment (SPE), as this six day venture

came to be called, agreed with the authors that it had demonstrated "the way in which social contexts can influence, alter, shape, and transform human behavior" (pp.709-10). Based on studies of this kind, some of them Gedanken experiments as in the following quotation from Mischel's influential textbook, many psychologists came to believe that social learning determines personality and that social context determines behavior.

"Imagine the enormous differences that would be found in the personalities of twins with identical genetic endowment if they were raised apart in two different families.... Through social learning vast differences develop among people in their reactions to most stimuli they face in daily life." (Mischel, 1981, p. 311.)

Mischel's experiment has been actually conducted by Bouchard (1990) and others with results opposite to his and other social-learning theorists' predictions. In their recent article, however, Haney and Zimbardo (1998; Zimbardo is the current president of APA!) reviewed what they consider important lessons learned from their SPE. These "lessons" include the proposition that behavior is determined by the situational context rather than by characteristics of the behaving individuals. Because the six day SPE "had painful, even traumatic consequences for the prisoners [Stanford students pretending to be inmates] against whom it was directed" (p. 719), Haney and Zimbardo concluded that real prisons must have devastating psychological effects upon real inmates serving long sentences. Perhaps because they are situationists, rather than trait psychologists, they neglected our extraordinary human capacity to adapt to circumstances, good or bad.

Suh, Diener, & Fujita (1996) have shown that both positive and negative life experiences have usually lost their effect on subjective well being after six months. A year after either winning the lottery or being permanently crippled in an accident, most people experience about the same average level of happiness that they felt before that event. In a study I did long ago in another Minnesota prison (Lykken, 1957), one inmate, the pitcher on the prison baseball team, had been paroled the previous fall. He made it back in time for the spring baseball season by the expedient of breaking the display window of a jewelry store and then leisurely collecting rings and watches until arrested on the spot. He admitted he was happier back in prison than he'd been on the outside.

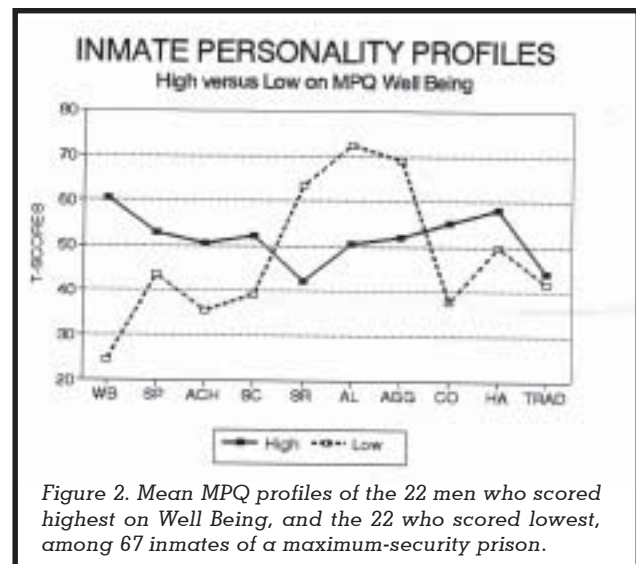


Figure 2. Mean MPQ profiles of the 22 men who scored highest on Well Being, and the 22 who scored lowest, among 67 inmates of a maximum-security prison.

The mean expected release date for our sample of Oak Park Heights inmates was thirty years hence yet, after having been there for an average period of three years, many of them appear to have become well-adjusted to prison life and many are surprisingly happy. Figure 2 shows the mean profiles for the third scoring highest, and the third scoring lowest, on MPQ Well Being. While the lowest-scoring third professed considerable pain and alienation, the upper-third scored higher on Well Being than three-fourths of our 850 noncriminal young men. Oak Park Heights is a modern prison, well run and reasonably safe because the staff rather than the inmates, are in control. The well-adjusted inmates can take classes, learn skills, find peaceful ways to pass the time. I would not wish to be incarcerated at Oak Park Heights, not even if I was made pitcher of the baseball team, but at least I could get a lot of reading done.

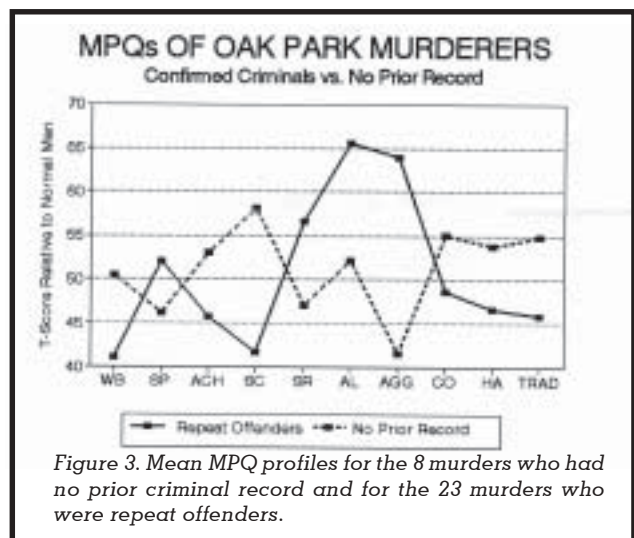


Figure 3. Mean MPQ profiles for the 8 murderers who had no prior criminal record and for the 23 murderers who were repeat offenders.

One more interesting bit: there were 31 murderers among the 67 inmates and 8 of them had no prior criminal record while 23 were repeat offenders. The mean MPQ profiles for these two subgroups, shown in Figure 3, indicate that even murderers are heterogeneous in their temperament and personality. The murderers who had prior criminal records looked quite like the more aggressive inmates shown in Figure 1. The murderers who were first offenders, in contrast, look quite tractable in personality. They are *above* average in Social Closeness, well *below* the mean for noncriminal males on Aggression, and *above* average in Control, Harm Avoidance, and Traditionalism. A parole board would be justified in predicting that men like these could be released with reasonable confidence.

I think that these are interesting and significant findings that would not have been obtainable if our IRB had had their way.

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Simplicity and Complexity in Human Concept Learning

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It's a pleasure to be here today to speak with you, and I'd like to extend my gratitude to Drs. Lyle Bourne and Linda Bartoshuk for making it possible.

The topic I'd like to talk to you about today is, I think, one of the oldest and most basic in cognition: how we learn from examples. As most famously pointed out by Hume, when we make a finite number of observations of an enduring phenomenon, there is no strictly logical (i.e., deductive) basis for forming any firm generalizations about it. Instead we must "induce," that is, make educated guesses about what its general properties might be. The need for this is especially clear in the case of category learning, or as it is sometimes called, concept formation. We see a few examples of a category—say, a straight-backed chair, a plush armchair, and three-legged stool—and must guess the true form of the category (*chairs*).

Categories differ widely, of course, in the ease with which people can learn them from examples. Some categories—*chairs*, say—are easily guessed from few examples. At the other extreme, extremely disjoint categories—say, the set including a hat, a piano, the sun, and the King of Sweden—are so incoherent and seemingly irregular that it seems *no* finite subset would suffice to communicate the essence of the category; such categories are consequently very difficult to learn from examples. The contents of such a category can only be effectively communicated, it seems, by simply *listing* its contents verbatim: no regularities or common trends hold sway. This idea—that a psychologically incoherent (and thus unlearnable) category is one that cannot be compressed or summarized—will be important later.

This spectrum of subjective complexity is of profound importance in understanding how we learn, because it reflects the underlying mechanisms of induction: what makes some inductive hypotheses—potential concepts—more attractive to the

human learner than others. When we have to wrap a concept around the few examples actually seen, we of course prefer the most *natural* concept available. But what exactly is a psychologically "natural" concept?

This question was extensively studied during the 1960s (a period that stretched from about 1953 to about 1973). During this period a great many experiments were conducted concerning the learning of artificial concepts, usually defined by some logical combination of two binary (Boolean) variables. These studies produced some extremely beautiful

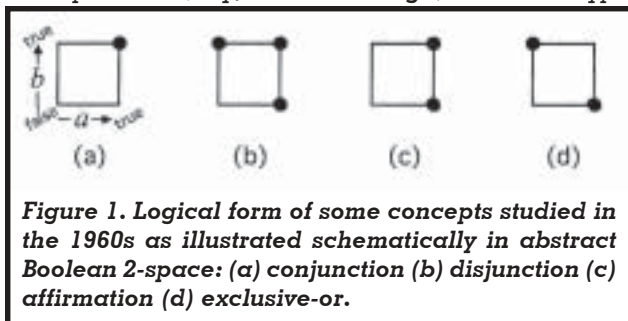
Jacob Feldman is the 2002 winner of the George Miller Award of the Society for General Psychology. This article is based upon his Award Address in Chicago, August 2002

and stable results, epitomized by the comprehensive studies by Lyle Bourne (summarized in his 1966 book, *Human Conceptual Behavior*). The general thrust of these studies was that the logical form of the rule defining a category can have a decisive effect on the difficulty subjects have in learning it. The most notorious specific conclusion, around which literally thousands of studies revolved, was that *conjunctive* ("and") concepts were easier to learn—subjectively simpler—than *disjunctive* ("or") concepts.

Indulging in a bit of retrospective psychoanalysis, it seems that some of the huge amount of interest in this issue derived from the apparent divergence it suggested between *logical* complexity, on the one hand *psychological* complexity, on the other. Conjunction ($\alpha \wedge b$ in mathematical notation, in which the variables α and b refer to features or properties of the objects) and disjunction ($\alpha \vee b$) are of equal complexity by almost any conceivable mathematical definition. Yet the subjective, psychological dif-

ference between them was stable and reliable. This contrast seems to suggest some special, mysterious, and quintessentially **human** bias in favor of disjunctive concepts—an enticing prospect to students of human learning.

In order to fully appreciate the variations in logical form at play here, it is convenient to depict Boolean concepts visually as patterns in a grid of spatial dimensions (Fig. 1). Almost all studies in the 1960s involved bivariate concepts, which we can depict in a two-dimensional square, called Boolean 2-space (top row). Here the horizontal (a) and vertical (b) axes each refer to a particular Boolean feature, for example size (say, **small** or **large**) and fruit type



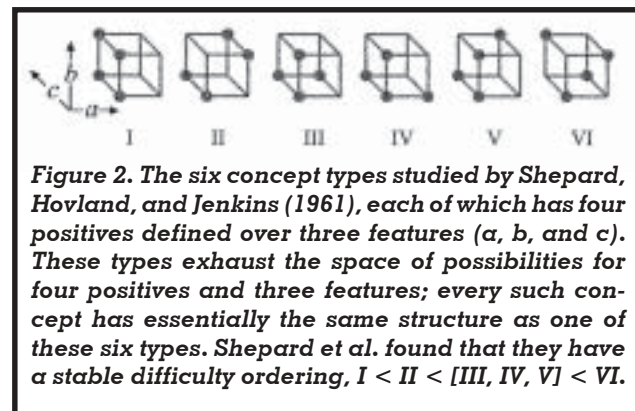
(say, **apple** or **orange**). The two values of each variable are conventionally referred to as **true** and **false**, though of course these labels are arbitrary and meaningless with certain features (e.g. **apple** vs. **orange**—neither one has any special claim to the role of “true” along the dimension **shape**). The four vertices of the grid thus each refer to a single uniquely-defined object: small apple, large apple, small orange, large orange.

In diagrams like these, a **concept** corresponds to a particular subset of the four vertices, which we depict by heavy dots at the “positive” corners. In this system, conjunctive concepts have one corner positive (Fig. 1a), and disjunctive concepts have three (Fig. 1b). Considering these figures, one immediately sees that there are number of other structural possibilities. One is to have two positives on one side of the square (Fig. 1c); such a concept is called **affirmation** (or **negation**) in the literature, because one of the two features is always true (or false, as the case may be). Another is to have two positives at opposite corners (called **exclusive-or** or **biconditional**; Fig. 1d). All of these varieties were systematically studied by Bourne and others in the 1960s; they differ in difficulty in a very reliable order (usually given as affirmation/negation < conjunction < disjunction < exclusive-or/biconditional). A number of researchers, notably Bourne (1974), offered satisfying quantitative accounts of this difficulty ordering.

However, there was one outstanding result that did

not fit into this clean bivariate hierarchy: a 1961 study by Shepard, Hovland, and Jenkins, then virtually the only extant research to have considered concepts involving more than two variables. Shepard et al. (1961) considered 3-variable concepts having exactly four positives and four negatives. I’ll refer to this as the case with $D = 3$ and $P = 4$, with D meaning the number of features (dimensions) and P meaning the number of positive examples. Concepts with $D = 3$ and $P = 4$, it turns out, come in exactly six distinct types (Fig. 2), designated types I – VI by Shepard et al. Mathematically, this typology is complete: no matter how you arrange four vertices in Boolean 3-space, you get something that has essentially the same structure as one of Shepard et al.’s six.

It is important in what follows to understand exactly



what is meant here by “essentially the same structure.” As mentioned before, typically, the features here (a , b , and c) don’t have an intrinsic “sign,” meaning we could just as well interchange the **true** and **false** labels. Similarly, since we are abstracting over the actual meanings of the variables themselves, we don’t really care which of a , b or c is pointing which way in the figures. These choices are really all arbitrary, and so changing them doesn’t really change the nature of the concept we are talking about. What this means visually is that we are free to **rotate** the diagrams rigidly through space (Fig. 3). As long as we don’t change a concept’s internal structure haven’t changed its essential logical form.

This sort of equivalence is called an **isomorphism** (technically, this particular equivalence is **isomorphism under permutation of feature labels and exchange of sign**). Critically, equivalence under isomorphism carves up the space of possible concepts: we can sort concepts into bins depending on their essential structure, so that all concepts within a bin are essentially the same, but all the bins are essentially different from each other. Shepard et