

# Operants

## A Newsletter from the B. F. Skinner Foundation



Third Quarter 2013

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### *Features & Articles*

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<b>Skinner's Corner:</b>	Per Holth .....	2
<b>Spotlight:</b>	Jerry Ulman .....	6
<b>Events:</b>	HABA .....	9
<b>Tribute:</b>	Peter Dews .....	10
<b>Awards:</b>	B.F. Skinner Foundation Student Research Awards: APA, FABA .....	15
<b>International:</b>	Interview with Willy-Tore Mørch .....	16
<b>President's Column:</b>	Julie S. Vargas .....	19

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## From the Editor

Joyce C. Tu, Ed.D., BCBA-D,  
Editor-in-Chief

We are excited to welcome four new associate editors in this third quarter issue of *Operants*: Dr. Aleksandr Fedorov from Novosibirsk State University in Russia, Anna Luzi of Alia Consulting in Italy, Sheila Habarad, an independent BCBA, and Steven Rodriguez of May Institute/ASAT, also a graduate student at Westfield State University.

In this issue, B. F. Skinner Foundation board member Dr. Per Holth talks about the interactions between heritability and contingencies. Our new associate editor Steven Rodriguez has a wonderful opportunity interviewing Dr. Jerry Ulman on behaviorologists' take on behavioral economics and his advice for young behavior analysts. Sheila Habarad reports on the upcoming HABA conference. *Operants* Associate Editor Erin Bremer contacted Dr. William H. Morse for an obituary on Dr. Peter Dews. Dr. Morse was kind enough to send it to *Operants*. We are also delighted to present the B. F. Skinner Foundation graduate student research award winners: through APA Division 25, Jeffrey Tiger and Ryan Ward, and through FABA, Catherine Kitts Martinez.

Also in this issue, Monica Vandbakk presents a thought-provoking piece on Willy-Tore

Mørch. Finally, we have Dr. Julie Vargas's reporting on the current activities of the Skinner Foundation. As always, we welcome your comments, and hopefully, you will enjoy this current issue!

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## Skinner's Corner: [A closer look at a particular aspect of Skinner's Theory of Behavior] Heritability and Contingencies

Per Holth, Ph.D., Director of  
B.F. Skinner Foundation  
Board, Professor, Oslo and  
Akershus University



Per Holth

Professionals and laymen alike often have strong opinions regarding "the relative importance" of heredity and

environment, and it is a topic which easily triggers a heated debate. In everyday conversation, a person's behavior, interests, and traits are often explained primarily, or even exclusively, by either heredity or environment. In what sense can environmental determinism be opposed to heredity?

A collection of genes will not become an organism without an interaction with the environment,

and an environment will never transform to become an organism without interacting with a collection of genes. Thus, nurture without nature is as dead as nature without nurture. Any organism, including its behavior (and features) must therefore be a joint product of heredity and its environment.

Nevertheless, even professionals have been tempted to discuss the relative roles of nature and nurture. For instance, what percentage of a property (such as behavior, interests, or intelligence) can be ascribed to each set of variables?

However, one may ask how much of the variation of some feature among the members of a population can be explained by variables in the environment during the lifetime of the individuals, and how much of the variation is explained by variables in the evolution of the species.

Let us assume that we are interested in the length of sunflowers and how variations in heredity and variations in environmental conditions affect the size of the full blooming flower. In a first experiment, we take a handful of sunflower seeds of unknown variations in their genetic materials, and plant them in a completely uniform soil, where we are allowed to ensure exactly the same nutrition, temperature, light exposure, and so on, for each of

the seeds. After a few weeks, we can measure the length of the flower stalks and note that they differ. If so, we can answer the question of where the variation in the length of the stalks comes from by pointing to variation in heritage. The variation in the length of the stalks is caused 100 percent by variation in heredity, because variation in the environment during the lifetime of the flowers was nonexistent.

In our second experiment, we select only one flower seed to be cloned, so that we get a handful of genetically identical seeds. We plant these seeds in a regular field, where we have very little control over varying environmental conditions across the field. After a few weeks, we can observe that the flower stalks, again, have grown to different lengths. This time, all of the variation in the length of the flower stalks must be caused by variation in the environment of the cloned seeds because, by cloning a single seed, we made sure that variation in heritability was nonexistent.

Hence, our first experiment showed a heritability of 100 percent, while the second experiment showed a heritability of 0 percent for the same phenomenon (i.e., the variation in stalk length in a population of sunflowers). In general, then, heritability varies with the extent of genetic and environmental variation for the popu-

lation under study. Heritability is higher when the environment is very uniform, and lower when the environment is more variable.

A curious implication of such a concept of heritability is that the more effectively a society arranges the environment to ensure equal opportunities for education and a job career for its citizens, for example, the more heritable social differences become. Thus, the relatively high heritability of social differences in social democracies clearly does not imply that the environment is less important for education and for professional careers. It is only less important as an explanation of their variation within populations in such environments. Thus, because the degree of heritability of a specific trait will vary with the variability of genes and environment in the lifetime of organisms, a specification of the degree of heritability will only apply to that property in populations of the same variability in genes and environment and for as long as the variability remains the same.

Research into the relative importance of heredity versus environmental variables in the lifetime of the individual becomes much more complex than the sunflower examples above. Except in the case of monozygotic twins, genetic variation is obscure, and environmental variation is just as complex.

Researchers have sometimes defined “the same environment” as growing up in the same family or under similar socio-economic circumstances. This is an example of what Skinner called “crude environmentalism,” and as he pointed out, “...a mere shift in emphasis from man to environment means very little” (Skinner, 1971, p. 181).

The major breakthrough in the scientific study of how the environment works on behavior during the lifetime of the individual was Skinner’s discovery of operant contingencies (see Iversen, 1992; Vargas, 2004). As a result of experimental analyses of behavior, built upon Skinner’s early discoveries, we now know a lot about how such contingencies of reinforcement in the lifespan of an individual organism can produce complex behavior interacting with a current environment. That fact has never left heredity or genes unimportant. Members of all species are born with important characteristics, including the capacity for their behavior to be reinforced by, and brought under the control of certain types of stimulation from the environment. The biologists are obviously right that, biologically, humans are animals and a result of contingencies of survival in the history of the species. From the embryonic state, we are unique compounds of genes that work in interaction with environmental variables. Interacting with the environment, the genes pro-

duce an organism which, next, is affected by gradually more complex interactions with the environment. For humans, this involves a social environment, consisting of practices that have evolved significantly over a time span during which genes must have remained relatively unchanged. Thus, if we could transplant a child from the stone age into a modern family, that child would probably operate cell phones and computers, very much like other children of our time.

Darwin, of course, was the “Skinner of phylogeny” (cf., Donahoe, 1988), and the major breakthrough in the scientific study of how the environment works on species over generations was Darwin’s discovery of the importance of contingencies of survival in natural selection. The dichotomy between heredity and environment is a false one, because heredity is also traced to the selecting environment, just on a different time scale, in the contingencies of survival of the species. One can still make a useful distinction between characteristics of individuals in current settings, based on whether they primarily evolved as a result of contingencies of survival or contingencies of reinforcement. To focus primarily on the role of the environment in the lifespan of individuals is not to dismiss the role of the environment in the history of the species.

As Skinner (1974) pointed out:

Not only is verbal behavior said to show the operation of innate rules of grammar, but “innate ideas such as size, shape, motion, position, number, and duration” are said to “give form and meaning to the confused fragmentary data that we experience every day in our lives.” Size, shape, motion, position, number, and duration are features of the *environment*. They have prevailed long enough and behavior with respect to them has been crucial enough to make the evolution of appropriate behavior possible, but contingencies of reinforcement are at work every day in the life of the individual to generate supplementary behavior under the control of the same features. The greatest achievements of the human species (not of the human mind) have occurred too recently to make a genetic explanation defensible, but whether we appeal to contingencies of survival or contingencies of reinforcement we can at least dispense with the appeal to innate ideas. It may be true that there is no structure without construction, but we must look to the constructing environment, not to a constructing mind. (Skinner, pp. 116-117)

Some of Skinner’s critics have insisted that he became interested in evolution and the phylogeny of behavior only very late in his career – sometimes implying that his engagement was just patchwork to save his operant formulation in the face of increasing evidence of biological “constraints on learning” (e.g., Delius, 1988; Herrnstein, 1977), such as “the Breland effect,” “autoshaping,” and taste aversion (“the Garcia effect”). Keller Breland had a prevision of potential effects of their paper (Breland & Breland, 1961) when he wrote a letter to Skinner after having read the galley proofs of their article: “...it occurred to us that it might convey impressions not intended” (Skinner, 1977). And in an interview, Chomsky (Virués-Ortega, 2006) while explaining the devastating effect of his review of Skinner’s (1957) *Verbal Behavior*, claimed that:

By the early 1960s, a couple of years after the review appeared, there was internal criticism which shattered what was left of the foundations of the subject. Two of Skinner’s major students, Keller and Marian Breland, went off into animal training. They were the main animal trainers, they wanted to train all the things, circus animals and so on. What they discovered was that this was just not working (Bre-

land & Breland, 1961). I mean, the trainers, the psychologists, they were actually using the instinctive behavior of the animal and slightly modifying them by a training routine. But then, the animals were just drifting back to their normal instincts, to their behavior, refuting all the theory. (p. 246)

Responding to the accusation of his (too) late interest in evolution, Skinner (1977) simply listed evidence to the contrary throughout his career. An interesting additional detail that has seldom been mentioned appears in the second chapter of *Schedules of Reinforcement* (Ferster & Skinner, 1957): “In such a bird as the pigeon, pecking has a certain genetic unity; it is a characteristic bit of behavior which appears with well-defined topography. Its features may nevertheless be modified by differential reinforcement ...” (p. 7)

Without dismissing the role of the environment in the phylogeny of species, there are still some particularly good reasons for distinguishing it from the role of the environment during the lifespan of individuals, and for focusing primarily on the latter:

Contingencies of reinforcement have the edge with respect to prediction and control. The conditions under which a person ac-

quires behavior are relatively accessible and can often be manipulated; the contingencies under which a species acquires behavior are very nearly out of reach. (Skinner, 1974, p. 44)

No matter how important the heredity of an organism in determining its behavior, it could not be changed after conception. (Skinner, 1983, p. 103).

Even if explanations of behavior require that both ontogenetic and phylogenetic variables are taken

into account, contingencies of reinforcement will require its own specialists. It is tempting to say that knowledge of how behavior is affected by reinforcement contingencies is crucial. In a Skinnerian analysis, however, what is a crucial goal is to create an environment which generates behavior of the kind that we colloquially may summarize as “knowledge of how contingencies of reinforcement work.” The verbal behavior of Chomsky (above) and the current diffusion of cognitive psychology may indicate how far we are from reaching that goal.

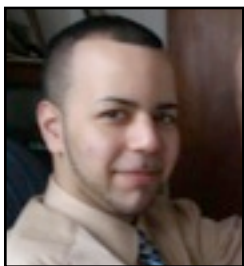
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## Spotlight: Jerome Ulman, Ph.D., BCBA-D

*Interviewed by Steven  
Rodriguez, BS, Associate Editor*



*Steven Rodriguez*

Professor Emeritus of Special Education as of last year, Jerome Ulman taught courses in

the areas of applied behavior analysis and behavior disorders at Ball State University since 1974. He also coordinated the online graduate program in applied behavior analysis.

Before joining the Ball State faculty, he was employed as a school psychologist in Florida, a research scientist at Choate Mental Health and Development Center in Illinois, and a behavioral consultant in Indianapolis, Indiana.

His research interests include behavioral research methodology, applied behavior analysis in special education, and the sociocultural implications of behaviorology. He is currently the Secretary-Treasurer of the International Society for Behaviorology.

***How did you come to develop an interest in the field of behavior analysis?***

During my master's work in psychology at the University of South Florida in 1966-68, some of my classmates were oriented toward clinical psychology, others toward behavior analysis. Initially, my career goal was to become a clinical psychologist.

But I soon became intrigued with my behaviorally oriented classmates—particularly at the local pub, where we engaged in debates about the nature of human behavior. After finding time after time that I was losing these arguments, I humbly asked what book would provide a good overview of Skinner's perspective. *Science and Human Behavior* was immediately recommended. Perusal of this book proved to be a life-changing experience for me, eventuating in my completing the doctoral program in behavior analysis at Southern Illinois University in 1973 followed by a 39-year academic career as a Skinnerian.

***Please describe your current research and recent behavioral interests.***

My research interest has been, and continues to be, the sociocultural implications of Skinnerian science. From there I began thinking about what relevance our science may have for the social sciences: anthropology, sociology, political science, and especially economics. I found very little mention of Skinner in the social scientists' discussions of human behavior,

along with a great deal of ignorance and misunderstanding about behavioral principles. Commonly, these writers confuse Skinner's behavior theory with that of Pavlov or Thorndike. For the most part, mentalism reins supreme within these disciplines.

My current effort is to explore which behavioral sciences appear closest to the natural science perspective. The one I came to focus upon, at least for now, is evolutionary economics. Currently, I am working on a behaviorological critique of a recent book titled *Darwin's Conjecture: The Search for General Principles of Social and Economic Evolution* by Hodgson and Knudsen; I am also participating in the authors' blog. I anticipate submitting my work to one of their journals, probably the *Journal of Evolutionary Economics*. To avoid appearing as a naive academic drifter from afar, it has become incumbent upon me to acquaint myself somewhat with their literature—a goal I have been working on a while.

***Through your experience in the field, what recommendations do you provide to new behavior analysts?***

First, a behavioral practitioner must always be on the lookout for agencyism (a.k.a. explanatory fictions) creeping into their work; for example, substituting hunches about hypothetical inner entities in place of conducting functional

analyses. As another example, when I was working as clinical director in an autism treatment center, I observed all too often that practitioners would misconstrue the term “motivating operation” as something operating within the client (e.g., “Jack’s aggressive behavior is motivated by anger”). Second, I would advise not getting so confined to applied work alone that the rest of Skinnerian science is overlooked—basic research as well as conceptual/philosophical work. And, of course, keep up to date in whatever applied area one happens to be in. Lastly, don’t become overly concerned about making mistakes; instead, focus on what can be learned from them.

***What would you rank as Skinner’s top three most important contributions to behavior analysis?***

I consider Skinner’s three most important contributions to be *Verbal Behavior*, *Behavior of Organisms*, and *Science and Human Behavior*. Skinner considered *VB* to be his most important contribution. And for me, unquestionably so. The acquisition of verbal behavior is the most consequential feature of *Homo sapiens*. *B of O* laid the foundation for our discipline’s experimentally based conceptual framework, and established contingency relations as the basic unit of analysis. I view *S & HB* as Skinner’s third important contribution because it

is so effective in explicating the relevance of contingency theory for achieving a scientific understanding of human behavior in the world at large.



*Jerry Ullman*

***What is behavioral economics and its relevance to the field of behavior analysis?***

Actually, there are several kinds of behavioral economics. First, there’s the operant approach to developing economic models; for example, increasing the response requirements on a ratio schedule to investigate the cost of a good or the amount of work expended for it. This research also has relevance for applied research, such as investigating supply and demand issues with substance abuse.

These concepts come from microeconomics, one part of mainstream or neoclassical econom-

ics. Focusing on the sociocultural implications of Skinnerian science, my interest lies in heterodox economics, consisting of a variety of perspectives but having in common the rejection of mainstream economics (not unlike behaviorology’s consistent rejection of mainstream psychology); in particular, rejection of the assumption of the completely rational economic individual (*Homo economicus*, a behavior-guiding homunculus). Thus, with the exception of the operant researchers, mainstream economists’ conception of human behavior relies heavily on agencyism. Although heterodox economics is not devoid of it, I imagine that sooner or later they will become open to exploring Skinner’s work. And I imagine that this will be particularly true of those heterodox economists who are most aligned with natural science; specifically, evolutionary/institutional economists and ecological economists.

***Historically, the field has had difficulty marketing itself. What suggestions do you offer for the field to better market itself and to overcome the misconceptions regarding the field?***

The problem lies in the fact that the society within which we live has to some degree demonized Skinner and operant conditioning. As with other historically important scientific achievements having a profound impact on the

status quo, we should anticipate nothing less concerning Skinner. Consider this—not only is the earth not the center of the universe (Copernicus); we are not special creations of a supernatural force (Darwin); and we do not have a mind/soul (Skinner). For Skinner, people are mindless, but certainly not unthinking creatures. In this society, the legal system clearly illustrates the ascribing of criminal acts to an agency within—the concept of intentionality. Thereby, this conflict-permeated society is conveniently exonerated of blame. It also serves to provide a lot of employment for psychologists and theologians.

I suspect that until there is a significant change in our culture, one that diminishes the strength of belief in agencyism, such misconceptions will persist. I contend that when Skinnerian science and its social implications receive general acceptance, then and only then will it become realistic to expect our science to make the headway it merits. Meanwhile, I'm afraid that Skinner's theory of behavior isn't going to be treated with much kindness by mentalistic/cognitive psychologists or—as consumers of their writing and lecturing—the general public. At the same time, however, we must not overlook exceptions; most noticeable today, parents of children on the autism spectrum.

***What is your point of view on private events and its relevance to the practice of behavior analysis?***

Without an adequate understanding of private events, one has an unsatisfactory grasp of Skinnerian science, and thus a deficient preparation for applied work.

The difference between private and public (i.e., covert and overt) events is but a matter of accessibility. For example, behavioral phenomena that were once private may, through technological advances, become public. Electronic voice analysis, like a lie detector, can identify emotional stress but without the speaker's awareness of being monitored (often used job interviewing; e.g., “Did you ever steal anything from your last employer?”).

Essentially, private events amounts to an ontological issue: private and public as the two poles of a single dimension, the very antithesis of the presumption of mind and body as different worlds. In short, what this question really amounts to is that events going on inside are not different in kind from those on the outside, merely differences in locus and accessibility. However, it is imperative that private events are never viewed as a cause of behavior; otherwise one reverts to agencyism.

***What do you see as the biggest challenge for behavior analysis in the future?***

First off, I think it is accurate to deem behavior analysis a field, not a discipline. Given the extent of contradictory viewpoints by those calling themselves behavior analysts, discipline doesn't seem appropriate. As I see it, what divides the field most is the enigma of agencyism, including blatant mentalism. It even appears in the basic behavioral literature, such as in the *Journal of the Experimental Analysis of Behavior*. And it is largely standard fare for behavioral psychologists, particularly those who refer to themselves as “contextualists.”

Underlying the perpetuation of agencyism in the behavioral literature, I believe, is the wide acceptance of pragmatism as a guiding scientific philosophy. Unfortunately, the ambiguity inherent in pragmatism permits an agnostic position with respect to believing in a mentalistic world as a reality. I posit that this problem can be eliminated by nothing less than a thoroughgoing adoption of the philosophical position of scientific materialism as exemplified in the natural sciences, coupled with Skinner's position on private events.

Lastly, as Skinner (1950) has made clear in his paper, “Are Theories of Learning Necessary?”, behavior theory eschews



“any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions.” The subject matter of our science is the investigation of contingency relations; not what happens at the neurophysiological dimension (but this by no means implies the exclusion of interdisciplinary research). Pavlov’s speculation about the conceptual nervous system never got us anywhere, nor have subsequent attempts. Have brain scans become the contemporary Rorschach for speculating about neuronal causes of human behavior?

***Do you have any thoughts on historical events or cases in the past that may have been detrimental to the field?***

I would nominate the publication of Chomsky’s review of *Verbal Behavior* as a major detriment to the field. Without a shred of evidence that he understood what Skinner wrote, Chomsky succeeded in defaming the operant analysis of language. Curious! His “review” is cited quite a lot in the social science literature. And of course, there are the cumulative effects of misinformation, if not baneful attacks, on Skinner’s work found in introductory psychology books and in the popular media: Skinner’s daughter, having been raised in a box, was committed to a mental

institution; Skinner’s theory leads to totalitarianism; and so on, ad nauseam.

***Any final thoughts you would like to share with Operants readers?***

I believe a big challenge for those who identify themselves as behavior analysts is to critically appraise precisely what that term means. It is in fact quite ambiguous, constituted of behavioral psychologists, interbehaviorists, contextualists, methodological behaviorists, and so on—some actually being anti-Skinnerian!

In contrast, I identify my discipline as behaviorology: the natural science of the relations between actions and the contingencies that control those actions, built on the behavioral principles and philosophical perspective that B. F. Skinner contributed to humankind. I may not see it in my lifetime, but I am confident that the day will come when the name Skinner will be just as prominent within the life sciences as that of Darwin.

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**Events:**  
**HABA’s annual conference**  
*Sheila Habarad, MA, BCBA,*  
*Associate Editor*

HABA’s 5th Annual Conference will be taking place October 11th and 12th in the heart of downtown Indianapolis, Indiana.

HABA’s mission focuses on facilitating humane, ethical, and effective behavioral practices in academic, research, home, school, clinic, community, and other settings.



*Sheila Habarad*

To this end, HABA has several objectives, one of which is to conduct an annual conference focused on research and

professional practices in behavior analysis. The conference has brought in a few hundred professionals, educators and parents from across the midwest into the heartland over the past five years. While HABA anticipates the number of attendees to continue to grow, it will be the first year offering a single-track conference lined up with quality speakers to provide an intimate and stimulating environment.

The Hoosiers will be hosting some of the finest speakers from across the nation: Dr. Barb Esch, Dr. John Esch, Dr. Peter Gerhardt, Dr. Richard Kubina, Dr. Eric Larsson, Dr. Patrick McGreevy, Dr. Henry Schlinger, Dr. Carl Sundberg, Dr. Jerome Ulman, Dr. Susan Wilczynski, and Dr. Merrill Winston. The material covered will range from beginning application to ad-

vanced theoretical topics providing interaction and learning opportunities for all attendees.

Each day will begin with an enlightening presentation by two renowned behavior analysts. Dr. Aubrey Daniels and Dr. Julie Vargas will be providing HABA's 2013 keynote addresses. Friday, Dr. Aubrey Daniels will start off the conference with an Organizational Behavior Management (OBM) presentation, which has been a growing focus in Indiana. Many members of HABA are highly anticipating Dr. Daniels's presence at the conference and the insight he will provide into OBM. Dr. Julie Vargas will begin Saturday's lineup of speakers, providing a peek into her life, titled "Dr. B.F. Skinner: The Scientist as a Father." The title of her presentation alone has many people enthusiastic for the upcoming conference.

Not only will HABA be hosting a conference with a distinct lineup of speakers, there will also be an array of events scheduled. The B.F. Skinner Foundation Auction scheduled for Friday the 11th during lunch always provides entertainment as the attendees and presenters bid on precious items from B.F. Skinner's life. The money earned from the auction goes back to the B.F. Skinner Foundation and to HABA.

HABA's poster session will occur Friday night after dinner, providing an opportunity for students and practitioners in the field to show off their research. The main author of each poster receives free admission into the conference for the weekend.

The annual raffle will also continue into 2013, where all posters are entered into a raffle for three \$100 prizes. HABA had a large number of posters in previous years and predicts more growth this fall.

HABA will be hosting a breakfast on Saturday morning to facilitate enlightening discussions regarding the presentations and current events in the field of behavior analysis. Lunch will consist of an annual update provided by HABA's subcommittees followed by voting for 2014's Board of Directors. Enriching presentations will follow both of these two events.

*To present news from your organization in future editions of Operants, please send an email to:  
info@bfskinner.org.*

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**Tribute:**  
**Peter B. Dews (1922-2012)**  
*Obituary by W. H. Morse, Ph. D., Professor of Psychiatry, Emeritus, Harvard Medical School*

Peter Dews died on November 2, 2012, after several years of declining health. He is widely recognized as the single individual most responsible for the emergence of behavioral pharmacology as an experimental science concerned with the rigorous assessment of the behavioral effects of drugs.

From the time of his first experiments on scheduled controlled behavior, Dews understood that the sequential scheduling procedures he was using in elucidating the pharmacology of central nervous system (CNS) drugs had general significance for all behavioral phenomena, including many aspects of biomedical sciences. It was through a series of fortunate circumstances and wise decisions on the part of Dews that this Englishman with no formal training in anything to do with behavioral science became a powerful advocate for an awareness of behavior as a part of biology, and of the need for biological scientists to have an understanding of behavior in physical as opposed to subjective terms.

Peter Dews was born in the north of England, in Yorkshire, and lived there until his mid-twenties. He had very good schooling in the fundamentals of mathematics, physical sciences, and the English language before going to the University of Leeds to study medicine. This early experience, coupled with his original, independent temperament, is reflected in his educational philosophy that one good introductory course is sufficient to prepare a student to begin independent, individual study—very different from the way the same material is taught repeatedly in high school, college and graduate school in the United States. Dews treated postdoctoral fellows as he would like to be treated—to be provided with adequate resources and then left alone to use them.

In medical school, Dews liked physiology and pharmacology the best of all subjects. After finishing his internship in 1945 and not wanting to be a junior person in the Department of Physiology at Leeds, he became a Demonstrator and then Lecturer in the newly established sub-department of Pharmacology under W.A. Bain. Bain had in his possession a large quantity of a potent extract of marijuana (itself a story of serendipity), and Dews began studying the actions of "red oil" on the behavior of laboratory animals with no success at all. However, from that time Dews had an in-

terest in the behavioral effects of drugs. (For more details see Dews, 1997.)



*Peter Dews*

When J.H. Burn came to Leeds to fill in for an unavailable external examiner, he met Dews and invited him to spend the summer of 1946 at Oxford. While there, Dews and J. D. P. Graham studied the diverse pharmacological effects of the antihistamine pyrilamine, a report of which appeared in the first volume of the *British Journal of Pharmacology*. Two years later, Burn suggested Dews as a possible alternative to replace the original candidate for a fellowship position at Burroughs Wellcome in Tuckahoe, New York.

Dews came to the United States in 1948 and spent two years as a research fellow at Burroughs Wellcome. He did collaborative research on the histamine libera-

tor 48-80, and conducted an independent study on the effects of various psychomotor stimulants and convulsants on what he termed "voluntary activity" in mice. During this time, he realized that he was interested in the CNS effects of drugs on behavior, but was still unsure about how to proceed. While at Leeds, he had sought advice from other pharmacologists on ways to study behavior in experimental animals, but nothing that was suggested (Pavlovian conditioning, learning in mazes) seemed suitable to Dews for determining quantitative dose functions. In the 1940s, there was literally no interest among pharmacologists in the United Kingdom in the behavioral effects of drugs, and even Dews himself, when studying the varied pharmacology of antihistamines, never considered investigating the "drowsy" effects reported in man.

Before coming to New York, Dews took a vacation in Switzerland and stopped in Paris at the laboratory of Daniel Bovet to visit with Bernard Halpern, another antihistamine researcher. Their friendship was renewed a year later when Halpern came to New York en route to a fall meeting of the American Physiological Society in Minneapolis, and to give a lecture at the Mayo Clinic. When Halpern suggested that Dews accompany him to Minnesota, Dews hastily changed his vacation plans

and did so. At the Mayo Clinic, Charles Code, Halpern's host, had a research interest in histamine and antihistamines, and during their first casual conversation, Code asked Dews if he would like to work at the Mayo Clinic, saying that he knew of Dews's anti-histamine paper.

In 1949, Dews married Grace Miller, also employed at Burroughs Wellcome, and after fulfilling his commitment there, moved in 1950 to the Mayo Clinic. He worked on the effects of cortisone, ACTH, and adrenalectomy on anaphylaxis in the Section on Physiology with Code, which formed the basis of his Ph.D. in Physiology from the University of Minnesota.

The person at the Mayo Clinic who most influenced Dews was the biostatistician, Joseph Berkson, with whom he worked in 1952 as a research associate in the Division of Biometry and Medical Statistics. His interest in statistical analysis and estimating error took form then and continued throughout his career, particularly when he later turned his attention to risk assessment in behavioral toxicology.

In 1952, Otto Krayer, at that time seeking someone interested in the CNS to fill a position in his Department of Pharmacology at Harvard, came to Rochester to give a Mayo Foundation Lecture.

Earl Wood, who had previously been in Krayer's department, knew that Dews had written a paper on voluntary activity in mice, and suggested Krayer meet him. When they met, Dews (now seven years past his medical degree and on leave from a permanent position in Pharmacology at Leeds) accepted Krayer's offer of an instructorship.

From the 1930s, B. F. Skinner had been interested in the effects of drugs on behavior, saying the brain could be "unlocked with a molecule better than with a scalpel." After Skinner returned to Harvard from Indiana in 1948, he periodically telephoned Krayer asking if any of his staff members were interested in drugs affecting behavior. Immediately after Dews arrived, Krayer suggested that he go over to Cambridge to see Skinner. Dews met Skinner in his office, and then Skinner's associate, Charlie Ferster, showed Dews around the research laboratory. There were more than a dozen setups where the pecks of pigeons inside enclosed picnic boxes were being recorded cumulatively in time on paper tracings. Dews had never heard of B. F. Skinner and he didn't know what he and Ferster were studying, but Dews instantly recognized the paper tracings as the equivalent of slope kymograph recordings, and that the procedures that produced them could be what he had been

looking for: a way to measure the effects of graded doses of a drug on a quantitative aspect of behavior in continuous time. As Ferster walked around showing the experiments that were going on in the different chambers, he understood that Dews appreciated what he was seeing even without specific knowledge of what the experiments were about.

Ferster invited Dews to come back and make injections in the middle of sessions to see what would happen. When such treatments resulted in an interesting change in the pattern of the cumulative response record it constituted an experiment for Ferster. He thought it was wonderful to have Dews coming over and altering schedule performances with injections of pentobarbital, antihistamines, LSD, and the marijuana derivative synhexyl. Single observations on all these drugs are reported in figures in *Schedules of Reinforcement* (1957). It took Dews a little longer to establish to his satisfaction that this was a worthwhile approach for studying the effects of drugs. He did this by chronically treating pigeons with sodium bromide and seeing that their altered performances were related to the bromide blood level.

Ferster gave Dews all the components for several setups and helped him put them into operation in the Department of Phar-

macology to study the effects of drugs on schedule-controlled performances in the pigeon. Pigeons were not a species used in pharmacological research, but Kraymer gave this venture and its subsequent expansion his full support. Kraymer championed the extension of pharmacology to other fields as strongly as Skinner championed the wider application of behavioral techniques. Kraymer tried to make the professional situation for every member of his department as good as could be, which Dews fully appreciated. In every way, Kraymer encouraged this rather unusual type of behavioral research as part of pharmacology, even reading and commenting on drafts of manuscripts, a helpful but humbling experience for authors.

Dews was also outstanding in giving editorial help, and it was a pleasure to get his good advice about manuscripts. If he thought a sentence wasn't quite right, he rewrote it and always made it better. Here and there he would scratch out one word and substitute another that had just the right nuance. He had a very good sense for using words. For example, in 1947, in characterizing the antagonistic properties of drugs such as atropine and antihistamines, Dews employed the term "agonist," a very early, if not the first, use of this word in a pharmacological context to designate

the substance against which a specific antagonist is effective.

In his initial experiment, Dews followed Ferster's advice and studied pecking in pigeons where a brief presentation of food followed a peck under two different scheduling conditions. The drug he chose to study was pentobarbital, and its effects on the rate and pattern of pecking were dose dependently related to the two schedules. (Later he commented that with some other drugs the results would not have been so clear and may have discouraged his continuing this type of research.)

This first experiment influenced Dews profoundly. He realized that he had, at last, quantitative assay procedures for studying the effects of drugs on behavior in a pharmacologically rigorous way. Perhaps equally importantly, he appreciated the positive advantages of Skinner's general approach of studying behavior in an isolated, controlled space without extraneous influences, and describing it in objective physical terms.

In subsequent experiments, he used scheduling procedures to study how behavioral effects of drugs were related to the established psychological concepts of motivation and discrimination by varying the degree of food deprivation and the complexity of

stimuli. He also continued studies on different scheduling conditions. Neither traditional psychological explanations of behavior nor the pharmacological classification of drugs as stimulants or depressants appeared to be useful in interpreting the details of his results.

Generalizing from the combined specific findings in these experiments, Dews concluded that the behavioral effects of drugs depended predominantly on the behavior engendered by the controlling scheduling conditions and could be changed by changing the scheduling conditions. Later, a relation between ongoing behavior and magnitude of drug effect was shown quantitatively for many drugs.

From the start, Dews regarded behavioral pharmacology as a discipline of pharmacology exemplified by rigorous assessment of the effects of drugs on objectively quantifiable behavior, and distinguished it from more clinically oriented psychopharmacology. When the *Journal of Pharmacology and Experimental Therapeutics* established specific field editors to oversee publications in various fields of pharmacology, Dews became the field editor for behavioral studies and in that capacity further influenced the direction of research in behavioral pharmacology. The Division of Behavioral Pharma-

cology within the American Society for Pharmacology and Experimental Therapeutics has ensured the perpetuation of the Dews legacy by establishing an award in his name to be given biennially. The most recent recipient of that award, James E. Barrett, has written a scholarly review of the experimental work of Dews and his formative influence on the field of behavioral pharmacology (Barrett, 2006).

Dews joined the Department of Pharmacology at the Harvard Medical School in January 1953, was promoted from Instructor to Associate Professor, and in 1962 was appointed Stanley Cobb Professor of Psychiatry and Psychobiology, assuming wider responsibilities in the Department of Psychiatry. He continued to conduct collaborative experiments on behavioral pharmacology, physiology, and toxicology as before, but from the early 1960s, his major independent research was the quantitative study of schedule-controlled performances. He regarded Skinner's early work and Ferster and Skinner's scheduling procedures as the most significant influences on his career, and a recurring theme in his many reviews and essays was the recognition of how the ongoing behavior of life is controlled by its sequential interplay with environmental happenings.

Dews did not enjoy giving formal lectures, nor were they charismatic, yet his impromptu speaking was elegant and effective, with a delightful quality of improvisation. Lectures in pharmacology given to Harvard medical students by the same individuals were similar year to year, except those of Dews. For example, when lecturing on antiepileptic drugs, which patients take all their lives, Dews once gave a mini-lecture on the importance of physicians working with parents to establish an effective therapeutic dose with minimal side effects for chronically administered drugs. The next year his lecture changed to emphasize neurophysiological aspects of epilepsy. Fortunately, many of the off-the-cuff remarks Dews made at symposia and meetings were preserved in published commentaries, which have the flavor of insightful originality and the use of apt analogies that characterized his impromptu speaking. These writings contrast with the discussion sections of his experimental papers, which never went beyond carefully-worded, logical inferences of the results.

Dews's ability to quickly and wisely discern and clearly express the gist of complex situations made him highly effective on committees at Harvard and nationally. He served on Harvard committees continuously for some 35 years, and nationally on committees relating to mental

health, drug dependence, social behavior, brain research, pharmacology, toxicology, space science, and evaluations of training and research programs. He said that he liked committee work and found it relaxing, taking him away from the rigor of laboratory experiments.

In contrast to his active participation in committees, Dews's habitual manner in conducting laboratory research was solitary. He preferred to do the actual work of some experiments himself, and visiting dignitaries were often surprised to find the professor doing the work of a technician. He never ever spoke about any research that he was conducting independently until it was completed and he had studied the data sufficiently to make some logical conclusions about the results.

For many years, Dews directed a successful National Institute of Mental Health training program for Biological Training in the Behavioral Sciences. He supported collaborations on research in different fields only if the joint research conformed to the accepted standards of each separate field. He believed most research should be conducted with the internal cohesion of a limited context, but that the results of such research took on a greater validity when they could be usefully applied to other areas of research. Understandably, he often cited

the successful use of scheduling procedures from psychology in conducting pharmacological studies and the reciprocal influence from studies with drugs in showing the role of ongoing behavior itself as a psychological principle. He felt strongly that medical students should be taught a rational perspective on behavior and behavioral pharmacology. Starting in the late 1950s, one of the twelve student laboratories in pharmacology at Harvard was on the effects of drugs on behavior.

Dews was elected to the American Academy of Arts and Science and the Institute of Medicine, and was a member of a dozen professional societies (pharmacology, physiology, toxicology, neuroscience, and psychology). He served for 15 years as the director of educational activities for the International Brain Research Organization. He enjoyed the fellowship of professional colleagues and attended many scientific meetings. Traveling for Dews was exploring where tourists never go, using public transportation, getting to know the local culture, and speaking the local language as much as possible. This robust adventurous spirit was also evident in the leisure activities Dews liked best: bicycling, swimming, hiking, and camping with his family.

Dews equaled the behaviorists Watson and Skinner in his disdain for mentalistic and subjective explanations of behavior. Like Skinner, he championed the wider understanding and appreciation of the concept of schedule-controlled behavior, in particular emphasizing in a non-polemic way its importance in other areas of science. After Skinner's death, Dews became the most eloquent advocate for the objective study of behavior as an experimental science and for understanding it in the context of physically defined concepts.

Peter Dews is survived by his wife Grace; daughter Pamela Rentschler; sons, Kenneth, Alan, and Michael; a sister, Jean Hilditch, in England; nine grandchildren, and a great-grandchild.

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## B. F. Skinner Foundation Student Awards

### Student Research Award Winner: APA

The B. F. Skinner Foundation congratulates 2012 APA Div 25 B. F. Skinner Foundation New Researcher Award recipient **Jeffrey Tiger** (below). His research focuses on the frequency and duration of observations required to correspond to students' problem behaviors during the school day.



The Foundation would also like to recognize **Ryan Ward** (below), also recipient of the 2012 APA Div 25 B. F. Skinner New Researcher

Award for his research entitled "Putting the behavior into behavioral neuroscience:



Lessons from an animal model of schizophrenia."

## Student Research Award Winner: FABA



The B. F. Skinner Foundation congratulates FABA student research award winner **Cather-**

**ine Kitts Martinez** (above), whose research was titled "Evaluating the Efficacy of Response Interruption and Redirection (RIRD) Using Empirically-Derived Consequences."

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## International: Interview with Dr. Willy-Tore Mørch: A trip with B. F. Skinner on the coast of Norway in 1982

*Monica Vandbakk, MA, Assistant Professor, Oslo and Akerhus University, Norway*



*Monica Vandbakk*

Willy-Tore Mørch is a Norwegian psychologist working as Professor at the University of Tromsø, at a regional

Center for Child and Youth Mental Health and Child Welfare. His work ranges from rehabilitation of children with infantile autism

to issues related to the legal protection of minors. Mørch has largely been the one who introduced *The Incredible Years Series* by Professor Carolyn Webster Stratton in Norway, which is a preventive and therapeutic intervention for children with conduct problems.

I contacted Dr. Mørch for an interview. Specifically, I wanted to hear him tell about Skinner's trip to Norway in 1982.

***Since I am a Norwegian, I am familiar with your background and most of your work and research. Though you've published widely internationally and had years of cooperation with the well-known Norwegian-American Ivar Løvaas, you're probably not as familiar to all readers of Operants. Please tell us a little bit about yourself:***

I came very early into the field of behavior analysis as a young student at the University of Oslo and started my career as a clinical psychologist, working with youth with mental retardation and autism. After some years, I was involved in a spectacular media discussion about behavioral treatment of a girl with MR after we had published a case study about that treatment in the Scandinavian Journal of Psychology. The case was displayed as an example of the consequences of a positivistic philosophy and the newspaper that

posted the news claimed in court that the treatment was violence against the client. The plea was rejected by the court and we were found not guilty.

It was a fresh start of my career. After some years, I received a Ph.D. in psychology and was appointed as associate professor in child and adolescent mental health at the University of Tromsø in 1994, then full professor in 1997. During the last 20 years, I have worked with young children with conduct problems and implemented program fidelity strategy for *The Incredible Years* (IY) program in Norway, in collaboration with the Ministries of Health and Family, and Children Affairs. For the last two or three years, I have worked closely with the Norwegian Government on revisions of the Child Welfare Act in order to increase the quality of the assessment of neglect and abuse in the child welfare services. The Act was passed by the parliament in May this year.

***As we know Skinner went to Norway two times, the first time in 1951, and then later in 1982. Rumors say that you were the one inviting Skinner to Norway in 1982. Can you tell us how this came about?***

I was at the Institute of Special Education in Oslo at that time. One Saturday evening, the dean of the Institute had a dinner with



my wife and me at my house and after a couple of glasses of wine, it struck us suddenly that we should invite B. F. Skinner to Norway. I called the operator and asked for his phone number in Boston, and immediately I had him on the phone. He was very kind and said that he would prioritize a visit to Norway, and he was aware of that there was a strong behaviorist group in Norway. And after a long planning period, including a visit to his office at the university in Boston, he and his wife arrived in Oslo.

***I heard that you had a little accident on the drive along the fjords on the west coast.***

One person from the group, Arild Karlsen closed the sliding door on the car over Skinner's fingers. He was not harmed but Mr. Karlsen was about to faint.

***What did Skinner do in Norway, and what were some highlights of the trip?***

He gave two plenary presentations, one he called "The shame of American education" and one about the future of behavior analysis. In the first presentation, he criticized the use of punishment and criticism in American schools. In the second presentation, he elaborated the mechanisms of the selection by consequences and the impact of this mechanism on the understanding of the society. The presentations were followed by

a lively plenary discussion. All of it was videotaped.

But the first part of his visit was a round trip in the fjords of the western part of Norway in an American van, where we, among many other things, visited several churches (he was very fascinated about the stave church architecture).



*Heddal stave church, Notodden, the biggest stave church in Norway (Wikipedia)*

***Can you describe what impression you think Skinner had of Norway?***

I am sure that he enjoyed Norway very much. I think he was impressed about the interests and knowledge about behavior analysis as it was displayed by the Norwegian behavior analysts.

***Can you say a few words about the influence Skinner and his work have had on the field of behavior analysis in Norway, as you see it?***

Skinner's impact on the development of behavior analysis in Norway cannot be underestimated. The teaching of behavior analysis at the University of Oslo

was strongly influenced by Skinner's work. A group of "Skinnerians" among the psychology students in Oslo had established "The Skinner Club" and we were very visible at the institute. Both the students and the teachers divided into two groups; the behaviorists and all the others. The others were in majority, but the behaviorists talked loudest.

In the beginning of the 70's, The Norwegian Association of Behavior Analysis (NAFO) was established, an organization that has had an important impact on applied behavior analysis in Norway. NAFO was the biggest behavioral organization in Europe for many years.

***What is your relationship to Skinner's work?***

I regard Skinner's work as fundamental to understanding the mechanisms of behavior. Skinner regarded himself as a descriptive positivist based on Francis Bacon and Ernst Mach's theories. It emphasizes the descriptions of natural phenomena and inductive thinking.

Skinner was therefore the first to admit that future research would change his findings. In that sense, we know more today about human behavior than Skinner did. Other natural sciences, like neuroscience and modern developmental psychology, will supplement behavioral analysis in the understanding of human activities.

***I know that you are very apparent in the media in Norway and a highly respected professional. You collaborate with a number of people within the field of behavior analysis. However, the society does not seem to consider you as a behavior analyst, and you have reached the public with your work in a larger extent than many others from our field.***

***In 1996, E. G. Carr wrote: “We spend much time decrying the fact that society does not listen to us. The real question, however, is why have we not listened to society? We have much to offer. Nonetheless, until we make it clear that we too cherish society’s highest values, speak its language, and are sensitive to its political yearnings, we should expect to be ignored; and we will be...” Do you think your success may be related to this?***

Yes, I do. The behavior analysts in Norway have spent too much time to detach behavior analysis from all other theories, insisting on the necessity to translate Norwegian language into behavioral terms and to disparage common sense explanations of everyday phenomena. This is unwise.

In the end, understanding of behavior analysis is much more powerful when it is applied in integration with other evidence-based knowledge about human activities. By acknowledging that not every human activity can be

understood by well-defined behavioral concepts, it is easier to adopt well-defined concepts from other research fields, e.g. the concept of “attachment.”



*Willy-Tore Mørch*

You don’t need to swallow the complete theory of attachment to use the concept as an important description of caregiver’s attachment responses as strong positive reinforcers for contact behaviors in the child. Regarding caregivers with adequate reactions to the child’s attachment behaviors as a safe base for the child is not incompatible with concepts in behavior analysis.

***What is your impression of the field of behavior analysis in Norway?***

The Norwegian behavior analysts have disseminated applied behavior analysis in a too narrow field of mental health: autism and mental retardation. I think the

reason is that they occupied a field without any established treatment activity. We dumped down in a free chair. We avoided the free space in the half full sofa. This exclusiveness in a small segment of a broader area created a need of defense. The danger is that closed environments develop private ethics. The history has told us that behavior therapy sometimes violates ethical rules developed in a pragmatic society.

***Behavior analysts claim that behavior analysis is extremely effective. Have you had any thoughts on why the number of behavior analysts is quite low? Do you consider yourself a behavior analyst?***

The answer to this question is partly in my answer to the earlier question: the exclusiveness and distance from other sciences. In addition, behavior analysts do not participate in the open debate about important topics in the areas of mental health and special education. They are silent. When they claim that the treatment is extremely effective, it is on the basis of the operational definitions of the concepts. The definition is a description of how to demonstrate the concepts, for example, a positive reinforcer. Therefore it has to work. But the behavior analysts have not been skilled in doing randomized controlled trials to demonstrate the superiority of a certain treatment

strategy, such as language development in autistic children.

In Norway, the research is limited to multiple baseline designs or simple pre-post designs. I know it has exceptions but not strong enough to convince political decision makers. When you are invited to contribute to the development of the mental health policy as a behaviorist, then you will have power.

***What are your thoughts about the future position of behavior analysis?***

Behavior-analytical knowledge will always be an important part of the mental health and school system, but as an integrated part of it, in collaboration with other evidence based knowledge.

***This seems to me a wise suggestion, and hopefully we will manage to do that in the future. I thank Dr. Willy-Tore Mørch for his time and his sharing with us.***

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## **President's Column**

***Dr. Julie S. Vargas, Ph.D.  
President, B. F. Skinner  
Foundation***

Good news. The grant for converting Skinner's books into e-book format that the Foundation submitted to the Behavior Analysis Certification Board was approved. The first books being converted are *Verbal Behavior* and *Contingen-*

*cies of Reinforcement*. We expect that *Verbal Behavior* will be out by the end of this year.

Many thanks to those of you who provided e-book information.

Who would think of checking



*Julie Vargas*

whether the original fonts are copyrighted and require permission for use? That information came from a friend. An-

other informative email was sent by a colleague who had spent quite some time looking for B. F. Skinner e-books. She discovered many of Skinner's books already in Kindle, Nook, and Apple's iTunes formats. For Nook, see <http://www.barnesandnoble.com/c/b.f-skinner>. For Kindle, try <http://www.amazon.com/B.-F.-Skinner/e/B001H6U0RE>, and for Apple formats you can start with *Science and Human Behavior* at <https://itunes.apple.com/us/book/science-and-human-behavior/id51092510>. There's a link to "View More by This Author."

Over the summer the Foundation became aware of problems with our website. The site was written in Apple's iWeb program. Some of the pages displayed incorrectly on Windows machines and on some other devices. With the help of Jennifer Wormald and

Amy Kucharik, the new site is being written in WordPress. That will solve the problem of formats for different devices.

Organization is another problem. When the Foundation receives emails requesting information that is *on* the website already, it is clear that the requested information is not easy to find. Reorganization should help. We hope to have the new site operational by 2014.

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*As always, the editors appreciate your commentary and feedback. We welcome suggestions and news items. Feel free to contact any of us by emailing [info@bfskinner.org](mailto:info@bfskinner.org).*

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*(Spanish)*

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*(Norwegian)*

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*(Italian)*

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*(Chinese)*

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*(Japanese)*

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*(Russian)*

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