Operants

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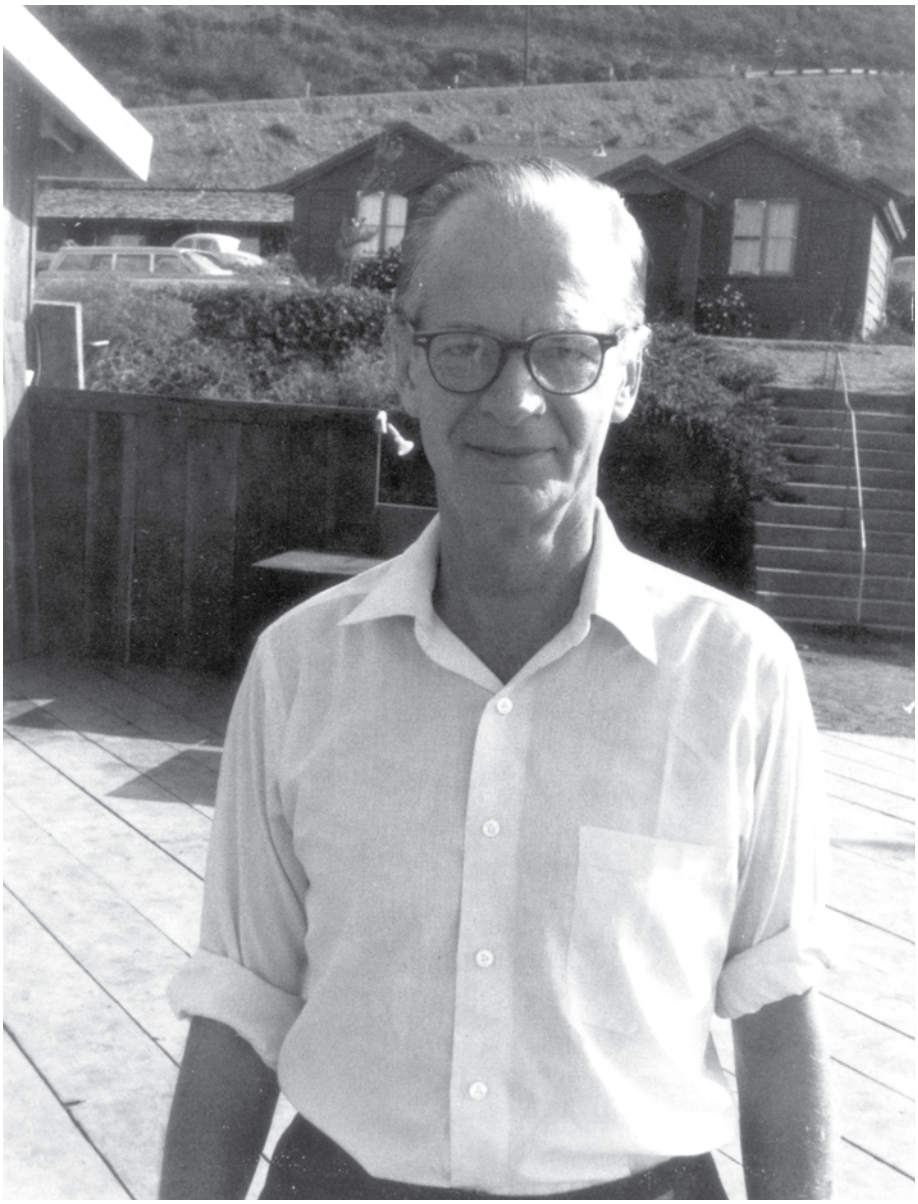


"A small part of the universe is contained within the skin of each of us. There is no reason why it should have any special physical status because it lies within this boundary."

B. F. Skinner

from the president

Twelve years ago, the B. F. Skinner Foundation moved from a tiny space in a university to an office in Cambridge, Massachusetts. We started without any paid personnel except for one student who worked a few hours each week. We didn't even have volunteers except for the Foundation officers and board members. Our work in those days consisted only of reprinting books, in paper editions only. What a difference a dozen years makes! Our reprinting program now includes electronic editions and world-wide accessibility. We have an active website, a quarterly magazine, social media outlets, an archival collection, student awards, and a large international presence. Dedicated volunteers throughout the world make *Operants* possible. Subscribers, too, come from all over the world. Our subscriber list has doubled several years in a row. Many of you may not have seen articles from the early *Operants*. In this issue we have selected features about science, applications, and theoretical issues — all relevant to today's always changing world.

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

Chinese Traditional Translated by Kiwiya Zhang

十二年前，B. F. Skinner Foundation从大学里的小角落搬到了马萨诸塞州剑桥的一间办公室里。我们起步的时候只雇用了一位兼职的学生，其他的人都没有拿报酬。机构干部和董事会成员就是我们的志愿者。那时候我们唯一的工作就是重印纸质书。十二年的变化多大呀！现在我们的重印项目中加入了电子版本，让全世界的人都能读到。我们建立了网站，出版了季度杂志，在社交媒体上发声，有收集，有给学生的奖项，在国际上都有曝光。世界各地的志愿者的力量，让Operants杂志得以延续。我们的订阅者来自世界各地，订阅者的数量在几年内增长了几倍。你们很多人也许没有读过早年出版的文章，所以在这一期中，我们精选了有关科学、应用和理论的文章，都与今天变幻莫测的世界息息相关。

French Translated by MarieCeline Clemenceau

Il y a douze ans, la Fondation B. F. Skinner est passée d'un petit espace dans une université à un bureau à Cambridge, dans le Massachusetts. Nous avons commencé sans aucun personnel rémunéré à l'exception d'un étudiant qui travaillait quelques heures par semaine. Nous n'avions même pas de bénévoles, excepté les membres de la Fondation et les membres du conseil d'administration. À l'époque, notre travail consistait uniquement à réimprimer des livres, uniquement dans des éditions papier. Quelle différence en une douzaine d'années ! Notre programme de réimpression comprend maintenant des éditions électroniques et un accès à travers le monde. Nous avons un site Web actif, un magazine trimestriel, des médias sociaux, une collection d'archives, des bourses d'études et une grande présence internationale. Des bénévoles dévoués dans le monde entier rendent Operants possible. Les abonnés aussi viennent de partout dans le monde. Notre liste d'abonnés a doublé plusieurs années de suite. Beaucoup d'entre vous n'ont peut-être pas vu les premières éditions d'Operants. Dans ce numéro, nous avons sélectionné des articles sur la science, les applications et les problèmes théoriques - tous pertinents pour le monde d'aujourd'hui, en constante évolution.

German Translated by Natalie Werner

Vor zwölf Jahren ist die B.F. Skinner Foundation von einem kleinen Raum in einer Universität in ein Büro in Cambridge, Massachusetts umgezogen. Wir begannen ohne bezahlte MitarbeiterInnen, mit Ausnahme einer studentischen Kraft, die einige Stunden pro Woche arbeitete. Wir hatten nicht einmal ehrenamtliche MitarbeiterInnen, außer den Vorsitzenden und dem Vorstand. Unsere Arbeit bestand damals einzig darin, Bücher nachzudrucken – ausschließlich als Printausgabe. Was für einen Unterschied ein Dutzend Jahre machen! Unser Nachdruckprogram beinhaltet nun digitale Buchversionen und weltweite Verfügbarkeit. Wir haben eine aktive Webseite, ein vierteljährlich erscheinendes Magazin, sind in sozialen Netzwerken präsent, haben eine archivarische Sammlung, Preise für Studenten und große internationale Bekanntheit. Engagierte ehrenamtliche MitarbeiterInnen in der ganzen Welt ermöglichen Operants. Auch unsere Abonnenten kommen aus der ganzen Welt. Die Liste unserer Abonnenten hat sich einige Jahre in Folge verdoppelt. Viele von Ihnen kennen vielleicht die Artikel aus den frühen Ausgaben von Operants nicht. In dieser Ausgabe zeigen wir ausgewählte Beiträge zu Wissenschaft, Anwendungsfeldern und theoretischen Themen – sie alle sind relevant in der heutigen, sich ständig verändernden Welt.

Hebrew Translated by Shiri Ayvazo

לפני 12 שנים הקרן של ב. פ. סקינר עברה ממרחב קטנטן באוניברסיטה למשרד בקיימברידג׳, מסצ׳וסטס. התחלנו ללא עובדים בשכר פרט לסטודנט אחד שעבד מספר שעות שבועיות. אפילו לא היו לנו מתנדבים מלבד פקידי הקרן וחברי ההנהלה. עבודתנו באותם הימים כללה רק את ההדפסה המחודשת של ספרים, במהדורות נייר בלבד. איזה הבדל עושות תריסר שנים! תוכנית ההדפסה המחודשת שלנו כוללת כעת מהדורות אלקטרוניות עם גישה כלל-עולמית. יש לנו אתר אינטרנט פעיל, מגזין רבעוני, במות מדיה חברתית, אוסף ארכיב, מענקי סטודנטים ונוכחות בינלאומית גדולה. מתנדבים מסורים ברחבי העולם הופכים את אופרנס לאפשרי. גם המנויים מצויים בכל רחבי העולם. רשימת המנויים שלנו הוכפלה במשך מספר שנים ברצף. רבים מכם אולי לא ראו מאמרים מהגיליונות הראשונים של אופרנטס. לגיליון זה בחרנו כתבות מרכזיות נבחרות על מדע, יישומים, וסוגיות תיאורטיות ־ כולם רלוונטיים לעולמנו הנוכחי, המשתנה תמידית.

Icelandic Translated by Kristjan Gudmundsson

Fyrir tólf árum flutti Stofnun B. F. Skinners sig úr þröngu húsnæði í háskólanum yfir í skrifstofu í Cambridge í Massachusetts. Við hófum þar störf án nokkura fastráðinna starfsmanna, nema hvað einn nemandi vann fyrir okkur nokkra klukkutíma á viku. Við vorum heldur ekki með sjálfboðaliða, fyrir utan meðlimi stofnunarinnar og stjórnarmenn. Vinna okkar fólst þá að mestu í því að endurprenta bækur, og þá bara í pappírsútgáfu. Hvílíkur munur á rúmum tíu árum! Nú felur kerfi okkar við endurprentun m.a. í sér rafrænar útgáfur og er opin öllum, alls staðar í heiminum. Við erum með virka heimasíðu, tímarit fjórum sinnum á ári, félagsmiðlatengingar, safnadeild, nemendaviðurkenningar og sterka alþjóðlega stöðu. Áhugasamir sjálfboðaliðar út um allan heim gera Operants mögulegt. Áskrifendur koma líka frá öllum heimshornum. Tala áskrifenda hefur nú tvöfaldast nokkur ár í röð. Mörg ykkar hafa ekki séð greinar úr eldri útgáfum af Operants. Í þessari útgáfu höfum við valið greinar um vísindi, hagnýtingu og kenningar – sem er allt mikilægt á síbreytilegum heimi nútímans.

Italian Translated by Anna Luzi

Dodici anni fa, la Fondazione Skinner B. F. si è trasferita da un piccolo locale in una sede universitaria ad un proprio ufficio a Cambridge, Massachusetts. Abbiamo iniziato senza personale retribuito, tranne che per uno studente, che vi lavorava per un paio d'ore a settimana. Non abbiamo nemmeno avuto personale volontario, eccezion fatta per i membri ufficiali del Consiglio di Fondazione. Il nostro lavoro in quei giorni consisteva unicamente nella ristampa di libri in sole edizioni cartacee.

Che differenza da una dozzina di anni fa! Il nostro programma di ristampa ora include edizioni elettroniche e l'accessibilità in tutto il mondo. Abbiamo un sito web attivo, una rivista trimestrale, social media, un fondo archivistico, premi per gli studenti e una grande presenza internazionale. *Operants* è reso possibile da una schiera di volontari dedicati in tutto il mondo. Anche gli abbonati provengono da tutto il mondo. Il nostro elenco abbonati è costantemente raddoppiato negli anni. Molti di loro potrebbero non aver letto gli articoli pubblicati nei primi numeri di *Operants*. Per questo motivo in questo numero abbiamo selezionato alcuni passi sulla Scienza del Comportamento, le sue applicazioni, e alcune questioni teoriche, tutti importanti per il mondo di in costante evoluzione di oggi.

Japanese Translated by Naoki Yamagishi

12年前にB. F. スキナー財団は、ある大学の狭いところからマサチューセッツ州ケンブリッジのとあるオフィスに引っ越しました。週に数時間働く1人の学生以外は有給の職員なしで始めました。また財団事務員と理事会役員以外にボランティアはいませんでした。その頃の私たちの仕事は、「紙版」の本を再版することだけでした。しかしこの12年で大きく変化しました！再版計画はいまや電子版もあり、世界中からアクセス可能です。現在は、活発なウェブサイト、季刊雑誌があり、ソーシャルメディアで発信し、アーカイブを持ち、学生を表彰し、国際的な影響力があります。世界中の献身的なボランティアがいるのでOperants誌が存続しています。購読者も世界中にいます。購読者リストはここ数年連続で2倍に増えています。多くの読者は初期のOperants誌の記事を読んでいません。今回の号では、科学、応用、理論的問題についての特集を選びました。すべての記事は今日の常に変わり続ける世界にあてはまるものです。

Norwegian Translated by Karoline Giæver Helgesen

For tolv år siden flyttet The B.F. Skinner Foundation fra et lite sted på et universitet, til et kontor i Cambridge, Massachusetts. Vi begynte uten betalte medarbeidere utover en student som jobbet noen få timer i uken. Vi hadde ikke engang frivillige utover stiftelsens medlemmer og styremedlemmer. Arbeidet vårt den gang bestod utelukkende av å produsere nye opplag av bøker, og da kun i papirutgaver. For en forskjell et dusin år utgjør! Opptrykksprogrammet vårt inkluderer nå elektroniske utgaver og verdensomspennende tilgjengelighet. Vi har en aktiv nettside, et kvartalsvis magasin, vi er representert på sosiale medier, har arkivsamling, studentpriser og stor internasjonal tilstedeværelse. Dedikerte frivillige verden over gjør Operants mulig. Våre abonnenter er også spredt over hele verden. Listen over abonnenter har doblet seg flere år på rad. Mange av dere har nok ikke sett artiklene fra de tidlige utgavene av Operants. I denne utgaven har vi valgt ut noen saker om vitenskap, anvendelse og teoretiske problemstillinger – alle relevante for dagens kontinuerlig skiftende verdensbilde.

Portuguese Translated by Bruna Colombo dos Santos

Há doze anos atrás, a Fundação B. F. Skinner se mudou de um pequeno espaço na universidade para um escritório em Cambridge, Massachusetts. Nós começamos com nenhum pessoal remunerado, exceto um estudante que trabalhava algumas horas na semana. Nós não tínhamos voluntários exceto pelos funcionários da Fundação e membros da diretoria. Nosso trabalho naqueles dias consistia apenas em reimpressões, apenas em edições de papel. Que diferença doze anos fazem! Nosso programa de reimpressão agora inclui edições eletrônicas e acessibilidade mundial. Nós temos um website ativo, uma revista trimestral, meios de comunicação em mídia social, uma coleção de arquivos, prêmios para estudantes, e uma ampla presença internacional. Dedicados voluntários em todo mundo tornam o Operants possível. Assinantes, também, vem de todas as partes do mundo. Nossa lista de assinantes dobrou vários anos seguidos. Muitos de vocês podem não ter visto artigos do antigo Operants. Nessa edição nós selecionamos características sobre ciência, aplicações e questões teóricas – todas relevantes para mundo de hoje, que está sempre em mudança.

Russian Translated by Konstantin Evdokimov

Двенадцать лет назад Фонд Б. Ф. Скиннера переехал из комнатушки при университете в собственный офис в городе Кембридж, штат Массачусетс. Начинали мы без штатных сотрудников, кроме одной студентки, работавшей по несколько часов в неделю. У нас даже не было волонтеров, если не считать руководство Фонда и членов совета директоров. В то время наша деятельность сводилась только к переизданию книг, и только в бумажном формате. Как все изменилось за дюжину лет! Наша издательская программа теперь включает электронные варианты книг и доступ к ним по всему миру. У нас есть активный вебсайт, ежеквартальный журнал, присутствие в социальных сетях, архивная коллекция, премии для студентов, а также физическое присутствие во многих странах. Преданные своему делу волонтеры по всему миру делают возможным выпуск журнала Operants. Подписчики журнала также находятся во многих странах. Количество подписчиков удваивалось несколько лет подряд. Многие из вас не видели статей из ранних номеров Operants. Мы отобрали статьи о научных исследованиях, их практическом применении, а также по вопросам теории из прошлых лет. Все они актуальны до сих пор, несмотря на постоянно меняющийся мир вокруг нас.

Spanish Translated by Kenneth Madrigal and Gonzalo Fernández

Hace doce años, la fundación B.F. Skinner pasó de estar en un pequeño espacio en una universidad, a estar en una oficina en Cambridge, Massachusetts. Salvo un estudiante, el cual trabajaba algunas horas a la semana, el resto del personal con el que contábamos no recibía remuneración alguna. Ni siquiera teníamos voluntarios, excepto por los miembros de consejo y directivos de la fundación. Nuestro trabajo en esos días consistía simplemente en la reimpresión de libros, exclusivamente de ediciones en papel. ¡Qué diferencia hacen tan sólo doce años! Ahora, nuestro programa de reimpresión incluye sus respectivas versiones digitales, y por supuesto, acceso a ellos desde cualquier parte del mundo. También contamos con una pagina de internet, una revista cuatrimestral, difusión en redes sociales, una colección de publicaciones, reconocimientos para estudiantes y una gran presencia internacion-al. Son los voluntarios alrededor del mundo quienes hacen posible la revista Operants. Nuestros sus-criptores también son de distintas nacionalidades y la lista se ha duplicado por varios años consecu-tivos. Quizá algunos de ustedes no tuvieron la oportunidad de leer las primeras publicaciones de la revista. Por lo cual, hemos seleccionado para éste número artículos enfocados a cuestiones teóricas, ciencia, y aplicaciones; todos ellos relevantes en un mundo en constante cambio, como el de hoy en dia.

Thai Translated by Sirima Na Nakorn

เมื่อ 12 ปีที่แล้ว B F Skinner Foundation ย้ายจากห้องทำงานเล็ก ๆ ในมหาวิทยาลัย มาเป็นออฟิสที่ Cambridge, Massachusetts พวกเราทำงานโดยไม่มีค่าตอบแทน ยกเว้นนักศึกษา 1 คนที่จ้างไว้ทำงานสัปดาห์ละ 2-3 ชั่วโมง เราไม่มีแม้แต่อาสาสมัคร มีแต่เจ้าหน้าที่ของมูลนิธิและสมาชิกกรรมการ งานของพวกเรามีเฉพาะการพิมพ์สำเนาหนังสือ เป็นรูปเล่ม เวลา 12 ปีเปลี่ยนแปลงระบบการออกหนังสือเป็นอย่างมาก เดี๋ยวนี้การพิมพ์ทำในรูปแบบอิเล็กทรอนิกส์ ช่วงเข้าถึงได้จากทั่วต่าง ๆ ทั่วโลก และเรายังมีเวปไซท์ นิตยสารรายไตรมาส สื่อสังคม คอลเลกชั่นจดหมายเหตุ รางวัลสำหรับนักศึกษา กลุ่มสมาชิกในระดับสากล
อาสาสมัครผู้ทุ่มเท อยู่ทั่วโลก ทำให้การออกนิตรสาร Operants เกิดขึ้นได้
สมาชิกนิตรสารอยู่ทุกแห่งทั่วโลก จำนวนสมาชิกก็เพิ่มขึ้นเท่าตัวในหลายปีหลังนี้ หลายท่านอาจไม่เคยอ่านบทความใน Operants ยุคต้น ๆ ในฉบับนี้เราเลือกสรรบทความทางวิทยาศาสตร์ ทั้งภาคปฏิบัติและทฤษฎี ที่เหมาะกับโลกที่เต็มไปด้วยการเปลี่ยนแปลง

in this issue

Operants *is produced by the B. F. Skinner Foundation. The opinions reflected in this* Operants *do not necessarily represent the views of the Foundation.*

We reserve the right to edit all submissions for factual and scientific accuracy, however, as a rule, we preserve the author's grammar and punctuation.

This edition of *Operants* is a collection of previously published articles. The original year of publications appears in parentheses. For more archival articles please visit bfskinner.org/operants.

essay

Learning About John B. Watson. The Story of a Detour

Joe Morrow, PhD

In the second quarter of 2008, Operants published this essay of a then member of the Board of the B. F. Skinner Foundation, Dr. Joe Morrow.

Dr. Morrow is a Licensed Psychologist, a Board Certified Behavior Analyst, and a Professor of Psychology and Behavior Analysis (Emeritus).

As a pioneer in the academic development of Behavior Analysis in California since 1966, Dr. Morrow has published papers on behavioral psychology, behavioral treatment of autism, and has been an active member of the behavioral community for over 50 years.

Dr. Morrow has been involved with legislative issues in California relevant to the practice of ABA and has testified on this behalf to legislative committees.

I noticed it as I passed Greenville, a small sign "Furman University next exit". I was driving south on I-85 on my way to Clemson to see a dear friend and give a talk. As I remembered from when I taught History of Psychology, Furman is where John B Watson did his undergraduate work. The talk I was to deliver "Behaviorism and the Autism Epidemic" had an elaborate PowerPoint slide on Watson. I had prepared to say that that the "root" of Skinner's "radical behaviorism" was the writings of Watson. It was Watson who said: Psychology must be the study of behavior; behavior can be accounted for without recourse to private events; the task of psychology is the prediction and control of behavior; consciousness is a non useful inference.

Skinner had no interest in the psychology of his day. But in the emphasis of psychology as an objective science, Skinner found what he had been looking for and set off to study it. I had the feeling that somehow I had passed hallowed ground and with my Arkansas cultural heritage emerging, I vowed to return and stand there.

I did so two days later. My friend had classes to teach and I had a free day, so I set off for Furman. Greenville is a rather large town for the area and I followed the signs to Furman until they ceased. It was a hostess in an IHOP that wrote out for me the last four turns. In the soft hills a bit beyond the town lies a beautifully manicured campus of modest and barely modern buildings that is Furman.

The students had not returned from spring break and the only life I saw was at the Admissions building. As I entered, a woman behind the counter smiled a greeting that led me to believe that she would be helpful. I had not rehearsed exactly what to say so I stammered out something about John B. Watson, my being a professor, and talking to someone in the Psychology department. She averred that she did not know Professor Watson having been there only a couple of years herself, but that professor, Brewer, who had been there "forever" could possibly help me. She immediately called Dr. Charles Brewer who seemingly picked up on the first ring. "He would be happy to see me" she said and began to give me directions to his office. In mid-direction she stopped and began to walk me to his office some 150 yards away.

As we emerged from the elevator on the second floor there was a display of Watson and several of his documents. She took me down the hall and knocked on Dr. Brewer's office door. He came to the door and instead of inviting me in, guided me to an empty conference room across the hall where we sat down. To the question "what would you like to know", I had no specific answer so we began to talk. He told me that when he first came to Furman in 1967, there was nothing on Watson. The Baptist trustees and "the little old ladies in tennis shoes" at the historical society deemed that the "scandal" (more on this later) precluded Furman from showing any interest in the history of John B. Watson. "I told them that was almost 50 years ago and this man and his history at Furman was important. So I began to do something about it."

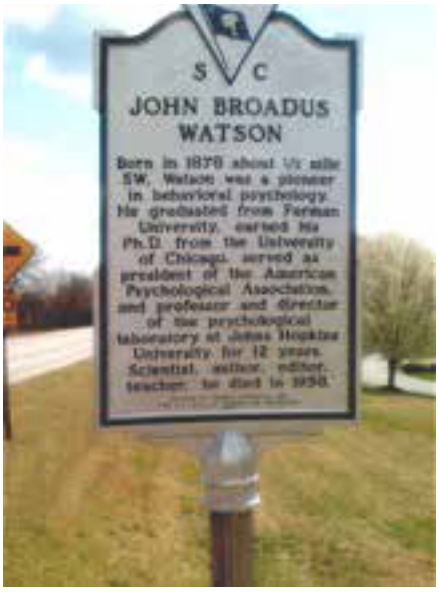
First, there was the display in the hall I had seen. Then an historical sign down the road a couple of miles that proclaimed this to be the birthplace of Dr. Watson. But, most importantly, Charles began the system-

atic accumulation of knowledge about Watson. "I probably know more about John Broadus Watson than anyone in the world" he said as a matter of fact and not at all immodestly.

He told me his office is full of materials, manuscripts, and films that he intended to leave to the University of Chicago (rather than Furman).

Most of our conversation consisted of discussing special events in Watson's life such as the "scandal". I never understood why Johns Hopkins University fired the most prominent psychologist of his day based solely on an affair with a graduate student. While improper, surely it was not that uncommon.

Well, it is a bit more complicated. The student, Rosa-



lie Rayner, came from a prominent Baltimore family. Watson was married to Mary Ickes whose brother Harold was to become a cabinet officer under Roosevelt. Watson himself was prominent in the higher circles of Baltimore. According to Charles, they were all at a dinner one night at the Rayner house. Mary Ickes feigned a migraine headache and asked to lie down. She went to Rosalie's room and instead of lying down began to rummage through it looking for some incriminating material. She found a packet of explicit love letters from Watson to Rosalie.

A divorce ensued and Harold Ickes gave the letters to every newspaper that would take them. "The letters were plastered on the front page of every newspaper in the country," according to Charles. The additional facts that Rosalie refused to go to Europe "until this thing blew over" and Watson's refusal to end the relationship, left the Trustees at Hopkins with little alternative. Charles Brewer in his many talks on Watson had occasion to meet and speak with Mary Cover Jones one of the pioneers in behaviorism. http://en.wikipedia.org/wiki/Mary_Cover_Jones. Rosalie and Mary were roommates at Vassar. They attended a lecture by Dr. Watson where Rosalie was smitten according to Mary and announced she intended "to marry Dr. Watson". Both went on to do graduate work at Hopkins with Watson. Rosalie and Watson did marry.

Charles suggested that Watson was not above an apocryphal story. For example, in a well known account, Watson insists that he flunked a course at Furman because he handed in a term paper "backwards". Yet in examining Watson's actual transcripts, Charles found no F's.

In 1979, to celebrate the Watson Centennial, Charles invited Keller and Skinner, both of whom were recorded, and the tapes sit in Charles's office. Skinner told him that choosing "radical" to describe his behaviorism was a mistake because of the political connotation. Charles and I then both laughed over the term "negative reinforcement". Watson's granddaughter (with Mary Ickes) the actress Mariette Hartley http://www.mariettehartley.com/ was also invited. She declined, not being a fan of Watson. She sent an autographed picture instead.

Another anecdote has to do with the filming of the famous work with "little Albert", forever memorialized on YouTube. (Charles came to know who little Albert was but then lost contact.) There was some question if the people in the film were really Watson and Raynor. So Charles brought in their son who identified them as his parents.

At one point Charles asked if I wanted to see Watson's house. I heartedly agreed thinking I would be given an address. Instead, he said "Well, let's get in the car and go see

it." We drove out a few miles to a well kept and occupied, rather large wood framed house. I took pictures and we went on to Watson's grandfather's house.

I'll end the story here. Those are the highlights. I counted it a good day.

This essay mentiones a recording of Skinner's and Keller's talks at the Watson's centennial celebrations at Furman. Unfortunately, the B. F. Skinner Foundation does not have this recordings in our archives. We are also looking for a talk in French that Skinner gave in Paris. If you have a copy of these and other archival material and would like to donate them to the Foundation to be restored, preserved, and made available to the interested public, please contact operants@bfskinner.org.

science corner

B. F. Skinner's Contingencies of Reinforcement

Commentary by David C. Palmer, Ph.D.
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David C. Palmer studied inter-response times and conditioned reinforcement in pigeons at the University of Massachusetts under John Donahoe in the early 1980s. Upon graduation, he took a job teaching statistics and behavior analysis at Smith College, where he remains today.

His interests in behavior analysis are broad, but his main contributions have all been attempts to extend Skinner's interpretive accounts of human behavior, particularly in the domains of language, memory, problem solving, and private events. He remains convinced that behavioral principles offer an adequate foundation for interpreting such phenomena. Together with John Donahoe, he authored the text, Learning and Complex Behavior, *which was an attempt to justify such optimism.*

This commentary first appeared as foreword to B. F. Skinner, Contingencies of Reinforcement, *B. F. Skinner Foundation, 2013, and was published as a standalone article in* Operants *in 2015.*

Like the early explorers of newly discovered lands, Skinner was a pioneer. With *Walden Two*, *Science and Human Behavior*, *Verbal Behavior*, and his early papers on the technology of teaching, he was the first to lay claim, in the name of science, to the whole panorama of human behavior, formerly mostly uncharted. But like all early explorers, in his push to the hinterlands of the field he had to pass by many alluring vistas, inviting paths, and curious landforms, leaving blank large sections of his chart, to be filled in by those who were sure to follow.

Many did follow, and the science of behavior developed inexorably. But after the publication of *Schedules of Reinforcement* and *Verbal Behavior*, pillars of the empirical and conceptual analyses of behavior respectively, when he might justifiably have settled into a comfortable retirement, Skinner himself was among the first to explore some of those byways and to extend his own earlier accounts. *Contingencies of Reinforcement* is a collection of diverse papers from this period, and it includes some of his sharpest conceptual analyses. He took the occasion of the publication of this anthology in 1969 to survey the domain one more time: he appended commentaries to the papers, and in some cases, his notes are longer than the papers themselves and as incisive. The book, then, is not merely a recapitulation of his earlier work but a refinement, clarification, and extension of it.

As the writer of this foreword I stand as an obstacle between the reader and text, but I hope to pay for the impertinence by alerting him to some of the cogent analyses to be found within. By identifying those that I have found especially helpful in my own attempts to understand complex behavior, perhaps I will whet the reader's appetite for what follows.

The Experimental Analysis/Interpretation Distinction

In early drafts of *Verbal Behavior*, Skinner supported his account with experimental data whenever possible, but he found that most of his analysis went far beyond what could be demonstrated in the laboratory. As the empirical work became increasingly incongruous, he dropped references to it entirely. He characterized the result this way:

> The emphasis is upon an orderly arrangement of well-known facts, in accordance with a formulation of behavior derived from an experimental analysis of a more rigorous sort. The present extension to verbal behavior is thus an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results. (p. 11)

Interpretation, in this sense, has a technical meaning for Skinner. The laboratory is the smithy in which the tools of science are forged and from which its principles emerge, but many natural phenomena are not amenable to experimental control. For Skinner, to interpret the fragmentary and uncontrolled data of everyday experience is to show how

such data might arise by appealing to principles that have emerged from an experimental analysis and to nothing else. An interpretation, then, differs from mere speculation in that it rests upon a set of principles that have been validated in the laboratory. Skinner had begun to articulate this technical sense of the term interpretation in several early papers (see *Cumulative Record*) but his most comprehensive statement awaited his commentary on a paper in the present volume:

> The use of concepts and laws derived from an experimental analysis in the interpretation of daily life is also a source of misunderstanding. An analogy from another science may be helpful. Geophysics interprets the present condition of the accessible parts of the earth in terms of presumed conditions in the mantle and core. It appeals quite freely to physical laws derived from laboratory analyses of matter under various pressures and temperatures, even though it is merely an assumption that comparable states actually prevail in the interior of the earth. In the same way familiar facts about verbal behavior are interpreted with principles derived from the laboratory study of contingencies of reinforcement, even though the contingencies maintained by the verbal environment cannot be precisely ascertained. In both these examples, principles derived from research conducted under the favorable conditions of the laboratory are used to give a plausible account of facts which are not at the moment under experimental control. Neither account can at the present time be proved, but both are to be preferred to treatments which lack the same kind of experimental support. (p. 100)

The distinction between interpretation, which rests solely on established principles, and mere speculation, is not widely understood, nor is it commonly appreciated how much of what we take as scientific fact is an interpretation. Most of the facts of cosmology, evolutionary biology, and geology, as well as virtually all of our explanations of everyday physical events, are interpretations, not the result of experimental analyses. Interpretation, then, is not a tawdry sideshow of science but an indispensable part of the main feature.

Evolution and Behavior

Darwin's exposition of evolution by natural selection is a parsimonious and powerful interpretation of adaptive complexity in nature. Repeated cycles of variation and selection adequately explain the origins of the myriad life forms that we see in nature. In addition, if behavioral contingencies are relatively constant over generations, adaptive behavior can be selected by contingencies of survival as well. In *Science and Human Behavior*, Skinner remarked on the evolutionary origin of reflexes as well as the adaptive significance of the principle of reinforcement and other behavioral principles:

> The process of conditioning also has survival value. Since the environment changes from generation to generation, particularly the external rather than the internal environment, appropriate reflex responses cannot always develop as inherited mechanisms. Thus an organism may be prepared to secrete saliva when certain chemical substances stimulate its mouth, but it cannot gain the added advantage of salivating before food is actually tasted unless the physical appearance of foodstuffs remains the same from environment to environment and from time to time. Since nature cannot foresee, so to speak, that an object with a particular appearance will be edible, the evolutionary process can only provide a mechanism by which the individual will acquire responses to particular features of a given environment after they have been encountered. Where inherited behavior leaves off, the inherited modifiability of the process of conditioning takes over. (p. 55)

Moreover, he pointed out the analogy between the processes of evolution by natural selection and the shaping of novel behavior by reinforcement:

> In certain respects operant reinforcement resembles the natural selection of evolutionary theory. Just as genetic characteristics which arise as mutations are selected or discarded by their consequences, so novel forms of behavior are selected or discarded through reinforcement. (p. 430)

In *Walden Two*, Skinner, speaking through Frazier, acknowledged the role of genetic variation in human behavior: "Our ten-year-olds have all had the same environment since birth, but the range of their IQ's is almost as great as in the population at large. This seems to be true of other abilities and skills as well." As *Walden Two* is a novel, this statement is merely a prediction, but it shows where Skinner stood on the topic.

Nevertheless, Skinner's critics, and even some of his colleagues, assumed that he neglected or ignored the role of evolution in behavior. No doubt this assumption arose from several circumstances: First his interpretations of human behavior invoked principles that had been derived almost exclusively from experiments with rats and pigeons. Second, the remarkable speed with which he was able to shape novel and arbitrary behavior in pigeons and other species suggested that such shaping was limited only by an animal's morphological features. Third, he studied arbitrary behavior in laboratory settings, rather than behavior typical of various species in their natural environments. Finally, and most importantly, the public, along with many scientists, are determined to believe that someone, somewhere, believes that the newborn infant is a tabula rasa. Not even Locke, who coined the term, thought that inheritance was irrelevant, and certainly Watson believed no such thing. But Skinner inherited from Watson the token role of extreme environmentalist assigned by popular prejudice to anyone who suggests that human behavior can be improved.

The *Phylogeny and Ontogeny of Behavior* was originally published in *Science* in 1966 and should have eliminated such misconceptions, for it is an explicit acknowledgement and comprehensive discussion of the contributions of both the ontogenetic and phylogenetic environments to the behavior of organisms. Many types of contingencies are

common to both environments, but some differ, and Skinner carefully discusses both cases. Moreover, he cites, with approval, the work of the Brelands, who showed that under some conditions behavior of phylogenic origin can interfere with shaping by reinforcement. Skinner's critics crowed with delight at these findings, but Skinner found the Brelands' conclusions to be "plausible, and not disturbing." He calmly noted that intrusions can happen in both directions. Indeed they can; he cited examples of behavior shaped in the laboratory that interfered with normal eating, and examples in human affairs abound, from the use of contraceptives and vows of chastity to hunger strikes and suicide bombings. Skinner was a consistent advocate for a thoroughgoing science of behavior, not a partisan trying to exaggerate the role of one aspect of the science as opposed to another:

> Early behaviorists, impressed by the importance of newly discovered environmental variables, found it particularly reinforcing to explain what appeared to be an instinct by showing that it could have been learned, just as ethologists have found it reinforcing to show that behavior attributed to the environment is still exhibited when environmental variables have been ruled out. The important issue is empirical: what are the relevant variables? (p. 199)

Of course, some of Skinner's proposals, presented elsewhere, were controversial and occasioned furious objections, for he dared to propose that the shape of the future is in our own hands. Whatever the genetic cards we have been dealt, we can play them much more skillfully and to better effect than we have hitherto. We cannot change the contingencies of selection that have shaped our genome, and our ability to engineer genetic change is still limited, but the scope of behavioral engineering is vast. That such an unremarkable observation should have engendered so much controversy and emotion is a testimony to the enduring influence of dualism in human affairs.

Rule-Governed Behavior vs. Contingency-Shaped Behavior

A major appeal of Darwin's theory is its power and scope. A computer program that simulates variation and selection of sequences of DNA bases can, at least in principle, generate the genome of every organism that has ever existed, as well has an indefinite number of other beasts that never have existed and never will. Thus, in principle, the explanatory adequacy of Darwin's theory is profound. Such a demonstration does not prove that the theory accounts for the origin of species in fact, but it shows that it is sufficiently powerful to do so, given the requisite variations and history of selection. An analogous conclusion can be drawn about behavior. Shaping generates novel behavior through the systematic reinforcement of variations along one or more dimensions. A computer program that simulates variation and selection of behavior can, in principle, generate any imaginable topography and sequence of behavior within the limits of the program. Since the potential scope of such as system is indefinitely large, contingencies of reinforcement are plausible candidates as explanations of complex behavior.

However, it is easy to show that much human behavior is not shaped through the reinforcement of successive approximations. Rather, complex adaptive behavior often occurs in its terminal form on its first occasion. If we need to drive to Boston, we do not wander aimlessly, getting successively closer with each excursion. We consult a map, ask a passerby, or follow the commands of an electronic navigator, and arrive at our destination, possibly without having taken a single false turn. When telephoning a plumber, we do not enter numbers at random; we look up the number and enter it correctly at once. If all behavior were shaped through the reinforcement of successive approximations to a target behavior, the process would be conspicuous; everyone would be a behaviorist. The abrupt appearance of adaptive behavior, common in humans but rare in other species, fuels speculation that human behavior requires special treatment, that it cannot be explained with the interpretive tools that have emerged from the animal laboratory.

In *Operant Behavior* (Chapter 5) and *An Operant Analysis of Problem Solving* (Chapter 6), Skinner introduced the concept of rule-governed behavior and contrasted it with contingency-shaped behavior. In verbal communities children quickly learn to respond systematically to verbal stimuli: "Turn left," "Look up," "Raise your hand," "Take one giant step forward." Given a sufficiently finegrained "alphabet" of responses under verbal control, virtually any topography and sequence of behavior can be evoked at once through the arrangement of corresponding verbal stimuli: "Take your first left, go two blocks, turn left at the church, then bear right onto Livingston Avenue." "Mix together one beaten egg, one cup milk, one tablespoon melted butter, two cups flour, one-half teaspoon salt, two tablespoons sugar, and two teaspoons baking soda." Initially the reinforcement for such behavior is generalized social reinforcement, but the behavior evoked by verbal stimuli can, and usually does, satisfy a second contingency as well: We arrive at our destination, and the pancakes are edible. A single topography of behavior satisfies both contingencies; it is evoked by the verbal contingency, but control transfers to the second contingency, which may be verbal or non-verbal, and in the future behavior will occur upon the appropriate occasion in the absence of the verbal stimuli.

The effect, of course, is dramatic. In a verbal community, when novel behavior is shaped in one person through cycles of variation and differential reinforcement, possibly over a long time, a second person can emit the terminal behavior through verbal instruction on the first opportunity. That is, the contingency-shaped behavior of one person can become the source of control for the verbally governed behavior of myriad others. Thus effective behavior can spread rapidly throughout a culture and across generations. Thus, Skinner's conception of rule-governed and contingency-shaped behavior was an important advance, for it accommodated many facts about human behavior that would otherwise have been difficult, if not impossible, to explain.

Radical Behaviorism and Competing Paradigms

The term radical behaviorism had been used in the

1920s by various writers to characterize the work of Watson and his supporters, and in this usage, radical was an adjective. That is, it was intended as a descriptive term, picturesque with a touch of the pejorative. Skinner appears to have been the first person to embrace the term to represent his own position. Although he occasionally wrote of a radical behaviorism, meaning fundamental or thoroughgoing, and certainly with no pejorative connotation, he generally used the term as a compound noun: radical behaviorism was a label representing a particular coherent set of assumptions and tenets about the science of behavior, distinct from the position he dubbed methodological behaviorism. He first used the term in print in *The Operational Analysis of Psychological Terms*, a paper that emerged from his early analyses of verbal behavior, but his unpublished notes reveal that he had been using the term for more than a decade. Noting the lapse of a half-century since Watson first raised objections to mentalism and three decades since the topic had been much discussed, Skinner wrote *Behaviorism at Fifty* as a restatement of radical behaviorism.

In several works, but particularly in *Science and Human Behavior*, Skinner discussed the status of private events in a science of behavior, but as the central role of interpretation in science was poorly grasped by others, his exposition was not influential outside his field. *Behaviorism at Fifty* was a renewed attempt to make his case. At the heart of the paper is an elegant analogy:

> Science often talks about things it cannot see or measure. When a man tosses a penny into the air, it must be assumed that he tosses the earth beneath him downward. It is quite out of the question to see or measure the effect on the earth, but the effect must be assumed for the sake of a consistent account. (p. 228)

Likewise, we must assume that a principle of behavior continues to operate in those instances that do not lend themselves to experimental manipulation. Radical behaviorism takes the position that behavioral principles apply to all behavior, public and private, observed and unobserved. Experimental analysis must confine itself to observable and manipulable events, but the scope of the principles derived from an experimental analysis includes all behavioral events.

The ramifications of this assumption differentiate radical behaviorism from competing paradigms. Human behavior is difficult to control experimentally for at least two reasons: First, much relevant behavior is difficult to measure with our current tools; that is, it is covert. Second, an experimenter has limited control over the histories of human subjects. But these problems do not go away by fleeing to another paradigm. When studying human behavior, all scientists, whatever their theoretical orientation, face the same ethical constraints and must accommodate the fleeting, subtle, and multidimensional nature of the subject matter. However, the behaviorist has an important advantage: He has a set of analytical tools that have been validated in the laboratory and are therefore empirical, not hypothetical:

> Unlike hypotheses, theories, and models, together with the statistical manipulations of data which support them, a smooth curve showing a change in probability of a response as a function of a controlled variable is a fact in the bag, and there is no need to worry about it as one goes in search of others. (p. 84)

It is the "fact in the bag" more than anything else that sets the behaviorist apart from those who subscribe to competing paradigms. The inductive principles that have emerged from the behavioral laboratory are not hypothetical. Any interpretations of the available data of human behavior that rest only on such principles are therefore superior to, not just different from, those that rest on hypothetical constructs. The facts in the bag are available to everyone, of every persuasion, and they don't go away when they are ignored. But at the time the papers in this volume (*Contingencies of Reinforcement*) were written, the social sciences were rapidly expanding and differentiating into subordinate disciplines whose advocates were commonly critical of Skinner and his science. As a result of this adversarial stance, they made the colossal error of ignoring the facts that had been uncovered by the experimental analysis of behavior. One looks in vain through textbooks in cognition, cognitive neuroscience, psycholinguistics, linguistics, philosophy of science, and related disciplines for discussions of reinforcement, extinction, stimulus control, and other behavioral principles. Contingencies of reinforcement are ubiquitous in human affairs. To overlook them is careless, but to deliberately dismiss them from consideration is foolish. The facts are in the bag, and turning one's back will not set them loose again.

Contingencies of reinforcement are ubiquitous in human affairs. To overlook them is careless, but to deliberately dismiss them from consideration is foolish.

reflections

A Daughter's Retrospective

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Julie S. Vargas is president of the B. F. Skinner Foundation. She began her professional life as an elementary school teacher, and has kept her interest in public education from that time on. After receiving her doctorate, she taught at West Virginia University, working with practicing teachers and with undergraduate education majors. Her publications include Behavior Analysis for Effective Teaching. *(2nd Ed. Routledge, 2013). She is currently working on the life and historical context of the works of her father, B. F. Skinner.*

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My father, B. F. Skinner, was born in Susquehanna a small conservative town nestled in the wooded hills of Pennsylvania. His father was a lawyer with a modest practice. His mother was a housewife. He went to the local public schools and graduated second in his class from high school. From these rather modest beginnings, he went on to establish a new science of behavior, different from the S-R psychology of Pavlov and Watson, and different from the "trial and error" analysis of Thorndike. Many people wonder what my father was like as a person and how he became so revolutionary. For not only did he discover the impact of contingencies on behavior, he also extended his analysis to epistemology, education, and cultural design.

Where does behavior come from? Obviously genetics contributes a large part. But the instant a child is born his or her interaction within the immediate setting begins to shape that child's repertoire. Parenting style is a large part of the initial interaction. At one extreme of parenting, a child is given much verbal instruction such as rules about how to behave. At the other end of the continuum a child interacts with nature and others without much adult supervision or guidance. My father's mother, who stayed at home once married, was the main influence in his early life. A clue to her mothering style is found in the "baby book" she kept on my father. One entry says, "Pulled himself up by a chair alone! fourteen months 2 days." Under that is written, "Walked alone July 20th, 1905. Sixteen months old." It took my father two months to take a first step after standing! Clearly his mother did not hurry his walking. Freedom in physical matters seems to have been consistent in his childhood. In his autobiography he mentions roaming the hills without restriction and extreme frustration at trying to twist a screw into an oak plank, not having been told to first drill a hole.

On the other hand, his mother was strict in social matters. Her usual sanction was to say "What will people think?" The two aspects of her child rearing are shown in a story my father reported in his autobiography to solve his forgetting to hang up his pajamas. At breakfast his mother would check. If he had left his pajamas on the bed or floor, he would "have to stop eating, go upstairs, and hang them up." Finally, my father rigged up a gadget to solve his problem. He described the contraption as follows:

> The clothes closet in my room was near the door, and in it I fastened a hook on the end of a string which passed over a nail and along the wall to a nail above the center of the door. A sign reading, "Hang up your pajamas" hung at the other end. When the pajamas were in place, the sign was up out of the way, but when I took them off the hook at night, the sign dropped to the middle of the door where I would bump into it on my way out.

I contrast this with my own upbringing: No one made me hang up my pajamas before breakfast, but I am not sure my mother would have permitted me to drive nails into the door frame and closet area of my bedroom.

By the time he was in elementary school, my father's first love was building things. Among the gadgets he made as a youngster are the

following:

- A small reading room with shelves and candle bracket
- Willow whistles, benches, and tables
- Miniature theater with cutout figures, strings and pulleys to open the curtain
- Small houses in backyard and a cabin in the woods
- Slides, teeter-totters, and merry-go-rounds from old lumber
- Scooters from wheels of old roller skates and steerable carts
- Pea shooter, bows and arrows, slingshots
- Steam cannon that would shoot plugs of carrot
- Elaborate hydraulic (water) systems with tubing in grandfather's garage
- Loom that he used to weave mats
- Sprinkler for cleaning floors at the shoe store where he worked
- Gadget to separate ripe elderberries from green ones
- Perpetual motion machine!!! (This one was unsuccessful.)

My father developed an independence both in design and in construction that was to be critical to his discovery years later. I remember him as a Mr. Fix-it. My mother told me that as early as two years old if anything broke, I look up at her with a big smile and say, "Daddy fix it". And he would. His gadgets and repairs were not usually very attractive, being made out of old scraps of wood, pieces of metal, coat hangers, or other debris kept in the shop of whatever house we were living in, but they worked.

Evidence of his "fixes" can still be seen in his last home. An antenna for a small TV is a bent piece of coat hanger. A light switch to enable you to turn off the light at the top of the basement stairs from the bottom consists of a shoehorn attached to nylon fishing line you pull to turn the light off, and a circular ring of metal to pull to turn the light back on. Anyone else would have hired an electrician to put in a downstairs switch. Not my father. Instead he rigged up the nylon lines to operate the upstairs switch from the bottom of the stairs.

In high school my father was not part of any fixed social group. He moved between an Erie Railroad band of mostly older men, a tennis friendship with a Catholic (when his parents were Protestants), friends for exploring, and a teenage dance band he organized. Thus be avoided the strict controls that teenage peers can impose. His independence continued with a canoe trip with four other boys down the Susquehanna River from his hometown to Harrisburg, a distance of over 200 kilometers. With another friend he built a cabin in the woods complete with glass windows. His mother's social control followed him even there. "We did not smoke cigarettes," he wrote, "because we were forbidden to do so, but we smoked corn silk and certain kinds of dried leaves".

In high school he encountered Francis Bacon's works. His teacher, Miss Graves, to whom he later dedicated *The Technology of Teaching*, had her students read Shakespeare's *As You Like It*. My father's father, probably over the family dinner, had mentioned the theory that Francis Bacon, not Shakespeare, had written that play. My father announced this in class and was roundly admonished. But he did not give up so easily. He went to the library and read everything

Miss Graves

he could by Bacon. No doubt his new revelations kept that English class interesting, but it is hard to imagine his continued reading just for that discussion. Something in Bacon must have appealed to him. Bacon's insistence that truth was to be found inductively and not through authority would have sounded good to a teenager who loved exploring and tinkering, and who had challenged his teacher. In any case he was to quote "Nature to be commanded must be obeyed" many times in print, and at home.

College furthered his independence in a backhanded way. My father, though very coordinated, had not practiced any sport enough to make a college team. He also did not know about the importance of picking the "right" fraternity and accepted the first request he received, one at the bottom of the social ladder. Thus his first year he did not become part of any one social group. His second year he became an editor of the student publication *The Royal Gaboon*, and through a tutoring job was invited into the home of the chemistry professor, Percy Saunders, for evenings of chamber music and conversations with well known liberal writers. The Saunderses life appealed to him more than the life he had left back home.

Writing seemed a good career. Encouraged by a letter from Robert Frost, commenting on a story he had sent the poet, my father returned to his parent's home to write the great American novel. He was not successful. Though his parents did not say much, their concern and disapproval must have shown daily in their expressions. Then, too, his new liberal views contrasted with the conservativeness of his parents and their friends. Finally his father gave him a job abstracting legal briefs. That completed, he escaped to New York and got a job in a bookstore. Although he enjoyed the bohemian life, it did not seem a good way to "make something of himself" and he applied to graduate school.

In graduate school at Harvard University, many of

my father's strengths came together to enable him to make the discoveries that began a whole new science. As before, he moved between social groups. Although he became friends with the more radical Watsonian students in psychology, his work was guided, at least initially, by William Crozier, the young chair of the new department of physiology. Crozier was a student of Jacques Loeb and expressed acerbic views against the mentalism of Titchener espoused by the chair of psychology, Edwin Boring. After my father's first year of courses, he was essentially on his own to conduct experiments. Here his independence and tinkering skills came into play. Where others might have used standard equipment or followed a professor's agenda, my father worked independently of such constraints. He loved to work alone and to fix things mechanically. He was quick to toss out equipment

Kelelr and Skinner, 1938

he had spent hours constructing when he had a better idea of an experimental procedure to try. The result was that the main control over his experimental behavior was the behavior of the organisms he was studying, exactly the right contingencies for discovery. It took over a dozen major pieces of equipment and two and a half years of intense research before he found that the probability of his rats' actions was controlled not by an antecedent stimulus as he had initially thought, but by the immediate postcedent stimulus. This was, indeed, as he wrote to his friend Fred Keller, "a brand new theory of learning".

Finally the small town boy from Pennsylvania had become a success. He was awarded a prestigious fellowship to continue his operant research. He varied every aspect of contingencies he could think of, with gratifying results. When the fellowship was over he found a job at the University of Minnesota. And he found a wife.

That is where I come in. The book about my father's

Fred and Eve Skinner, 1936

operant research, *The Behavior of Organisms*, came out in 1938, and I was born the same year. According to my mother, my father, now an expert at behavioral control, discussed child's raising with her. His lifelong fight against punishment of children must have been part of those discussions. My parents had a low coffee table with some attractive knick-knacks on it. At first, like many parents, when I reached for things I was not to touch, my parents gave my hands a little slap. But reaching did not decrease and my father, remembering experiments that showed slaps to only temporarily suppress behavior, suggested never punishing my behavior again. My mother readily agreed. Many years later I heard my father talk about a similar situation. He said something like, "You have a low coffee table with things on it. They are designed to be attractive and they will attract a young child. Instead of punishing the baby reaching for them, simply put them on a higher shelf, out of reach. The whole idea is to design contingencies to encourage the behavior you want and to eliminate situations that produce behavior you don't want." The knick-knacks in my parents' home were moved from the

coffee table to a high shelf.

Although I do not remember the coffee table incident, I do remember allergy testing. I must have been around two years old. In those days the method of testing for allergies was to lay the patient on a table face down and scratch little bits of potent allergens in rows on the patient's bare back. They must have tested a hundred substances on me. I remember squeezing my father's hand and wincing every time a new scratch was made. Needless to say, I did not like going to the hospital. My father mentioned this to the physician who suggested telling me next time that we were going for ice cream. My father was horrified. Instead, for the return trip he told me exactly where we were going. Unwilling to drag me into the hospital, he watched paiently as I walked away from the door, around the grass, and finally into the building.

When I was five years old, my mother was pregnant again. She asked my father whether he could make a better crib than the one I had used. As a baby I had worried her when my bed clothes ended up over my head. My father loved to build "equipment" so he happily set to work. The result was the baby tender, an enclosed crib that was heated enough so no sheets or blankets were needed. Pleased with

Deborah Skinner, 1945

his new invention, my father wrote an article for the Ladies Home Journal. The editor changed his title to "Baby in a Box" thus beginning the confusion between the experimental chamber that everyone but my father called the "Skinner box", and the baby tender. In fact, the new crib was used like other cribs, for sleeping. My sister had a playpen like other babies of the l940's.

By this time we had moved to Indiana where my father had become chair of the Psychology Department. This was a barren time for research, but he used his administrative position to gather operant researchers together, resulting in the organization from which the Society for the Experimental Analysis of Behavior came. The difference had never been clearer between a mainstream psychology that looked inside the behaving organism for causes, and an operant analysis that looked for causes in the interaction between actions and their surrounding contingencies.

My father loved children, especially his own. He spent much time with my sister and me as we were growing up. Perhaps because of his own frustrations in learning to use tools as a child, he taught my sister and me to use hand tools. We were shown how to drill a hole before putting in a screw. Both Deborah and I had our own hand tools as young children, and my father even built a separate workbench for me in our summer cottage when I was around 13. It had a vise and a set of Craftsman tools mail-ordered from Sears.

In I947 Edwin Boring, the same professor whose Titchnerian position my father had opposed as a graduate student, invited my father to give the William James Lectures. It seemed to be a way to look over prospective candidates, because at the conclusion of that fall, my father was offered a professorship in the Psychology Department. He accepted and we moved to Cambridge. After two years, my parents built the house they lived in for the rest of their lives. This house was less than a kilometer of my sister's and my elementary school and only two kilometers from my father's office. Every morning we set out together on the few blocks common to our routes. At the end of the day we again had some time with our father. Most evenings, it was our father that put us to bed. reading stories or just talking. Deborah being the younger, was put to bed first. Then he'd come to my bedroom. Both of us developed strategies to keep him talking a little bit longer. Mine was to ask a question about science. The only one that he didn't answer to my satisfaction was "What is beyond space?" As a holding technique this was quite effective, because my father turned on the light, made a mobius strip and had me draw a line, showing how the surface turned back on itself so that, without crossing over an edge, your line covered both sides. "Space," he explained, ,"is like that, but in three dimensions."

One of my father's favorite activities was taking walks. Our house was near a "garden cemetery" that had wooded hills and ponds. Often my father would take me or Deborah on a walk there. In the other direction, a four kilo-

meter path through woods and meadow circled the Cambridge reservoir. Walks around the reservoir took longer. My father used the longer walks to explain material he was working on for a book or article. Years later, when I read my father's publications, I recognized discussions, like those of the "homunculus" or "operationism" from conversations during those walks.

As much time as our father gave us during the

academic year, we had even more of his attention during our summers on Monhegan Island. The island is a small island an hour's boat ride off the coast of Maine. As my father's parents had done, he and my mother gave us complete freedom about where on the island we could go. The only rule was to be back by dinnertime. My sister and I explored the high cliffs on the backside of the island, discovered blackberries on little used trails, and generally went all over the island. The freedom we were given extended to the water. My mother preferred the land to the water, but my father loved to be out on the water. He bought me a Folbot, a rubberized kayak-shaped boat with lee boards and a lateen-rigged sail. It had a tiller that stuck out into the rear sitting space. Always on the lookout for improvements, my father replaced the tiller with a pulley system. Instead of holding your arm out in front of you, you could rest your arm on the side of the boat, moving a cord that ran around the sitting space to move the rudder. My sister had a boat, too. He built her (letting her help, of course) a flat bowed rowboat. Neither of our boats held more than two people, so my father would accompany me sailing, or Deborah rowing and fishing. But often we went out by ourselves.

We kept both boats on Fisherman's Beach, high enough on the beach to escape the high tides of that northern latitude. I could not get my boat down to the water by myself. So my father solved the problem by building a carrier to help. He made a cradle for the bow that rested on two large wheels. By lifting the stern I could roll the boat down to the water's edge or push it back up to its resting place. Many days I went out sailing accompanied only by my dog or my guitar. Like the stipulations about land, the only rule I had about where I could go was to be back by dinner.

From a World War Two surplus catalog, my father bought a steerable kite with a large picture of a Nazi plane on it designed for target practice off of navy warships. Many days my father and I would launch the kite and steer it back and forth in front of our house. One day my father thought the kite might be used to power my sailboat. The problem with a sailboat is that when the wind blows, the boat tips, so that much wind is spilled out of the sail. My father thought, "Why not pull the boat with our steerable kite?" Somewhere he researched this idea and discovered that the ancient Greeks had tried it, but the fact that they had not adopted it did not discourage him. So one day he got the kite up in the air from the beach and set out in the kayak. Fortunately, he had stowed the paddles that came with the boat, because although the kite steered beautifully, the strings curved around in a beautiful arc, so that the kite pulled only in one direction.

There were few organized activities on Monhegan Island. It was up to our family to create things to do. My father was often given things by the fishermen or by the summer artists and he inevitably found something to make of them. One day he came home carrying two enormous pulleys. Soon a "tightrope" appeared. It was only a foot off of the ground. but still offered a challenge. Using old broomsticks for balance and keeping our eyes straight ahead as advised by our father, Deborah and I tried getting across the four-meter stretch. Neither of us was successful and we soon gave up. The next day, the pulleys appeared in a new role. They supported what we came to call the "trolley". One pulley was attached 3 meters high up a tree at the top of the hill near our house. Another pulley was attached to a tree 20 meters away down over the hill, pulling a rope between the two pulleys taut. Along the rope a third pulley rode with a bar attached. You would start at the top of the hill, hold on to the bar and ride to the bottom, keeping your feet up as high as you could to avoid hitting the ground. This use of the pulleys was successful. We used the "trolley" quite often.

In 1953 my sister was in fourth grade. Visiting her mathematics class one day with other fathers, my father became agitated. Teaching was supposed to be going on, but almost none of what twenty years of research had showed was needed for successful shaping was being used. It was not the teacher's fault. No one person could possibly do what was required for each child. Teachers needed help, and my father had just the solution. As usual, it was a mechanical gadget. That same afternoon he designed the prototype of the first teaching machine. Unlike the worksheets it was designed to replace, it gave feedback following each answer, and it adjusted what the child got next according to performance. Several models of this teaching machine were made and tried out in schools. But it was not until a sabbatical in 1955 that my father tackled the shaping of new skills. He took the sabbatical to finish his book *Verbal Behavior*. To get away from distractions, he found a little inn near the school in Vermont I attended. He took Deborah with him and she stayed downstairs with the family that ran the inn. While working on his book, he realized that a teaching machine could not only provide practice on skills presumably already taught, but with careful design, material could be presented step by step to shape new skills. He tried out some of his instructional programming with Deborah to great success. Now a new kind of teaching machine would be required. When his sabbatical was over, he designed this new machine and got a small grant to use them to teach his own course.

The students' responses were analyzed for revisions

and the project was a success. Meanwhile, my father found working with big business terribly frustrating, and he gave up work in education. But the results of his work can be seen today not only in computer-assisted-instruction, but also in the emphasis on performance objectives, the use of reinforcement instead of punishment, and the increased use of frequent (if not immediate) feedback both to students and to teachers on progress rather than on final performance.

After leaving the field of education, my father turned to society at large. All of the major problems of the time, – overpopulation, the depletion of resources, pollution, and war – involve human actions. In *Beyond Freedom and Dignity* (Skinner, 1971/2002). my father argued that as a species we have solved physical problems with physical science and that for behavioral problems we need behavioral science. In particular, we need to give up the fiction that our behavior is free, which leads to blaming individuals, and instead to design better the contingencies that actually control what people do. *Time*, a prominent weekly magazine, came out with a picture of my father on the cover under the headline "Skinner says we can't afford freedom." Suddenly my father was famous. Or infamous. Many reviewers attacked *Beyond Freedom and Dignity* with a vehemence that surprised my father, even though he was used to criticism. He pasted the reviews, both good and bad, into a large scrapbook, and tossed it into the back of the basement where it collected spiders along with the boxes of old notes, honorary degrees, and old suitcases. He was now 66 years old.

His new prominence produced many invitations to speak. Too many. As long as I can remember, Deborah and I would encourage our father to turn down talks and consulting invitations, and gradually he turned down more

and more. But he rarely turned down requests from former students and never, to my knowledge, an honorary degree. "Resignation" describes how he approached most trips. The commitments he accepted had a way of getting out of hand. He would be invited to give a short talk. Usually he could use something he was working on or planning to write, so that part of the commitment wasn't a problem and he would accept. Then the schedule would come: Since he was going to be on campus, would he visit Professor So and So's class from 9:00 to 10:00? Some students had arranged morning coffee from 10:30 to ll:00. Luncheon would be at the Alumni center with the Dean and a few Department Chairs. At 2:00 the local Public Radio would like an interview. At 5:00 cocktails and a dinner with department members was planned, with his talk at 8:00 and a short reception afterwards. That was the official schedule. In addition to that, former students, students working in his field, student reporters, autograph seekers, and various others would call to arrange time to talk. Once accepting a talk my father never learned to say "no" to all the rest. So when an Honorary degree request would come, with the suggestion that perhaps he might "say just a few words" no one in the family was under any illusions about the level of activity expected. Yet when Mother or Deborah or I would say, "Oh yes, just a few words. Oh, and while you're at it, could we have the article? And how about a few informal talks, or a couple of dinners where you could make a few comments...," our father would say, "Oh, you can't turn down an honorary degree!" I don't think my father accepted these invitations with the kind of noblesse oblige that prompted him to accept even inconvenient invitations from former advisees and colleagues. Rather, I think he liked getting honorary degrees. They were proof he had achieved the social status his mother was so concerned about when she asked, "What will people think?"

From his family, my father carried the ethic of leaving the world a better place than you found it and of self-improvement. As long as I can remember, he worked on his French. He bought the Goncourt journals, but found them too difficult, so he settled on detective stories, and read Simenon's Maigret series in French. Even in his last year, diagnosed with leukemia, he practiced his French pronunciation with the French-speaking secretary that came to the house.

When my father was diagnosed with leukemia, I took a leave of absence to be with him his last few months, and again lived in the house where I grew up. My father continued the early morning schedule I knew so well. He got up before 5:00, got a cup of coffee and went down to his desk. He turned on a light that also controlled a clock. The cumulative records of hours writing each morning show consistent work, even during the early hours of holidays when Deborah's or my family were visiting. If I interrupted his work, he would switch the light off as long as I wanted to talk. When I left, the light, and clock, went back on. At around seven in the morning, he would come up to the main floor, have breakfast while reading the morning paper, and then shower and dress. Then it was time for a walk. Instead of walking the two kilometers to his Harvard Office, his last year he walked around the neighborhood, and of course I would go with him.

From ten until 12:00 he worked with his secretary, answering correspondence, and getting newly typed revisions of manuscripts on which he was working. Unlike his teaching years, he ate lunch at home. After lunch he relaxed with light reading or listening to Wagner, then came up at

5:00 for cocktails with my mother. After dinner, my father watched a bit of TV and then went to bed early, by 9:00 or 10:00. He planned his afternoons and evenings so that he would be in good shape for the next morning's writing.

He was productive to the end. Ten days before he died, he gave a talk to a huge crowd at the annual meeting of the American Psychological Association. My mother and I were there and were impressed with the force of his speech. Still fighting agencyism, he called cognitive psychology the "creationism" of psychology, getting a gasp from most of the audience and a scattering of applause. But at the end he got a standing ovation that lasted the whole time he was helped down the steps and out of the auditorium.

I wish my father were alive to see the ripple effects of his life's work. Operant procedures have created entire fields like behavioral pharmacology. Operant techniques and analysis underpin operant behavior therapy. "Clicker Training", "Tag Teaching", "Precision Teaching", programmed instruction, and his book *Verbal Behavior* has been responsible for breakthroughs in the teaching of verbal behavior to children with autism. My father always believed that the best measure of a good science was the technology it spawned. By that measure, his discovery of the selection effect of contingencies qualifies as a very successful science.

brevis

B. F. Skinner's Aircrib

Sheila Habarad, M.A.
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In 2013, Sheila Habarad reviewed an article published earlier that year on Skinner's aircrib.

In 2014-2016 Sheila contributed to Operants *as the Editor-in-Chief.*

B. F. Skinner's aircrib, which looks like an elevated box with a transparent front panel, was recently investigated by Maggie Koerth-Baker in her article, *B. F. Skinner Totally Geeks Out Over the Box He Built for his Baby*. The article reviews the inventive crib, highlighting special features that made it superior to the standard crib, while identifying the aspects that made the idea of using an aircrib odd to the general public. The article contrasts the truth of Skinner's invention versus shameless rumors that surround the aircrib's impact on the Skinner family.

The aircrib was a practical idea designed by B. F. Skinner for his wife with the physical and psychological health of their baby in mind. Decreasing the amount of labor involved in raising a newborn as well as improving the layout of the nursery also played major roles in the planning.

The article notes advantages the aircrib holds over standard cribs, which resulted in many benefits that were intended by Skinner or later became evident with use.

Instead of having a mattress with sheets and blankets, Skinner's younger daughter Deborah, wearing only a diaper, slept in the aircrib on a tightly stretched canvas covered with a sheet. The maintenance of the crib was easy; since the sheets were on a revolving canvas they could be "changed" daily by pulling a specified length of the sheet. This revolving system provided clean bedding for an entire week paired with minimal clothing, therefore, the amount of time-spent doing the laundry decreased significantly.

The air in the crib was maintained at a comfortable temperature, humidified, and free of airborne diseases. The walls were insulated to shield the infant from loud sounds. Even though the walls were insulated, Deborah was still able to hear family and friends outside of the crib. The ample space along with the lack of bedding and minimal clothing allowed Deborah to play more freely than she would have been able to in a standard crib. The Skinners enjoyed many benefits of the Aircrib provided to the mother as well as to their baby's development.

The bonus, according to Koerth-Baker, was a significant reduction in

crying, since the baby's body temperature was kept comfortable. Koerth-Baker also proposes that families in the 21st century would be much more attracted to the cleaner environment of the Aircrib due to a decrease in risk of suffocation and SIDS than families of the mid 20th century.

For the most part, the article presents Skinner's Aircrib in a positive light, in keeping with his original statement from his article in *The Ladies' Home Journal*: "It is quite in the spirit of the 'world of the future' to make favorable conditions available everywhere through simple mechanical means."

Skinner's daughter, Julie, tends to her own daughter in the Aircrib.

While the article adequately laid out some of the facts about the aircrib, it overlooked the fact that the Skinner family was living in Minnesota when they designed and used the aircrib. The temperature-controlled environment provided an increase in the amount of time for baby Deborah to play due to needing fewer blankets and less bedding. This self-regulation potentially guided the development of a stronger and healthier baby. Further, the article left the readers to form potential misconceived inferences about the Skinner family. Koerth-Baker does emphasize the benefits of the insulated walls to provide a quality napping environment, allowing the baby to easily develop a sleeping schedule as well as making it easier for parents to allow their infants to "cry it out." The article does not specifically state that the Skinner's utilized this method of parenting; the interpretation was left to the reader. The article mentioned that the Skinners only kept their daughter in the aircrib for about the same amount of time that a family would keep a baby in a standard crib. Yet, Koerth-Baker states:

> "One of the key features of the Aircrib was also the thing that makes it look a little sketchy… the crib had to also be a sealed environment, where the baby interacted with the outside world through windows on the side... Skinner is probably right in pointing out there is, technically, nothing particularly different about leaving your baby for long periods in a crib compared to leaving them for long periods in an Aircrib. But it does come across as a bit more problematic."

The author does not explain her use of the term "problematic." It simply hangs there, an implied criticism without support. B. F. Skinner's persistent efforts to share his findings to those outside his science might explain the amount of criticism that he faced throughout his career. B. F. Skinner's article in *The Ladies' Home Journal* in 1945 is just one of many examples of how he reached out to the greater community in attempt to share his findings. While Skinner and his family did encounter more rumors regarding the aircrib than imagined, they also connected with impressed families who sought out to design their own versions of the aircrib.

In the 2004 *European Journal of Behavior Analysis*, A. Rutherford pays tribute to B. F. Skinner's writings outside his immediate behavioral science as one of the many reasons Skinner became such a renowned scientist. Based on Skinner's continued efforts to share his findings with those on the outside, Rutherford describes Skinner as a legend. B. F. Skinner's brave attempts at putting his findings out there to the general public should inspire all emerging professionals in behavioral fields to continue to research, experiment, and share the results with not only one another but also with other fields.

B. F. Skinner and F. S. Keller at the 12th annual meeting of ABA in Milwaukee, Wisconsin holding 7-month-old Jonathan Kupfer during the annual banquet dinner honoring Skinner and Keller (Photos by Jeff Kupfer)

research

Operant Principles in Drug Discovery

by Brian D. Kangas, Ph.D.
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Dr. Brian D. Kangas is an Assistant Psychobiologist at McLean Hospital and Instructor of Psychiatry at Harvard Medical School. His primary research program focuses on the development and empirical validation of behavioral methods to assess the effects of both commonly abused drugs as well as the potential side effects candidate therapeutic drugs may have on complex behavioral processes.

This article originally was published in Operants *in 2014.*

In the spirit of highlighting the enduring impact of Skinner's science on modern society, the use of operant conditioning techniques in drug discovery will be discussed. As the search continues for new drugs to treat a range of medical conditions with greater efficacy and improved safety, operant principles are involved in the preclinical evaluation of nearly every drug introduced to the public. Perhaps the greatest contribution of operant principles to drug discovery is their effective appraisal of a candidate therapeutic drug's "abuse liability." Abuse liability can be broadly defined as the probability a drug will maintain non-medical, and often maladaptive, self-administration behavior. Put simply, the likelihood it will be abused.

Assessment of abuse liability has obvious value for government and other regulatory agencies seeking to limit the availability of addictive substances. In fact, the pharmaceutical industry is legally required by the Food and Drug Administration (FDA) in the US and the European Medicines Agency (EMA) in Europe to conduct operant-based assessments of abuse liability. Due to this requirement, evaluation of abuse liability usually begins in early stages of drug development in order to assess a molecule's viability. In addition, the Drug Enforcement Agency (DEA) uses these same operant techniques to evaluate emerging drugs of abuse to inform legal control and scheduling under the Controlled Substances Act.

A thorough pharmacologic profile, including information about a drug's receptor binding, bioavailability, pharmacokinetics, and metabolism, is necessary but not sufficient to evaluate adequately a drug's abuse liability. Behavioral data on 1) the subjective effects of the drug and 2) the likelihood it will be addictive are also required. Importantly, operant conditioning techniques can offer highly predictive information relevant to both concerns. And these assessments can be conducted in laboratory animals, which have several advantages including the ability to evaluate during early stages of drug development and to examine a large range of doses over an extended period of time.

First, to examine the subjective effects of a drug in nonverbal animals, the drug discrimination procedure is used. For almost 50 years, drug discrimination has been an effective tool that has provided a wealth of pharmacological information about behaviorally-active drugs including receptor selectivity, potency, efficacy, and indications of abuse liability. This procedure takes advantage of basic operant principles of stimulus control. In its typical arrangement, pre-session administration of a drug produces an interoceptive (internal) stimulus. This interoceptive stimulus can then be conditioned to serve as a discriminative stimulus. For example, responses on one lever can be reinforced following administration of a training drug and responses on another lever can be reinforced following administration of saline (i.e., no drug). Thousands of drug discrimination

studies have clearly indicated that a discrimination using an interoceptive stimulus can be effectively acquired by a wide variety of laboratory animals as well as human subjects. That an internal stimulus can serve as a discriminative stimulus with the same effectiveness as an external stimulus, such as a light or a tone, is interesting. But, for purposes related to drug discovery, the important feature of drug discrimination lies in its ability to examine the similarities and differences in the subjective effects of drugs, a feature that was initially thought too complex and too private to characterize. After the subject has acquired the drug discrimination, other test drugs can be administered on select sessions allowing the experimenter to essentially ask if the test drug "feels" like the training drug. That is, examination of response allocation on the drug- and no-drug-levers reveals the extent to which a test drug generalizes to the training drug. These discrimination conditions consistently yield highly selective and replicable generalization gradients that comport well with receptor and other substrate mechanisms as well as verbal reports in humans. Moreover, allowing the subject to report these interoceptive effects has proven to be an excellent means to assess a novel drug's abuse liability. For example, if you have a drug that is known to possess therapeutic value but is also commonly abused in humans (e.g., prescription opioids), and you have a candidate replacement drug known to produce comparable palliative effects, the extent to which the candidate therapeutic generalizes to the known drug of abuse under drug discrimination procedures has been repeatedly demonstrated to be predictive of the abuse potential for that candidate therapeutic.

Many of the most important advances in behavioral pharmacology generally, and drug discovery specifically, have relied heavily on the experimental analysis of behavior.

Second, to determine how likely a candidate drug is to have addictive properties, the drug self-administration procedure provides high levels of predictive validity. This procedure takes advantage of basic operant principles of reinforcement. In its typical arrangement, administration of a drug serves as a consequence that maintains an operant response. For example, responses on a lever under a schedule of reinforcement will result in administration of the drug. Like drug discrimination, there is a long and extensive literature on drugs as reinforcers. Environmental variables known to reliably affect standard reinforcers like schedule, magnitude, and delay have functionally similar outcomes on responding for drug reinforcers. Again, that administration of a drug can maintain operant responding is interesting. But, for purposes related to drug discovery, the important feature of drug self-administration is the strong correlation between the drugs laboratory animals will self-administer and those that are used and abused by humans. For example, drugs widely abused in the human population such as cocaine, heroin, methamphetamine, alcohol, and nicotine have all been shown to be self-administered under laboratory conditions. Interestingly, the conditions under which various drugs are self-administered are not always identical, and these differences reveal clues as to the environmental and pharmacological variables that are involved in both the initiation and maintenance of drug use. Moreover, they provide important preclinical information regarding abuse liability by evaluating whether, and how robust, a candidate therapeutic drug might initiate and/or maintain drug-taking behavior. That is, if a candidate therapeutic drug is readily self-administered by laboratory animals, it will likely be self-administered by humans.

Many of the most important advances in behavioral pharmacology generally, and drug discovery specifically, have relied heavily on the experimental analysis of behavior. Drug discrimination and self-administration procedures derived from basic operant principles of stimulus control and reinforcement, respectively, provide critical information regarding a drug's abuse liability. Indeed, this information is so valuable, both the FDA and EMA require these assessments before considering approval of any new drug. And therefore, they play an essential role in academic and industry-driven drug discovery by providing an appraisal of a candidate molecule's viability. Here again, we see another example of how Skinner's science laid the foundation for effective and indispensable operant techniques used widely today.

interview

Robert Mellon, PhD
Athens, Greece

Interview by Dr. Katerina Dounavi, BCBA-D

Robert C. Mellon is the professor and chair of the Department of Psychology at the Panteion University of Social and Political Sciences in Athens, Greece, where he established an undergraduate course of studies in behavioral philosophy and science, and directs the Laboratory of Experimental and Applied Behavior Analysis. He received his doctorate from the University of North Carolina at Greensboro in 1987; his master's and doctoral research was directed by Richard Shull and Aaron Brownstein. He completed the Clinical Psychology Internship Program at New York University-Bellevue Hospital Center. Mellon was a postdoctoral research fellow at the Center for Developmental Psychobiology at the State University of New York at Binghamton and an NIMH National Research Service Award fellow at the New York State Psychiatric Institute and Columbia University. For four years he travelled Asia, the Middle East and Europe teaching in the Overseas Programs of the University of Maryland. Since 1995, he has lived and worked in Greece, initially at the Hellenic Republic University of Crete. Mellon's empirical and theoretical work, principally in behavioral variability, resistance to change and aversive control, and the implications of these processes in understanding the provenance and treatment of problematic patterns of behavior, has been published in both behavior-analytic and mainstream psychology journals. He is also author of numerous behavior-analytic texts in the Hellenic language and has collaborated on translations of canonical works of B.F. Skinner, including Walden Two *and* About Behaviorism.

This interview was originally published in 2014.

How did you become interested in Skinner's work?

I first encountered Skinner's work in 1978, as a university undergraduate on academic probation. I was loading trucks at night to make rent and tuition, and I did not like school, but I liked loading docks even less. On a whim I registered for a course entitled Psychology of Learning in a vague hope that it might help me in my academic struggles. The course was on radical behaviorism and the temporally-extended experimental analysis of behavior-environment relations, and it helped me in every aspect of my life. For some time now I have been privileged to make my living in an effort to replicate this delightful effect in others.

Could you tell us about your research interests and current projects?

My core interest is the experimental analysis of processes that characterize so-called "psychological disorders" and the employment of general principles in their scientific interpretation, prevention, and treatment. As B. F. Skinner showed us, such phenomena are best viewed as problematic manifestations of "normal" adaptive processes that, in large measure, emerge from the widespread employment of contingencies of punishment and negative reinforcement in the social control of behavior. With adult human subjects, we are currently studying how the stimuli produced by the problematic perceptual and interpretive behaviour (including repetitively self-abusive thinking and imaging) might acquire reinforcing potency adventitiously (as "safety signals") when they repeatedly accompany motor acts that terminate social threats.

This same process of essentially adventitious control by self-produced signals of safety from social punishment might maintain the bizarre-appearing form of "autistic" motor behavior. We are thus investigating how an understanding of the discriminative processes that inhere in the differential reinforcement of response-form variability might help us to more effectively establish the positive reinforcing potency of self-produced stimuli that differ from those of recently emitted acts. This work is being conducted with children in whom "stereotypic" acts are frequently emitted.

What can you tell practitioners about your research, how is it applicable to their work?

When we consider the social significance of the varied phenomena described as "psychopathological", the number of people working in clinical behavior analysis is very small. Currently, the dominant theoretical perspective in this small group is a "post-Skinnerian" contextualism which is based on a radically generalized conception of the operant class. In the therapeutic approach based on this view, people troubled by their own problematic thinking are instructed to indefinitely suspend all efforts to understand these processes. I hope our own work might give

practitioners pause before rejecting (as a source of clinical case formulation leading to effective treatment design) Skinner's fine-grained interpretation of perceiving and thinking as the privately-observable generation of biobehavioral events that acquire eliciting, reinforcing, discriminative and motivational effects just as publicly-observable events do.

Moreover, I would encourage my fellow practitioners to provide such interpretations of problematic perception and thought to their higher-functioning clients, who might otherwise be baffled, embarrassed and frightened by their own natural and scientifically explicable behavioral processes. I believe that the ability of many troubled persons to acquire a beneficial understanding of their own behavior is frequently underestimated, and their needs are underserviced in consequence.

In the last three decades, you have taught numerous students at undergraduate and postgraduate level. You are also well known in the field for being able to create really engaged students who will serve as the future generation of behaviour analysts in Greece and Europe. Could you identify some key aspects of your teaching that lead to this increase in students' interest in Behaviour Analysis?

You are most kind to say so. Skinner taught us that effective teaching is a matter of building on extant repertoires by the gradual adjustment of setting events and consequences, ensuring not only that new discriminative behavior occurs, but that it is automatically reinforced by the events that it produces. Here as well, we endeavour to follow his lead.

The Hellenic language is spoken by a small minority of the world's population, and beyond their native tongue, all of our students speak English and at least one other European language. Despite this, we use no English at all in the first three semesters of training in behavior analysis; we build on the well-established and familiar verbal repertoire employed in everyday affairs. This is indispensable to success, because our philosophy and science are, of course, themselves antithetical to the essentialist popular understanding of the nature and provenance of human behavior. So alien a perspective has little hope of success when introduced in a foreign tongue.

Another important aspect of students' extant repertoire is its general avoidant character. As is true elsewhere, in the Greek education system the chief reinforcing event is the termination of threats of failure, an event generally contingent upon rote repetition of curriculum materials. In this context we are called upon to explain to our students that much of what they always knew about themselves and the people around them, including much of what they have learned in other psychology classes, is directly contradicted by scientific analysis—in a word, wrong! This trauma, which often evokes unconsidered rejection or temporary rote memorization of the behavioral perspective, might be lessened if preceded by a frank and clear presentation of the general process of scientific investigation, proceeding as it does on systematic self-doubt and the arrangement of conditions designed to reveal the inaccuracies, inconsistencies and limitations of our current interpretation of the physical world. It is easier to be wrong when we understand that there is no other option—and that, if we cannot be absolutely right, we can choose to be more effective in our efforts to predict and control behavior.

And of course we want to assess the events that typically function as positive reinforcers, and arrange for their provision consequent to our students' efforts to understand behavior as an object of scientific investigation in its own right. We provide analyses of many examples of phenomena that already pique their interests, such as lying, sexual preference, procrastination and paranoia, encouraging them to critically analyse and improve upon these efforts. Comparing these efforts to the mentalistic explanations that we all once held is a staple of good-natured humour and fun, further reducing the fear and avoidance of error that are so fundamentally incompatible with scientific inquiry.

Could you tell us about the status of Behaviour Analysis in Greece when you first started working in this field and how this has changed until its current status?

When I arrived at the University of Crete in the mid 90's, the first academic departments of psychology in the state university had just been established. There were no systematic courses in behaviour analysis and, naturally, no texts. In a number of psychology and education texts, there were (and indeed still are) brief, precise translations of the misrepresentations of our perspective (as S-R or black box psychology, etc.) that commonly appear in English-language textbooks; nothing more. At the outset I was allowed to teach two courses a year in behavior analysis in exchange for teaching two courses in psychometrics! In collaboration with my students over many years, we developed a comprehensive introductory text and have translated and published canonical works of B. F. Skinner.

Since 2006, the Panteion University Psychology Department has provided us with an opportunity to conduct a seven-semester undergraduate cycle of studies in conceptual, experimental, and applied behavior analysis, including three lecture courses, a laboratory course, a two-semester undergraduate thesis and a one-semester practicum in applied behavior analysis. Unfortunately our faculty development has been delayed due to the IMF-imposed austerity measures; in consequence we cannot yet staff a master's program. However, a number of our students have been able to continue their training elsewhere, and many are working in applied settings in the Hellenic Republic and abroad. This is of course very satisfying, but much remains to be done.

In recent years several private and publicly-funded centers have been founded for the provision of applied behavior analysis services for children with developmental delays. Two of these centers as well as a local private college have established seminar courses in applied behavior analysis. This of course is no substitute for a laboratory-based scientific training program, but it is helping to make our approach better known and appreciated, and less frequently misrepresented.

Could you highlight some events that have helped Behaviour Analysis progress in Greece?

Our efforts received a terrific boost from the decision of the executive board of the European Association for Behaviour Analysis (EABA) to hold its 2010 bi-annual conference on the Greek island of Crete. Imagine how import-

ant it was for some forty of our dedicated students, most of whom had never laid eyes on but one working behaviorist, to enjoy four days of live presentations of the latest developments in the work of skilled scientists from across Europe and around the globe! Some of these students presented their own research and the balance enthusiastically helped in the conference organization; all rightfully felt, for the first time, part of the international behavioral community.

This event led directly to the founding of the Hellenic Community for Behavior Analysis. The organization's name is a direct reference to Skinner's use of the term "verbal community," as its purpose is to foster the development and dissemination of our philosophy and science among speakers of the Hellenic language. Its first two-day scientific conference, which was free and open to all interested parties, was held in 2013 (Greek speakers can find videos of many presentations on the community's webpage www.behaviorism.panteion.gr).

Of course, it is equally important that we retain and further develop our relationship with the international scientific community and Hellenic behaviorism is well represented at the September 2014 conference of the EABA in Stockholm, Sweden. Moreover, we will have the honor of hosting the EABA's first Summer School of Behavior Analysis in July of 2015; an event that will bring together advanced students and accomplished instructors from across Europe for a two-week intensive period of scientific and social exchange.

As President of the European Association for Behaviour Analysis (EABA), what are your thoughts about the current status of Behaviour Analysis in Europe?

Well, things are looking up. There are a fair number of quality advanced training programs taught in a range of languages, several impressively large and well-organized national behavior-analytic organizations, and an EABA that has truly made strides in establishing a pan-European forum in which substantial cultural differences might enliven and enhance rather than retard the development and dissemination of a science of behavior. In some countries the cultural penetration of behavior-analytic thinking rivals or even exceeds that in the U.S. (unfortunately, that is not saying much). In many other countries such as my own, we have just gotten started down the very long road to an equitable sharing of the fruits of the behavioral enlightenment.

Can you identify a number of obstacles in the dissemination of Behaviour Analysis in Europe and suggestions on how to overcome them?

I have already touched on the difficulties related to the wide range of European verbal communities. Beyond that, it seems to me that the contingencies (and lack of contingencies) that need to be addressed if Skinner's "happy few" are ever to become "many" are pretty much the same everywhere.

After Skinner's death, our public criticism of the almost universally-held belief in Autonomous Man, with all of its attendant implications for social policy, has been negligible. In a period in which internet access to scientific analysis and the rhetoric of enlightenment has led millions of believers to question the existence of celestial spirits, even the leaders of "new atheism" assert with assurance that an Unmoved Mover resides in our minds, characters, or nervous systems, blithely actuating our thoughts and actions. Behaviorism simply cannot coexist with Autonomous Man, yet we seem to be doing little to hasten his demise.

If we are to help people past an extensive history of reinforcement for spurious beliefs, we must arrange for powerful events to occur contingent upon experimentally-derived interpretation. To think like a behaviorist, they must get something really good out of it. But we have been peculiarly hesitant to offer people help with the problems that they really care about when they try to think behaviorally. Nobody needs a natural science interpretation when things are going well. We need it when we cannot understand our own behavior or someone else's; when we seem to be acting for no reason, or against our own interest. This is why abnormal psychology is always, and by far, the most popular psychology class.

Yet, as a field, a large proportion of our clinical interpretative efforts are devoted to relatively rare conditions that many or most people have very little experience with. Applied behavior analysis has been extraordinarily successful with otherwise intractable clinical conditions such as autism, but we have allowed the field, in the public eye at least, to become virtually synonymous with its treatment. If people are to become behaviorists, behaviorism must help them with the problems that are troubling them. Problems like anxiety, depression, difficulties with food or drink, obsession, paranoia and related interpretative difficulties, sexual dysfunction, aggression, self-abuse. If people are not getting what they want and need for thinking about behavior the way that we do, we should not expect them to do so. Again, if we want to change behavior, we must utilize the extant reinforcers.

But we should not limit contact with the behavioral position to the relatively aversive contexts of the psychology classroom and clinic. Just think how many behavior analysts have been affected by a chance reading of *Walden Two*. We published our Greek translation just one year ago, and it is remarkable how many people have discovered behaviorism in the context of a good read on the nature of the "good life" and its practical realization (a topic especially reinforcing in the midst of an economic crisis). Yet here we are, going on seven decades later, and *Walden Two* remains the sole example of behavior-analytic fiction! And not one fictional film to counter with when people cite *A Clockwork Orange*. Why are we not utilizing such effective means of changing how people think about our science?

Perhaps we are tripping up in a failure to think about dissemination itself as a problem for applied behavior analysis. It seems doubtful that our failure to more effectively propagate behavioral thinking is based in the weakness of our basic principles. A proper test would be a redoubling of our efforts to apply them.

Skinner's Doctoral Thesis: Stepping Outside the Formula

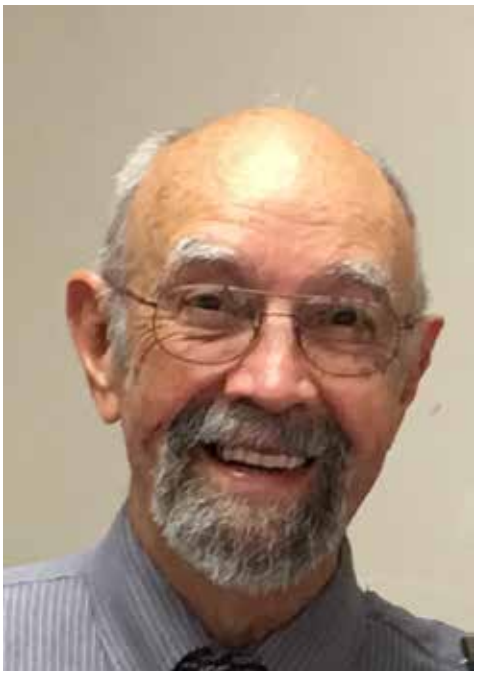
science corner

Ernest A. Vargas, PhD

The second part of Skinner's doctoral thesis (1930) presented his experimental operations on what he then called the "reflex" relation. He submitted a good many records of rats' eating rates. In the first part of his thesis, well over half of it, he reviewed the history of the concept of the reflex. What he drives home in this history, whose manner is adopted he says from Mach, Poincaire, and Bridgeman, is how "the conflict between observed necessity and preconceptions of freedom" produced the tension in interpreting the observed results of surgical preparations of decorticate animals. In all animal life, the soul was held to be responsible for movement. As Skinner states, "the movement of an organism had generally been taken as coexistent with its life and as necessarily correlated with the action of some such entity as soul. The necessary relationship between the action of soul and the contraction of a muscle . . . was explicit. As a consequence, it was disturbing to find, experimentally, that a muscle could be made to contract after it had been severed from a living organism or even after death."

"Movement, far from being the objective manifestation of the activity of soul, had become an organic process subject to experimental investigation." Unpredictable variability still occurred, and when it did, a "non-physical concept" such as mind or volition was asserted as its cause. Additional experimental work with "spinal frogs," for example, dispensed with the variability and thus with causes outside of those of immediate mechanical or chemical applications. The physiological examination of basic muscle motion eventually replaced, with the concept of "stimulus," the cause for movement that formerly had been given to "causes" such as the earlier one of "soul" and the later one of "mind." The further step was taken by Pavlov. "Pavlov was engaged in the investigation of the activity of the digestive glands. For much of this activity it was possible to identify the necessary antecedent events (the mechanical or chemical changes acting directly or reflexly upon the glands). The greater part of the normal secretion . . . was . . . not under the control of the experimenter. . . . [T]his was called "psychic" secretion. Pavlov undertook the investigation of this activity."

"The work of Pavlov may be taken as historically fundamental ... The principle of conditioning supplied the extended range of stimulation needed to account for the complex behavior of the total organism." Pavlov's findings became a cornerstone of behaviorological science, but these discoveries are not the focus here. What is of import is the further exclusion of a non-material cause. In all his writings Skinner drives home the point that the description of behavior (and its explanation when description is extended) rests on two relationships: the correlations between actions and stimuli; and based on those primary correlations, the secondary correlations with a class of events he calls "third variables"—events such as "emotion." But critically, the primary functional relations between actions and stimulus variables do not derive from antecedent events.



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Stimulus events prior to an action cannot account for all of the variability between stimulus variables and response variables. The typical solution has been to construe an agency (such as a "mind") or agency-like structure (such as a "grammatical mechanism") between the antecedent stimulus events and the response outcomes that adjust for the discrepancy in the paired values of the observed sets of values. Almost all of behavioral science operates within this stimulus-agency-response formulation. Skinner stepped outside this stimulus-response formula. Skinner's position is radically contrary to other behavioristic positions, including that of John B. Watson. Skinner investigated the selective effects of an immediate milieu upon those actions that impact it. It made unnecessary any agency. The selection effects of particular properties of internal or external milieus combine with the appropriate effects of third variable events and produce changes in classes of action. By accounting for variability, such postcedent effects dispense with the necessity of an agency.

Skinner's thematic interpretation not only applies to the events the organism's behavior directly encounters. It also applies to those actions mediated by others, for example language. Skinner (in *Verbal Behavior*) summed his position as follows:

> Whenever we demonstrate that a variable exerts functional control over a response, we reduce the supposed contribution of any inner agent. For example, if we can show that the occurrence of a response is due to the presence of a stimulus of specified properties, then it is not necessary to say that a speaker uses the response to describe the stimulus. If we can show that a response is stronger when we deprive the individual of food, then we do not need to say that a speaker uses the response to describe or disclose his need. If metaphorical extension can be shown to take place because a particular stimulus property has acquired control of a response, we do not need to say that a speaker has invented a figure of speech to express a perceived similarity between two stimuli. If an audience can be shown to strengthen a particular subdivision of a verbal repertoire, we do not need to say that a speaker chooses words appropriate to his audience.

In this analysis of verbal and lingual behavior the agent disappears.

This exclusion of agency puts forward the most radical thema in the Skinnerian frames of reference by which actions are interpreted. It is not an issue of whether there is an "inner life" or not. For Skinner, undoubtedly there was. The issue was how we talked about behavioral events wherever situated. What were the descriptive concepts and explanatory principles by which that talk occurred? As an explanatory principle, his theory excludes "mind." It excludes as well any agency or feature within the organism that "intends" or "decides" or "chooses" or any of the other vast array of words that center an analysis upon an organism and its presumed inner doppelgänger. Skinner's analysis moves to the contingencies between actions and events, wherever and however those contingent relations are located. It finalizes the dethronement of humankind's dominion over nature, which earlier featured Copernicus's and Darwin's analyses. The implications have not been lost to those who object to such a dethronement. And it is this kind of thema, as Gerald Holtan, physicist and historian of science (in *Thematic Origins of Scientific Thought*) points out, that so upsets those with an opposite one—"the widespread feeling of paradox and outrage when a new thema is proposed in opposition to the prevalent ones—as was, of course, the case with relativity theory, so much so that Poincaire, to the end of his life in 1912, never once referred to Einstein's theory of relativity in print (and to Einstein, as far as I could discover, only once on the subject of the photon, and in a derogatory way)."

Eventually, physicists got over their hissy. It will take longer in the behavioral sciences, for so many of society's institutions operate on the presumption of an agency with free will who sins, buys, and votes. These social institutions directly (e.g. grants that fund projects to explore agency action) and indirectly (e.g. socialization that builds beliefs in agencies) shape the presumptions of behavioral scientists.

Heritability and Contingencies

A Closer Look at a Particular Aspect of Skinner's Theory of Behavior

science corner

Per Holth, PhD
Oslo and Akershus University

Professionals and laymen alike often have strong opinions regarding the "relative importance" of heredity and environment. 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? 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. In general, then, heritability varies with the extent of genetic and environmental variation for the population 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, 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,

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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." 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. 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 produce 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.

As Donahue pointed out, Darwin, of course, was the "Skinner of phylogeny," 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 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.

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," 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." And in an interview, Chomsky while explaining the devastating effect of his review of Skinner's *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. 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.

Responding to the accusation of his late interest in evolution, Skinner in 1977 simply listed evidence to the contrary throughout his career. An interesting additional

conclusion on page 30

Iver Iverson, PhD
University of North Florida

interview

Interview by Monica Vandbakk, PhD

Describe your work and your recent interests.

My research centers on establishing basic knowledge of operant behavior based on sound methodology with a high degree of replicability. Of particular interest, I have found that individual reinforcers can control behavior quite vividly during early acquisition of operant behavior. One can even design an experiment around giving just a single reinforcer to rats and examine how various behaviors change over time before and after the single reinforcer. Currently, I do research on stimulus control of operant behavior in rats where a novel response is introduced in a familiar discriminative stimulus, and I find that rats have to learn the discrimination all over each time I introduce a novel response. It means that the rats do not really learn a general S-dee S-delta difference; that difference is specific to the response that is reinforced in S-dee. In the laboratory, we also examine basic chaining procedures, and currently I work on intermittent reinforcement of stimulus control in behavior chains. Apparently, intermittent reinforcement of stimulus control units is an understudied area. We are able to maintain over a thousand stimulus control trials in one session with just 50 reinforcers. All the research themes I work on have a core of interest in methodology, and I suppose I had that interest since I started as a psychology student in Copenhagen where I would build boxes and special levers all the time.

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

First, Skinner's most important contribution is the early demonstration that voluntary behavior can be brought under experimental control, including stimulus control. The second most important contribution is probably that behavior can be maintained with intermittent reinforcement. The third contribution is probably that he developed a fairly consistent vocabulary for use in behavior analysis, a vocabulary that sought to eliminate references to causes of behavior that have no means of scientific verification.

Which is your favorite book by Skinner?

Science and Human Behavior. This is the first book where Skinner really articulates all the societal implications of an experimental analysis of behavior.

Which other authors do you think are of great importance in the development of our field?

Professors Keller and Schoenfeld had a tremendous influence on the field through their textbook, *Principles of Psychology,* and through mentoring a high number of graduate students. Most of the top people in the field, both past and present, were in fact students of Keller and Schoenfeld. These many students include Professor Murray Sidman, whose *Tactics of Scientific Research* should be read by all students of behavior analysis. It was one of the first books on behavior analysis that I read as a graduate student, and I never forget the excitement and respect for methodology that I developed as I read it (several times in fact).

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

Probably the survival of the field as a science. I mean that applica-

Iver Iversen received his PhD in Experimental Psychology from University of Copenhagen, Denmark (1978). He has been a professor of experimental psychology at University of North Florida, Jacksonville, since 1986. His research focuses on basic mechanisms of operant behavior, primarily in non-human subjects. Examples are detailed analyses of effects of individual reinforcements in rats, intermittent reinforcement of stimulus control in rats, and visual guidance of drawing in chimpanzees. His research also involved operant conditioning of brainwaves in humans to enable communication in completely paralyzed ALS patients.

Dr. Iversen believes that strong methodology is necessary to advance the science of behavior, and he has developed several automated methods to shape and control behavior, as well as methods to analyze complex data from behavioral experiments. Dr. Iversen edited a two-volume text on methodology in experimental analysis of behavior (1991) with Dr. K. A. Lattal (West Virginia University). In addition, he has published several papers that document development of behavior control techniques and methods of data analysis.

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tion is mainly technology of behavior and does not always generate novel information about behavior. Only basic research can generate novel information and novel methodology. Many behavior analysts worry about the future of behavior analysis and as a solution want to extend behavior analysis to other areas of psychology and incorporate standard methods from such other areas into behavior analysis. This may wash away the uniqueness of behavior analysis, which is rooted in single-subject designs, analysis of causes of variability of behavior, and exploratory research.

Do you have any thoughts on historical events or cases in the past that may have been detrimental to the field? Events you would like to erase if you could?

Personally, I believe that the heavy emphasis on mathematical modeling may have been detrimental to the field because it may have stifled exploratory research among young researchers and placed an undue emphasis on "averaging behavior" as opposed to examining behavior at the level where it actually occurs. In addition, I believe that the emphasis on "constraints on learning" in the 1970s may have tarnished the reputation of behavior analysis because the idea that behavior analysis is fairly limited in use became dominant in introduction to psychology textbooks for decades thereafter.

Do you have any thoughts on events, cases, people, or writings you think are underestimated, and that you would like to bring to light if you could?

In general, I believe that research on stimulus control is not emphasized enough in the area of behavior analysis. There are hundreds of "gold nuggets" of already published articles on important stimulus control research that never get cited, and this research will most likely be forgotten. I have often thought of writing a textbook on stimulus control research, but it will be a major undertaking.

What is your point of view considering neuroscience and its relevance to the practice of behavior analysis?

I believe that neuroscience will keep using the techniques of behavior analysis to examine how the brain works and that some important findings about the brain can be obtained that way. However, my experience has also taught me that neuroscientists, in general, often do not fully grasp the core idea in behavior analysis that voluntary behavior is controlled by environmental variables and that behavior of the individual subject can be controlled fairly accurately at the level of seconds, if not milliseconds. I believe that collaboration between behavior analysts and neuroscientists can benefit both areas. In fact, such collaborations already exist and have been very fruitful.

Any final thoughts you would like to share with our readers?

I would like to recommend to behavior analysts that they always read articles about basic research even if they do not have an opportunity to conduct basic research.

Per Holth: Heritability and Contingencies (continued from page 28)

detail that has seldom been mentioned appears in the second chapter of *Schedules of Reinforcement*: "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 ..."

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. As Skinner wrote:

> "Contingencies of reinforcement have the edge with respect to prediction and control. The conditions under which a person acquires behavior are relatively accessible and can often be manipulated; the contingencies under which a species acquires behavior are very nearly out of reach."
>
> "No matter how important the heredity of an organism in determining its behavior, it could not be changed after conception."

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.

The Analysis of Behavior in Instruction: Science and a Technology Based on Science

history of science

by James G. Holland, PhD

For me, the beginning of behavior analysis in education began when I arrived in the fall of 1957 at a gray clapboard building, Batchelder House. Batchelder House, then in decay, had been a rambling residence just across the street from Harvard's Memorial Hall, where the Psychology Department, including Skinner's office and laboratory, was housed. A year earlier, Skinner had received a modest grant from the Ford Foundation and, to accommodate the new staff of two, was assigned one medium size room in this off-campus building which had dust that must have dated back to the days of the McGuffey Reader. Memories of those days in Batchelder House give me a special personal verification of humorist Francis Parkinson's claim that active, productive, and innovative activities are to be found, not in new buildings that instead house moribund organizations, but in small, converted, understaffed, and unkempt buildings. In this light, it seems fitting that this room in Batchelder House served as cradle for an offspring of Skinner's basic science, the experimental analysis of behavior. The infant, programmed instruction and teaching machines, was to take many forms as it grew and exerted an influence on many educational practices. Moreover, the efforts at instructional design were to reveal omissions in the basic science and were to prompt new directions of research, which would, in time, enrich the parent theory.

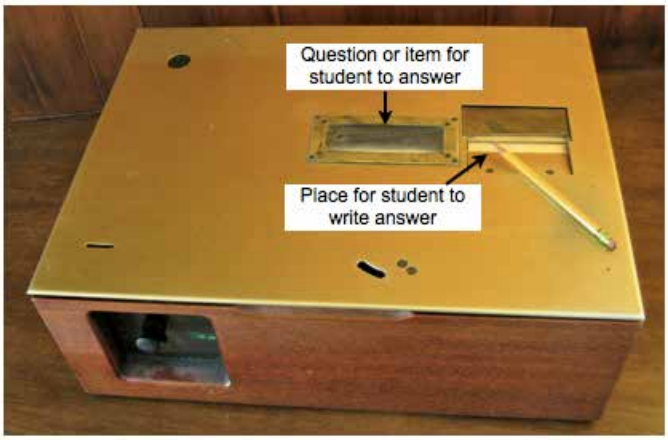
But when I moved into Batchelder House that fall day in 1957, this scenario was unclear. Lloyd Homme and Sue Markle had been at work for a year. Homme was about to return to the University of Pittsburgh as his year's leave was over. In Batchelder House, he had prepared units teaching the uses of suffixes and prefixes to build vocabulary. These units were both exercises in programming aimed at discovering more about the process, and examples of the possibilities that this use of the science of behavior held for instruction. I joined this enterprise by setting out to prepare a program to teach the content of a course that Fred Skinner had taught for many years.

Harvard's course—Natural Sciences 114—taught undergraduates the nature and findings of the experimental analysis of behavior pioneered by Fred Skinner. It dealt considerably with Skinner's extrapolation of the science to interpreting human behavior in society at large. He had earlier written his book, *Science and Human Behavior*, for this course, and now, our task was to prepare a teaching machine program covering this content. We were particularly pleased that the first actual use of our new technology in a regular educational setting would be to teach the science which provided the fundamental principles of the technology itself.

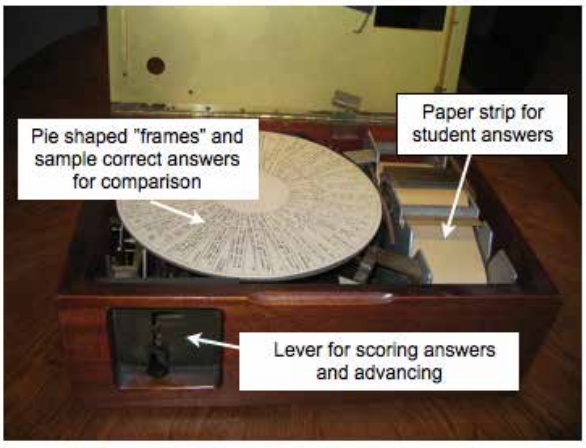
The teaching machine portion of the course took place in a small room in the basement of Sever Hall, a venerable old building in Harvard Yard. Our room had been used for storage but now was remodeled to accommodate ten cubicles each lined with acoustical tile and each containing a teaching machine. The machine itself was one of several designed by

These memoires appeared as a Foreword to The Technology of Teaching *by B. F. Skinner, published by the B. F. Skinner Foundation in 2003 and published in* Operants *in 2015.*

Skinner. It was a mechanical marvel and was reminiscent of the age of brass instrument psychology. It was, in size and shape, like a small suitcase. The brass coated lid and face was one of the larger sides of this box. The student opened the lid and placed in it a paper disc, 12 inches in diameter, which was divided into 30 wedge shaped areas each containing a single item, or frame, of the teaching program. The usual form was a completion item, a sentence with one or more words missing. A small triangular corner of each frame contained the answer to the item. With the lid closed, a single frame was exposed. Under an additional window, the students could write their answers on a strip of adding machine tape. They would then move a lever that operated a small shutter that exposed the correct answer, simultaneously advancing their own constructed answer to a position under a glass plate, where it could be seen and compared with the correct answer but not changed. If the student judged the



answer to be correct, an additional movement of the lever punched a small hole beside the constructed answer and internally set a detent so that this item would not be presented again. On completion of all 30 frames, the student would



start through a second time, and the disc would rapidly rotate past all correctly answered items stopping only on the few items answered incorrectly. The lever used for exposing items and indicating correctness of answers also wound a spring that powered the disc mechanism.

It was not long after my arrival that Natural Science 114 was due to be taught, so we rapidly began to program material but had only a small portion of the course ready on the first day of class. The lights of Batchelder House burned late as I worked to stay ahead of the students in generating material for the machine. During the day, students appeared at the machine room at times of their own choosing, worked as long as they wished, and left better prepared to understand and enjoy the lecture part of the course.

The 30 small wedges were a tight constraint on the writing of material. Strunk and White in their classic book, *Elements of Style*, gave the would-be author the strong dictum, "Get rid of unnecessary words." Writing small frames to fit the boundaries of the wedge made it important to get rid of unnecessary words. Unfortunately, those very small frames became identified by many as the defining characteristic of programmed instruction, a characteristic that took a decade to outgrow.

As the semester progressed, box after box filled with strips of adding machine paper covered with student answers. There were our data. At the end of the term, we tallied item by item, correct and incorrect, for each student. Each of the 250 students had generated about 3000 answers. We were interested in precisely what answers they might give when the item was answered incorrectly. We had attempted to prepare items that were correctly answerable only through mastery of what the item was supposed to teach. That is, we designed, in the language of our science, a contingency of reinforcement. At the same time, it was important for the student to be able to perform what was expected of him at each step along the way. Hence, we were striving for error-free performance. In this first year we were very far from error free performances or even the 5% error rate which, as pragmatists, we considered the maximum allowed without requiring revisions. After our tally of the data, we carefully rewrote the program. We were excited by the fact that unlike any other efforts in education, we had the means to gather detailed data on our teaching procedures and were able thereby to make fine adjustments. As behaviorists, after all, we were not allowed the luxury of accusing the nonlearner of stupidity. The fault, according to an experimental analysis of behavior, must rest in environmental contingencies, and it was just those contingencies which formed our program.

Three development cycles, classroom use, data analysis, and revision, were completed with dramatic improvement in our program after each recycling, and eventually, it was published under the title *The Analysis of Behavior*. But back at the time of our first use at Sever Hall, interest and activity in programming materials began to sweep the country. The concept had excited many in universities who enthusiastically set out to program their courses or to prepare materials for the primary grades. Publishers became

interested. Authors of industrial training material turned in overwhelming numbers to programming. Special new companies devoted to teaching machines and programming emerged, and large industrial firms explored the possibilities for teaching machines. But before considering where this interest led, let us consider the antecedents, for programmed instruction is an example of the use of a basic science in generating specific, deliberate applications. In addition, the use of this basic science in programmed instruction eventually permeated standard practices until the new principles became intuitive truths.

In the 1930s, Skinner had developed the concept of operant behavior and the means of analyzing the controlling variables for the behavior of individual organisms. His approach and the shape of his science was articulated in 1938 in his book, *The Behavior of Organisms*. Most that has followed in the science has been refinement and expansion of the discoveries revealed in this seminal work. In the concluding chapter of the book, Skinner says:

> The reader will have noticed that no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs. But it is a serious, though common, mistake to allow questions of ultimate application to influence development of a systematic science at an early stage. I think it is true that the direction of the present inquiry has been determined solely by the exigencies of the system. It would, of course, still have been possible to suggest applications to human behavior in a limited way at each step. This would probably have made for easier reading, but it would have unreasonably lengthened the book. Besides, the careful reader should be as able to make applications as the writer. The book represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will.

It was not long after this that Skinner did extrapolate. He did so first in his teaching. Natural Science 114 was just such an extrapolation to day-to-day life. But it was only when he began using these principles to design teaching machines that an explicit effort was made to apply his science and create a technology for the solution of behavioral problems.

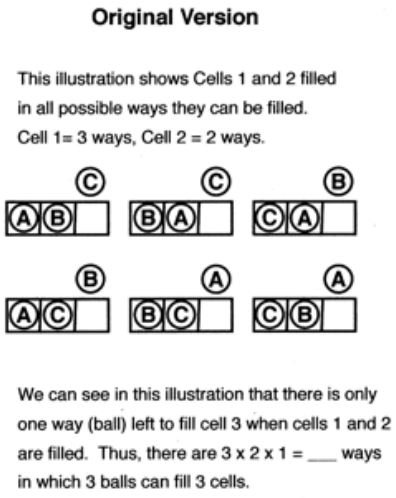
Our early programming activities functioned in the development of the technology. These served as models for the use of fundamental behavior principles and the basis for describing this new technology. The lab had taught us the power of establishing contingent relationships between behavior and reinforcement, and we defined programmed instruction as the arrangement of careful sequences of contingencies of reinforcement leading to the objectives of education. From the laboratory, we knew that through shaping, difficult forms of behavior could be established that would never appear naturally without the arrangement of a progressive series of contingencies, and here was the basis for designing programs. The science had abandoned mythical inner causes of behavior and had demonstrated the power of analyzing behavior and its controlling events. In this, the science has provided the basis for behavioral objectives in education and holds the possibility, as yet unfulfilled, of an experimental analysis of knowledge itself.

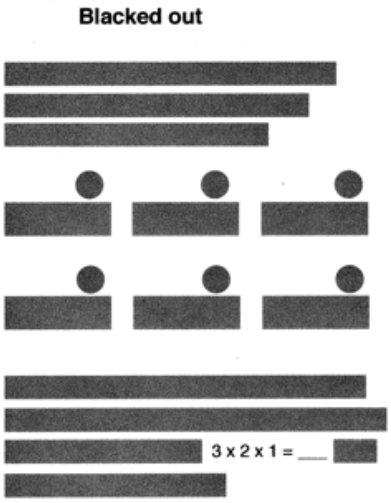
In the flurry of activity that followed these first examples of applied behavior analysis in instructional design, many impressive results were obtained for a wide variety of skills and subject matter areas. At the same time, a number of programs followed the superficial characteristics of the techniques without reflecting the laboratory-based principles. One common failure of teaching materials is to aim at certain behavioral objectives while allowing the student to perform tasks that only superficially resemble the desired behavior. For example, science education materials may have a goal of teaching scientific inquiry, while the instructional techniques only guide the student through certain problem-solving methods without ever teaching the student to generate the steps.

But perhaps, the most frequent and damaging problem in poorly designed educational materials is the failure to ensure a contingent relationship between the student's correct answer and what is to be learned through that answer. A student learns what he or she performs. Usually, in an instructional situation, only a small part of the student's activity is public and available to the instructor; i.e., a question is answered about material the student has read, or an answer is written to a problem in the lesson material. The task of the developer of educational materials is to ensure that the final public performance depends upon the correct execution of the private act—a correct answer indicates that the material has been read and that the problem has been worked out. This is the problem of response contingency. This common failing in poorly-prepared materials involves over-cueing or inappropriate cueing, which enables the student to answer correctly without having actually performed the task that the lesson was intended to evoke.

We had failed apparently to make this principle clear. Subsequently, we developed a technique that would make response contingencies very clear. This technique involved deleting, by covering with black crayon, all material which did not contribute to reaching a correct answer. For example, a lengthy exercise in a statistics program for engineering students described the determination of arithmetic combinations and permutations, but when the student was finally asked to do something with this information, the question was simply "3 x 2 x 1=__". None of the information on combinations or permutations was necessary for the answer. A contingent relationship was lacking since all of the preceding material could be covered with black crayon

without affecting the student's answer. This total blacking-out of the material demonstrates the need to rewrite the material so that the student must make use of the information to obtain a correct answer. This technique permitted a quantitative measure of the degree to which the contingency principle was met. We called it the black-out technique.





On the heels of this first effort to program, our lab as well as others began turning away from programming verbal knowledge. We moved to areas and skills that traditionally have been taught poorly. Demonstrations were prepared for teaching difficult musical discriminations, and a gadget was designed to reinforce matching an auditory rhythm. Visual discrimination programs were developed to teach spatial thinking and inductive reasoning skills. Under a grant from Carnegie Corporation, the Committee on Programmed Instruction was formed to facilitate Harvard and MIT faculty efforts in programming skills in their own areas. Languages and sciences were particularly emphasized, and I enjoyed the paradox of two Chomsky students programming language teaching objectives derived from Chomsky's structural linguistics, which he felt to be a refutation of Skinner's analysis of verbal behavior.

Across the country, programming efforts had become so widespread that Carl Hendershot provided a major contribution by keeping an updated compilation of programs.

But gradually, the term "programmed instruction" became less fashionable even as the influence spread more widely. Objectives in education became behavioral objectives. Books and lesson plans, whether they were touched by programmed instruction or not, at least benefited by borrowing the method of defining their teaching objectives.

Doug Porter, from the beginning a resident of Batchelder House although not administratively on the project, branched out from his early involvement to work for the Office of Education in developing a training package for The Job Corps. Faced with the immediate problem of creating a reading curriculum for Job Corps trainees, he gathered together a variety of curriculum materials from pre-reading to high-school level, including a programmed package for beginning reading. Porter then designed a graded examination for diagnosing the particular needs of the corpsmen for placement in these materials. Shortly after this, one of the leading centers in programmed instruction at the University of Pittsburgh, spearheaded by Robert Glaser who had carried out research in programmed instruction, turned to the idea of diagnosing individual needs through prescriptive testing and placement under the coined name "individually prescribed instruction." While this new emphasis focused on diagnostic procedures, the teaching material generally followed the experimental analysis of instruction.

To implement developments in individualized instruction, in 1964, Glaser and Gow formed a new organization, the Learning Research and Development Center, devoted to facilitating education through fostering an interplay between science and practice in education. The creation of the Center embodied the metaphor of a long hallway with a lab at one end, a classroom at the other end, and between the two, all the sequential stages of technological development with busy scientist-developers running back and forth through the hall. A few years later, Fred Keller extended the concepts into the Personalized System of Instruction. In his system, the wedge-shaped frame is gone, the teaching material comes in larger hunks, and students answer questions of larger scope, but still, the questions are prepared so that an answer is contingent on mastery of preceding material.

The influence of the beginning of these applications of our science was not limited to the world of education. More than an opportunity to improve education through behavior science had begun. The teaching machine was the first step in what we now call applied behavior analysis. The science was there waiting to be used to improve conditions for people. No doubt various areas of application could have emerged but one opening was made through programmed instruction. Many of the simple applications involve only reinforcing a single response already in the person's repertoire. For example, orienting toward the teacher might be reinforced. When more difficult performances are involved, however, the similarities to the techniques developed in programmed instruction are apparent. Establishing speech in an autistic child requires a slow, gradual shaping process that carefully constructs utterances of sounds, then simple single words, and later sentences.

By the end of the 20th century, even clinicians explicitly drew upon principles of programmed instruction. For example, Israel Goldiamond suggested that the therapist

in producing a clinical program specify target or outcome, specify entry behaviors and beginning repertoire of the person, sequence behavior—change steps, and finally provide maintenance consequences for each step in the sequence. This clinical approach emphasizes constructing new operants by building on the current repertoire of the individual as in programmed instruction.

Programmed instruction illustrates the usefulness of basic research in leading to important applications, but the flow of influence goes the other way as well. Attempts at using basic science in dealing with real-world problems removes the tunnel vision of the basic scientist. The complexities of the applied settings may reveal oversights and gaps that exist in the theory. The practitioner, to solve his immediate problems, does the best he can by improvising to cover the deficiencies, but when basic and applied scientists are closely related, or even perhaps the same person, experience in application can open new research areas and enrich the parent theory.

One of the several serious gaps was revealed as we set out to teach discriminations errorlessly. Until this time, laboratory research in stimulus discrimination had always proceeded by reinforcing a response to one stimulus while extinguishing it in the presence of another stimulus. Animal discrimination typically progressed slowly. They responded in the presence of what was to be the negative stimulus as well as the positive stimulus until gradually, after hundreds or even thousands of responses to the negative stimulus, extinction was complete with the animal responding only to the positive stimulus. This was the only way discriminations were formed in the laboratory, and it was assumed that it was the only way to do it. As Keller and Schoenfeld put it in their textbook, *Principles of Psychology*, "Extinction is the hallmark of discrimination."

Nevertheless, when we attempted to program discrimination learning, we worked out gradual progressions of stimuli to obtain as close to errorless performance as we could. Even relatively simple discriminations were unmanageably difficult otherwise. Children could not be kept at the task long enough to run off the necessary extinction curve. But here was a paradox. The way we were teaching discriminations in an applied context was not in agreement with the basic research. A graduate student, Herb Terrace, looking for a dissertation topic, saw this paradox, and he carried the problem into the laboratory. He established errorless discriminations in pigeons and began investigating the properties of discriminations established this way as contrasted with the classical procedure. It turned out that the two types of discrimination learning were quite different. Not only was the errorless procedure faster, but the resulting discrimination differed in ways that are important to a systematic understanding of behavior.

Terrace, and the work he stimulated, focused on the properties of discriminations after they were formed. Forming errorless discriminations in the laboratory or in practice was still an art. Not every progression worked. Here, another gap in our knowledge was revealed, and the interplay between laboratory and practice continued. Subsequently, an active area of laboratory research was the determination of the conditions for establishing control by a new stimulus dimension. This work involved a number of people, such as Paul Touchette and Judith Doran, and moved back and forth between laboratory and practice. Studies seemed to indicate that successful fading is not due to an "associative" transfer of control by pairing a controlling stimulus with the new stimulus. Instead, successful fading seems dependent upon the arrangement of conditions that ensure a response contingent relationship with the new stimulus similar to that found in response shaping. Again, we saw that a steady interplay between research and application improved both.

In sum, the analysis of behavior in instruction, from the early teaching machines to today, is an interesting case study of the interplay between basic science and a technology based on science. The effects of contingencies of reinforcement, the nature of shaping, and the analysis of psychological phenomena in behavioral terms were learned from our basic science, which now serve us as we attempt to arrange sequences of contingencies to meet behaviorally-defined educational objectives. Applications spread to the modification of behavior outside educational settings into therapy and social management situations. When practice remains true to the proven principles of the laboratory, impressive gains are made; when basic principles are neglected, the results are less impressive or even embarrassing. At the same time, practice is the ultimate test of theory, and applied behavior analysis in instruction opened new directions that continue to provide a more complete understanding of behavior.

In 2016, with the generous support of ***Operants'*** readers, the B. F. Skinner Foundation started to work on a state-of-the-art interactive version of Holland and Skinner's ***The Analysis of Behavior***. The program will work on all Internet-connected devices and run on all operating systems. It will provide feedback not only to students, but to instructors when students register for courses. The static figures in the paper version will eventually be replaced with improved graphics, video, or animations. The new online platform will also provide detailed data on student performance to enable the Foundation to add helpful features in the future. Currently, the beta-version of the program is being tested in classes. ***Operants*** will publish an update of progress in the near future.

fundraiser

B. F. Skinner Virtual Museum: The Foundation Needs Your Support!

B. F. Skinner's study, just as he left it, will become a part of the video tour prepared for the virtual museum.

The B. F. Skinner Foundation receives many inquiries about Skinner. Researchers, students, and members of the general public request information about his study, his apparatuses, his unpublished materials, and his personal life.

With the technology now available, it is possible to show this material in virtual formats, including print, audio, and video as well as 360º views of objects as they are rotated and operated. The Foundation plans to create a virtual museum, showcasing the major aspects of Skinner's career and personal life. Fortunately, many materials are already available for this project: The Foundation has access to a large number of videos, including one showing special adaptations he made in his home study. The Foundation also has photographs, both professional and personal, and digital copies of all of his articles and most of his books, along with the copyrights to many of them.

Through donations, the Foundation has collected teaching machines, operant chambers, cumulative recorders, and films showing Skinner giving lectures, shaping a pigeon, and discussing ethics and cultural design.

The personal side of this scientist will also be included in the virtual museum. The Foundation will include photos or videos of toys that Skinner made for his children and grandchildren, of his study that has been preserved largely as he left it, of models of apparatuses he made, of artifacts like a mask and props he made for a banquet celebration, and of many other items saved by his family.

The museum will be available online at www.skinnermuseum.org , and is anticipated to be an ongoing endeavor as new materials are found and added to the exposition.

We need your help to get the project off the ground. Our goal is to recruit at least 100 "sustainers" — people or organizations who will commit to monthly donations of at least $25 through 2018. Of course, all one-time donations, large and small, are appreciated as well. Please contribute as much as you can! To set up your monthly contributions, please go to bfskinner.org and press the *Donate Now* button. If you prefer to send a check, please make it payable to B. F. Skinner Foundation and mail it to:

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