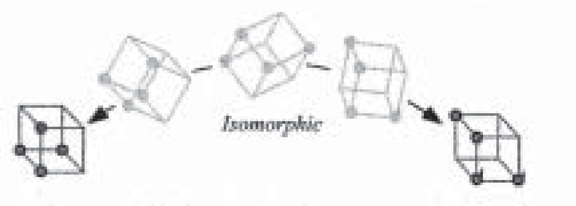

Figure 3. Two Boolean concepts are isomorphic—of the same logical type—if they are equivalent after interchanging the labels and signs of the features, which can be pictured visually as a rigid rotation throughBoolean space.

al.'s six types are one such typology—the one appropriate for three featural dimensions and four positives. Other numbers of dimensions or positives lead to other typologies (some of which I'll get to in a moment). From the point of view of a psychologist interested in the effect of logical form on learning, these typologies give a crucial road map: they tell you exactly what forms to study—namely, the bins, or equivalence classes. Once you understand how each of these bins is treated psychologically, you understand everything that depends on logical form alone.

Note that this notion of equivalence, and the resulting typology, applies to the bivariate case as well, as was recognized in the 1960s. For example, affirmation and negation, though they are different in the details, are really the same ***type*** of concept, once we decide that we don't care about which value is labeled positive and which "negative"—both are bivariate concepts in which membership depends only on the value of one variable. Similarly there is really only one kind of concept with one positive, namely conjunction—because all four vertices of the Boolean square are equivalent if we can rotate the square freely. Finally, there is really only one kind of concept with two positives on opposite corners, whether it is labeled either biconditional (when the corners are oriented northeast and southwest) or exclusive-or (northwest and southeast). In terms of pure logical form, these three types are really all there is.

Now let's get back to the psychology. In their study of the three-features four-positives case, Shepard et al. found that the six essential types exhibited a very reliable difficulty ordering: I < II < [III, IV, V] < VI (with a tie among III, IV and V). This result has been replicated a number of times since, and has come to be viewed as a standard benchmark in the field. Notice that there is no simple way to explain it in terms of a preference for conjunctive concepts, or any of the other theoretical constructs proposed in the 1960s. More recently, a number of learning models (mostly exemplar models and connectionist networks) have successfully modeled it, but only in terms of the asymptotic behavior of a large and complex simulation with many parameters. There has never been any simple, theoretical account of the ordering: no way of deriving it from first principles of learning.

Almost lost amid the torrent of learning papers in the 1960s were two that proposed a different principle: ***simplicity.*** Neisser and Weene (1962) and, separately, Haygood (1963) had proposed that the known difficulty ordering of bivariate concepts could be explained in terms of the length of the logical formulas required to express them. For example, exclusive-or requires more symbols to express than conjunction *(ab+a'b' vs. ab),* and this might be part of why it is more difficult to learn. (Here and from now on I'm adopting the standard mathematician's notation in which *ab* means *a* and *b*, *a* + *b* means *a* or *b*, and *a'* means not-a.) One big problem with this explanation, though, is that it fails to explain by far the most famous case: conjunction *(ab)* vs. disjunction *(a + b),* which require the same number of symbols (counting only variable names, not the + or' symbols, which are operators).

But the idea that simplicity was the overarching principle at work did not gain any adherents. Part of the reason, perhaps, is that to fully work out this idea required a mathematical notion of simplicity that had not yet, at that time, been developed. But at about the same time (around 1962) three mathematicians (Kolmogorov, Chaitin, and Solomonoff) independently developed the required notion. Their idea, now usually referred to as Kolmogorov complexity, is that the complexity of a symbol string is the length of the ***shortest*** description that is required to faithfully express it. Simple strings can be very compactly described. Complex strings require longer descriptions.

In the limit, if a string is so complex that it can't be compressed at all, one can simply ***quote*** it verbatim—list its members—as a way of expressing it. By this way of measuring it, then, complexity is intrinsically capped at about the length of the original object: if all else fails—i.e. with maximally complex strings—one can always enumerate their contents, which automatically takes about the same number of symbols as were in the original string itself. Kolmogorov complexity also has certain very desirable mathematical properties, chiefly that it is "universal:" in a certain well-defined sense the complexity of a string is independent from the details of the language in which you choose to express it.

In the realm of Boolean concepts, the natural analog of Kolmogorov complexity is what's called the ***Boolean complexity:*** the length of the shortest logical expression that is equivalent to the set of positive examples—called the ***minimal formula.*** The more you can reduce the logical expression of your concept, while still faithfully expressing it, the simpler the concept is. Normally we measure the length of the minimal formula by counting variable symbols only, not operators. For example, the concept ***big apple or small apple*** (expressed symbolically as $ab + ab'$, with a = ***apple*** and b = ***big***) is logically equivalent to ***apple*** (i.e., $ab + ab'$ reduces algebraically to a); it has Boolean complexity 1. Conversely the concept ***big apple or small orange*** ($ab + a'b'$) can't be similarly reduced—no shorter formula is equivalent to it—so it has Boolean complexity 4. The same reduction trick can be applied to any Boolean concept, of any length, to give an estimate of its intrinsic complexity.

So how does Boolean complexity match up to the subjective difficulty of concepts—that is, to subjective complexity? The touchstone is Shepard et al.'s six types, whose difficulty ordering, you'll recall, was a bit tricky to explain. The Boolean complexities of the six types come out as 1, 4, 6, 6, 6, and 10 respectively (see Fig. 4)—perfectly agreeing with the famous difficulty ordering I < II < [III, IV, V] < VI. The exact minimal formulae for each of the six types are given in the figure (lower panel).

This agreement gives a good prima facie boost to the theory that Boolean complexity dictates subjective complexity. A more comprehensive test, though, would require trying new cases. Bivariate cases were exhaustively studied in the 1960s, and Shepard et al. completely covered the $D = 3$, $P = 4$ case. What about other values of D and P? These had never been tested. The aim of my study (Feldman, 2000) was to do so.

The first step is to work out the typologies for other values of D and P. For each value of D and P, the typology completely changes, producing a different "family" of basic concept types, referred to as the $D[P]$ family. The sizes of these families as you change D and P vary in a somewhat unpredictable way (for example, 4[4] has 19 types—who would have guessed?)—though the correct combinatoric

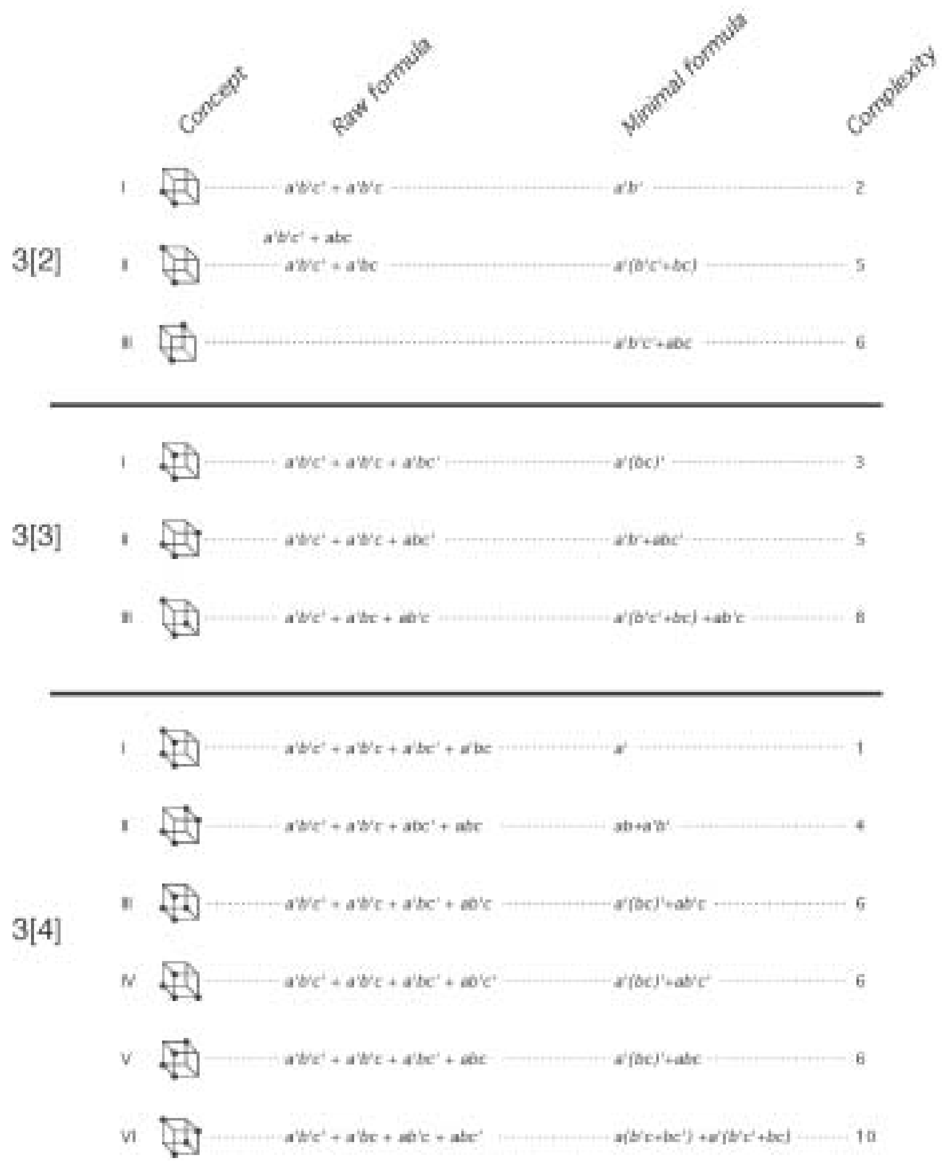

Figure 4. Concepts from three of the families tested (3[2], 3[3], and 3[4]), showing the "raw" (uncompressed) formula, minimal formula, and Boolean complexity (length of the minimal formula in literals).

theory was worked out as early as 1951 (by Aiken and his staff at the Harvard computation laboratory; see Feldman, in press). When we need to refer to particular types within each family, we designate them with Roman numerals subscripted with the family name, like $I_{3[4]}$, $II_{3[4]}$, etc., for Shepard et al.'s family.

For my study, I wanted to be as comprehensive as possible, but above 4[4] there are simply too many types in each family to test conveniently; in the end I used families 3[2], 3[3], 3[4], 4[2], 4[3], and 4[4]. This means that, up to isomorphism, the study considered ***every*** Boolean concept with three or four features and up to four positives. Fig. 4 shows the 3[2], 3[3], and 3[4] families, along with both the raw and minimal formula associated with each concept in each family. As you can see in the figure, just as with Shepard et al.'s six types (the 3[4] family), each of the 3[2]-family concepts is distinct from each of the other ones—they can't be rotated through 3-space to become equivalent; and likewise for each of the 3[3] concepts. (Of course, concepts from different families are never isomorphic to each other either, since they have different numbers of dimensions or positive vertices.) The 3[2] and 3[3] families each have three concepts, none of which as far as I know had every been tested psychologically before.

There is one additional complication before the study can be completely laid out. With Shepard et al.'s, study there were four positives drawn from the eight objects, and thus also four negatives. But most of the families I was interested in have ***different*** numbers of positives and negatives; for example 3[2] concepts each have two positives and six negatives. Each such concept has a "mirror image" concept in which the labels are swapped, so that the positives become negatives and the negatives positives. I'll refer to the "orientation" of each concept in this sense as its ***parity,*** designating the version with the smaller half labeled positive as the ***Up*** version and the mirror image as the ***Down*** version. What is interesting about this is that, from a logical point of view, Up and Down versions of a concept are essentially the same concept—specifically, the Down version corresponds to the same formula as the Up version with an extra "not" sign placed in front of it. Operator symbols don't count towards the complexity, so Up and Down versions of the same concept ***always*** have the same complexity. They differ only in this parity, which is a new variable orthogonal to complexity.

What is the psychological significance of a concept's parity? Will Up and Down versions of concepts be treated identically by subjects? In the experiments, it is very straightforward to test this: we simply include every concept in both Up and Down versions, and treat the parity factor as an independent manipulation fully crossed with Boolean complexity. (Actually, they aren't quite crossed, because 3[4] concepts only come in one version, because they have equal numbers of positives and negatives.) For simplicity of notation, from here on I will use P to mean the number of examples in the "smaller half" of the concept, which by definition is positive in the Up version and negative in the Down version.

In testing all these concepts, I was primarily interested in the ease subjects had in learning their members. So the procedure was simply to show the subject the entire space of objects (8 for $D = 3$ cases, *16* for $D = 4$), separated into positive and negative groups, with the positive group labeled "Examples" and the negative "Not examples" (see Fig. 5). First, the subject would study these for a fixed period of time (a few seconds, the exact period depending the case). Then, the subject was tested on all the objects in random order. We can then look at their performance (percent correct) as a function of the structure of the concept—specifically, of its Boolean complexity and its parity. Each subject saw concepts from only one *D[P]* family, including all concepts from the family in both parities. Thus the manipulations of complexity and parity were both within-subjects, while family was between-subjects.

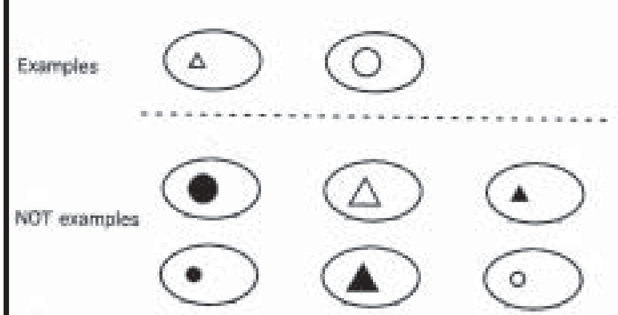

Figure 5. A simple "learning" screen as viewed by subjects. This screen shows a concewpt of type $II_{3[2]}$.

The results are summarized in Fig. 6, which collapses over all the families to highlight the effects of Boolean complexity and parity. Both factors plainly and systematically influence performance. As complexity increases, performance worsens steadily; more complex concepts are more difficult to learn than less complex cases, with a roughly constant advantage for Up cases over Down cases.

The main result—the complexity affect—suggests that human learners are doing something like ***minimization*** or ***compression*** when they represent concepts. As the subject studies the set of examples, he or she seeks to encode them in as compact a manner as possible. The more effectively the examples can be compressed—the lower the Boolean complexity—the more successful this strategy will be, and the more effectively the examples will be retained.

This result is especially intriguing because it ties human concept learning to a very famous and ubiquitous principle—simplicity: the idea that observers ought to favor simple hypotheses. In the philosophy of science literature this is known as Occam's razor, but the same idea turns up in a multitude of settings in other fields concerned with inference from examples—including machine learning and inferential statistics, where it is often called the Minimum Description Length principle, a term introduced by Rissanen (1978). This principle has been growing in influence during the past decade, where it lies at the heart of many of the most sophisticated and successful automated inference systems. In perception, the same idea is familiar in the guise of the Minimum principle, or in the Gestaltists' term ***Prägnanz***. All these principles point in the same direction: observers profit by drawing the simplest interpretation available of what they observe. Surprisingly, this idea had never really penetrated the field of human concept learning (except for Neisser, Weene, and Haygood's doomed hypothesis) despite this seeming like a pretty apt place to apply it. But the data in Fig. 6 suggest that human category learners, too, obey a minimization principle.

The other main result, the parity effect, suggests that subjects have some kind of complexity-independent preference for looking at concepts through their positive examples. Indeed others had noticed the same

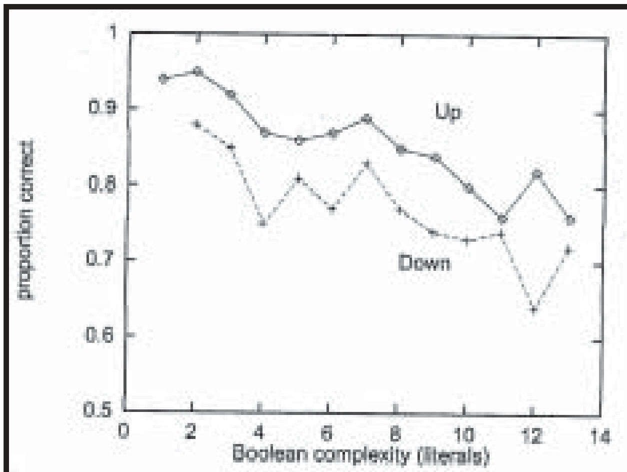

Figure 6. Results of the experiments. As Boolean complexity increases, performance steadily declines, with a roughly constant advantage for Up parity cases over Down parity cases.

tendency as early as 1953 (e.g., Hovland & Weiss, 1953); so this is not really news. The novelty here is to see this as a factor orthogonal to complexity, because seeing it this way changes the way you see older results—specifically the old conjunction/disjunction dichotomy.

Recall the typology of bivariate logical I discussed before. The way it ran, there turned out to be only three essential types: affirmation, conjunction, and exclusive-or. So where does ***disjunction*** fit into this scheme? The answer is that disjunction is the same type as conjunction, except with opposite parity: disjunction is conjunction "upside-down." A conjunctive concept such as *ab* has one positive example (ab); its complement, in this case the concept *(ab),* has three positive examples (*ab'*, *a'b*, and *a'b'*), and can be rewritten as *a' + b'*—a disjunctive concept. Thus conjunction is the Up parity version of this basic type and disjunction is the Down parity version. The point is that the famous superiority of conjunctive over disjunctive concepts was just a reflection of the parity effect—Up parity cases are learned more easily then Down parity cases—and had nothing to do with complexity. But when a wider range of concept types is tested, as in this experiment, we see that there is a substantial complexity effect, even though it doesn't show up in the conjunction/disjunction comparison. Contrary to how it must have looked focusing on only that comparison, a lot of the variance in conceptual difficulty is driven by differences in complexity.

I think the idea of complexity minimization also sheds some light on a more recent controversy in the field of concept learning: the distinction between the encoding of rules vs. the storage of exemplars. Many recent concept learning theories have revolved around the explicit storage of specific examples: in such theories, new objects are evaluated by comparing them with stored exemplars. By contrast, many areas of learning, such as the acquisition of language, quite obviously involve the extraction of rules from what is heard (i.e. descriptive grammatical rules, such as how to form the past tense, what order to place adjectives in, which part of a sentence receives tense, etc.). Recently several theories have been proposed that mix these two strategies for learning: one component for extracting rules, and another component for storing examples that don't fit into the rule scheme (exceptions).

But the distinction between the "rule" and the "exceptions" gets a little hazy when one thinks about minimal formulae. Some concepts, like Shepard et al.'s type I (see Fig. 2), reduce to one very simple rule that covers all objects. Others, like type 3 of family *3[2]* (see Fig. 5), are completely incompressible, which means their minimal formulae consist essentially of a verbatim list of their members. But in between these extremes are some concepts whose minimal formulae have a component (literally, a disjunct) that covers most of the objects, plus one or more additional objects (again, more disjuncts) that ***aren't*** covered by the "main rule." An example is type $VII_{4[4]}$, whose uncompressed rendition is

$$a'b'c'd' + a'b'c'd + a'b'cd' + abcd,$$

which compresses to

$$a'b'(cd)' + abcd.$$

The first disjunct ($a'b'(cd)'$) is the "rule," and the second disjunct *(abcd)*, a single object, is the "exception." But the exception is also in a sense *part* of the rule—part of the minimal formula.

In a sense, this makes the dichotomy between rules and exceptions a bit fuzzy. But alternately one can view this situation as clarifying the distinction between rules and exceptions, by showing exactly what the exceptions are exceptions to. When some objects need to be explicitly listed as part of the most compact rendition of the category, then you know in a deep sense that they are really exceptional. Compression points the way to a more rigorous basis for the distinction between rule and exception.

I'd like to end with a very superficial reflection about the underlying reasons for complexity minimization in concept learning. The principle of simplicity is so familiar that one hardly stops to wonder why it makes any sense. But why should we try to reduce a set of observed examples to a minimal form? What advantage does this afford us? Is it just the saving in storage space? To me, this saving seems a bit trivial in the context of a brain with 10^{11} neurons.

I'd suggest—echoing an enormous amount of technical advances in statistics and machine learning—that minimization of complexity subserves the more basic goal of ***extraction of regularities***. Our deepest cognitive impulse is to understand the world. And in a profound mathematical sense, compressing our description of it helps to accomplish this. How? Because all compression schemes depend on finding and benefiting from regular tendencies in the data—places where the data is a bit redundant or repetitive or orderly. This is how a photograph with large uniform areas or repetitive textures can result in a very small file on your hard drive. (The image compression scheme known as JPEG—like all compression schemes—is based around a clever and systematic use of this idea.) In file-format compression schemes, an understanding of the form of regularities in the data is leveraged into a method for reducing file sizes. The flip side of this idea is that compressing data can be leveraged into an understanding of what regularities the data obeys—it is how regularities implicit in the data become ***explicit***.

In the realm of concepts defined over Boolean features, this idea is particularly transparent. Take the concept *ab* + *ab'* discussed before (e.g., ***big apple or small apple***). Compressing this algebraically to ***a (apple)*** makes explicit that all objects in this concept are apples—a regularity of this small world. By compressing the formula, the observer has discovered a grain of truth about this world. Ideally, one would like a theory of category formation that was organized around this principle, in order to fully understand how categorization relates to inference. In other more recent work, I have attempted to develop this idea into a more thoroughgoing "concept algebra," modeling in more detail how human observers extract regular tendencies from the examples they observe.

The American Mathematical Society, in its newsletter, summarized the Boolean complexity result as ***incompressible is incomprehensible***—an elegant phrase I wish I'd thought of first. But I'd rather turn it around: in a sense, ***compressible is comprehensible***, or, perhaps, ***compression is comprehension***. Minimization and inference are deeply intertwined, and I think one of the major challenges of psychology now is to understand how and why.

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The GENERAL Psychologist

Getting Down to BUSINESS

The Spring issue of TGP is the Election Issue, featuring bios and statements of the candidates for Society offices who have been nominated and who have agreed to serve if elected. This year there are three candidates for the office of President-Elect but no openings for other offices. Recall that recent Bylaw changes reduced the number of Members-at-Large of the Executive Committee from 6 to 3 and a decision was made to let attrition take its course to get down to that number. For President-Elect the candidates are Janet Matthews, Bonnie Strickland, and Jeremy Wolfe.

Candidates for President-Elect

Janet R. Matthews. I completed my Ph.D. in clinical psychology at the University of Mississippi following my internship at the University of Oklahoma Health Sciences Center in 1976. I am board certified in clinical psychology through ABPP and in assessment psychology through the American Board of Assessment Psychology. I was unique among my classmates in both my doctoral program and internship in my career goal – to be an undergraduate educator. I was a tenured Associate Professor of Psychology at Creighton University before moving to my current position of Professor of Psychology at Loyola University New Orleans. My broad interests within psychology seemed best suited to the liberal arts environment. I currently teach courses from the freshmen to senior level. I co-created our Psychology of Women course and continue to teach it annually. I was recruited by Loyola in 1984 to develop our mental health field placement program and continue to direct it. Over the years, I have found one of the great rewards of this position is to assist students in finding a good career and graduate school match to their interests. My publications have been generalist in nature spanning the pedagogy of teaching, ethical issues, and clinical neuropsychology. About nine years after receiving my doctorate, I took a one-year leave to complete a formal postdoctoral fellowship in assessment with a focus in neuropsychological assessment. I have been professionally active on many levels having served, among other roles, as president of both Southwestern Psychological Association and APA Division 2, 3 terms on the APA Council of Representatives, and an at-large seat on the APA Board of Directors. I am honored to be nominated as a candidate for President-elect of APA Division 1 and, if elected, would endeavor to continue the great work of my immediate predecessors Linda, Bruce, and Peter.

Matthews' Statement. As a member of APA's Task Force on Membership Recruitment and Retention and then President Sternberg's Unification Task Force, I have developed a strong interest in those factors that seem related to the integration of all facets of our discipline. With the growth of specialty professional organizations, I see the need for APA to continue to be a place where all voices are not only heard but interact to be even more important. Without a place for this type of interaction, we lose the benefit of discussing our broad base of knowledge among specialists. Over my years in APA governance, I have noted that colleagues often have trouble placing me in a particular "slot." Am I a practitioner, educator, scientist, advocate for public interest? My response is that I truly believe I am a bit of each of them. To me, that background seems highly correlated with the purpose of our Society. For us to serve the unifying function suggested by Peter Salovey in his candidate statement last year, however, I believe our Society needs to grow. In recent years, the Society has actively added Fellows to our rolls. Data suggest such recognition increases the likelihood of remaining a member. I would like to build on this base. APA data I have studied suggest that about the third year of membership is when APA loses members. Although that may be related to the dues structure, I also wonder if it is also related to searching for a place to become active and not finding it. From conversations I have had with leaders of APAGS, this is especially true to those new members who have been active as graduate students but are now full members. I would like our Society to make special efforts to learn what activities these new professionals value and see which ones fit with our mission. That project would be the major goal of my presidency should I be elected.

Bonnie Strickland. Although I received my Ph.D. in clinical psychology and hold a Diplomate, my research has spanned clinical, developmental, personality and social psychology. I have spent my professional career as a professor of psychology at two universities, Emory University and the

University of Massachusetts at Amherst. I take great joy in teaching ranging from large undergraduate courses to the clinical and research supervision of graduate students. Within academia, I have also held a number of administrative roles at the same time that I have been a consultant and in long time independent practice. Especially as I have grown older, I consider myself a general psychologist and have welcomed the opportunity to become involved in Division 1.

My early research focused on internal versus external locus of control expectancies in relation to health, prejudice, social action, and trust. Some of my initial work during the civil rights days became a Citation Classic when I found that internal expectancies were related to Black social activism. Steve Nowicki and I also developed a Locus of Control Scale for Children that is still used internationally and is one of the most heavily cited references from the ***Journal of Consulting and Clinical Psychology***. My later research involved women and health especially in regard to gender differences in depression. I believe that my students and I were the first to present data on the preponderance of depression among various cohorts of women, an interest that eventually culminated in an APA Task Force and book on ***Women and Depression***. I was also involved in research on the mental health status of gay men and lesbians as early as the mid 1960's. This interest continues and much of my latest writing has been on gender roles and gay and lesbian issues.

Within the American Psychological Association I have been a member or chaired numerous boards and committees. I have involved in almost every one of the graduate education and training conferences in psychology over the last four decades. I have welcomed the opportunity to present testimony on behalf of psychology to the United States Congress. I was also intimately involved in the reorganization plans proposed for APA some 15 years ago. I have been President of the Division of Clinical Psychology, President of the Association, and am currently a member of the Executive Committee of Division 1. In these capacities, I have continually been impressed by the need to consider psychology as a coherent and unified field and discipline, one in which psychologists of every persuasion can feel at home.

Strickland's Statement: When the American Psychological Association reorganized in the mid 1940's following World War II, Division 1 became the keystone division for the Association. With over fifty years of growth and change, I still believe that Division 1 continues to serve that role for APA. No matter what our interests, that unifying mark of a psychologist is that we have been educated and trained in general psychology. Wherever our specialty interests may take us, psychologists continue to share methodologies, to respect the scientific method and use the basic and fundamental tenets of psychology. Now with some 55 or so Divisions and even more State and Provincial Associations, psychologists have ample opportunities to pursue their specialties with like minded colleagues. Division 1, however, is that unique group within APA that welcomes all psychologists. We provide a home where psychologists can consider issues of unity and the cohesiveness of the field with other psychologists of enormously diverse backgrounds and interests.

When I invite colleagues to consider joining Division 1, they often remark that they already belong to too many divisions. Why should they join yet another, especially one that doesn't represent their specialty interests? But, that's the point. While we may all appreciate and benefit from our specialty divisions and state associations, by definition these are focused on specific concerns. We also need our links with the basic areas of psychology and with each other. Division 1 provides those opportunities for us to keep ourselves abreast of the on going developments across all aspects of psychology, to build links to our specialties, and to present a unified and coherent vision of our field.

Over the last few years, our Officers and the Executive Committee have worked valiantly to expand our membership, especially through inviting Fellows of other Divisions to be Fellows in Division 1. The journals are impressive; the programming is comprehensive, and the Division is thriving. I would like to build on these efforts and especially continue to reach out to all psychologists to come and join us. We are a welcoming and vibrant home proposed over fifty years ago to meet the needs of general psychologists (who I think are the most of us). We still serve those needs as well as the interests of our Association and the greater society when we represent and nurture a unified psychology.

Jeremy M Wolfe - I would imagine that it has been a long time since a Professor of Ophthalmology was a candidate for President of APA's Division One but this is not quite as odd as it may sound. I am a vision and visual attention researcher and research on these topics can occur in a wide range of departments. Whatever my affiliation, I am an Experimental Psychologist at heart. I got my start very early when my father, a physicist at Bell Labs at Murray Hill, NJ, decided that his high school-aged child should do some paid work. He set me up with John Krauskopf, then also at Bell Labs and I got my start on a bite bar, trying to name the colors of nearly invisible spots of light. You can't bite a bite bar all day, so I wandered the halls asking other people what they did for a living. Since "other people" included Saul Sternberg, Bela Julesz, Charlie Harris, George Sperling, Dave Meyer, et al., I got quite an introduction to the field. Naomi Weisstein was visiting Bell at that time and from her I learned that a commitment to research in Psychology could go hand in hand with a commitment to broader social issues. I arrived at Bell Labs thinking that I would go to college to major in something like History. I went to Princeton the next fall determined to major in Psychology in general and vision in particular.

My real introduction to General Psychology came from Leo Kamin's Introductory Psychology class. I have now taught Intro. for more than 20 years and I still draw inspiration from that first exposure. I went from Princeton to MIT where I studied for my PhD under Richard Held. After marrying Julie Sandell (an undergraduate student with Charlie Gross at Princeton and a PhD student with Peter Schiller at MIT) and after completing a thesis "On Binocular Single Vision", I joined the MIT faculty. In an era when the Department was changing from "Psychology" to "Brain and Cognitive Sciences", I continued to teach a broad Introductory Psychology course and to advocate for the value of the field as a whole.

In the late 1980's, my research focus shifted from binocular vision and visual adaptation to visual search and attention. In 1991, I moved to Brigham And Women's Hospital and Harvard Medical School where I am now Professor of Ophthalmology. My research covers all aspects of visual search from the visual features that guide the deployment of attention, to the rules of deployment, to the consequences of attention for vision and memory. The work of the lab is primarily basic research with occasional forays into applied problems such as the search task faced by x-ray scanners at airports.

I have continued to teach Intro. at MIT as well as a course combining Psychology and Literature. This year I am teaching Intro. in the Harvard Psychology Department. I am a fellow of Divisions 1, 3, and 6 of APA and have served as Program Chair for Div. 6. I am also a fellow of APS and the AAAS and an elected member of the Society for Experimental Psychologists. I have been President of the Eastern Psychological Association, a NIH Study Section Member, an Associate Editor for Perception and Psychophysics as well as member of an assortment of other program committees and editorial boards. At last count, I have authored something like 70 papers, 18 book chapters, 21 other publications, and 170 published abstracts. I am grateful to be currently funded by the National Eye Institute, the National Institute of Mental Health, the Air Force Office of Scientific Research, and the Federal Aviation Administration.

Wolfe's Statement. I am honored to be nominated as President of Division One. I see the Division as a bulwark against the forces of excessive specialization. My present position is a pure research position. I have made the deliberate choice to teach Introductory Psychology because I know that otherwise I would read nothing but papers on visual attention (Actually, it's worse than that. The burden of reviewing being what it is, I would read nothing but unpublished manuscripts in visual attention.) I am very fond of my corner of the research world but I did not get into the field because visual search was interesting. I got into the field because it encompasses a vast array of topics of interest and importance. I suspect that the vast majority of members of Division One are, themselves, Psychological specialists of some sort. We value our specialties (and serve those APA divisions as well.) However, we join and serve Division One to nurture that part of us that looks over the wall, leans over the back fence, and, against the advice of our mothers, talks to strangers.

I would consider the Presidency of Division 1 to be an opportunity to foster interactions across the field. I don't think we need to argue for some strained "unity" of Psychology. The task of Division One is to introduce neighbors to one another through our publications and through our activities at the APA Convention. We can nurture the mutual respect and mutual interest that will strengthen the field. Moreover, we can advocate for the field, as a whole, in the broader community. We can convince newcomers and remind old-timers that Psychology, broadly conceived, is an intellectual enterprise as fascinating as any on the planet.

The Society for General Psychology

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Spring, 2003, Volume 38 Number 1

The Society for General Psychology
American Psychological Association
750 First Street, NE
Washington, DC 20002-4242