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Editors

Autism Service Delivery

Bridging the Gap Between Science and Practice

Springer

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Autism Service Delivery

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Preface

According to the Centers for Disease Control and Prevention (2014), approximately one in six children in the USA has a developmental disability with 1 in 68 diagnosed with an autism spectrum disorder. The societal costs for caring for children with autism are estimated at over $61 billion per year in the USA (Buescher et al. 2014), suggesting a need for high-quality research on assessment and treatment procedures to address this growing public health concern. While applied behavior analysis has emerged as a clear scientifically validated approach to the assessment and treatment of behavior associated with autism spectrum disorders (e.g., Odom et al. 2010; Thompson 2014; Walsh 2011; see also http://www.asatonline.org/treatment/recommendations), the field of behavior analysis has seen much fracturing between the basic and applied wings of its discipline in its recent history. The divergence of behavioral science and practice has been of concern for decades (e.g., Poling et al. 1981; Reed et al. 2014). An unfortunate source of this separation may be the lack of coordinated research interests aimed at translating principles and findings from the basic operant laboratory to the front lines of service delivery (Mace and Critchfield 2010). Toward this end, we were inspired by Murray Sidman's call to educate practitioners on the basic behavior-analytic science that serves as the backbone to applied methods (2011). A noteworthy addition by Sidman is that basic researchers ought to have an understanding of how practitioners use basic science, and what aspects of practice warrant additional inquiry in the highly controlled operant laboratory. This bidirectional approach to advancing behavior analysis thereby serves as the major influencing factor for the format of this book.

The purpose of this book is to compile the most recent research on areas that practitioners tackle in their daily lives when making clinical decisions to benefit individuals with autism. Although existing research and books address this general area, our book is unique in that each topic includes two chapters, one of which summarizes basic research and the other on applied research. Presently, there is no book that synthesizes this literature into a single resource. We identified topics with direct relevance to everyday clinical decisions of practitioners and educators that have a substantial and profound impact on the learning and adjustment of children with autism. The book begins with an introductory chapter on the definition of translation, the importance of highly controlled laboratory research as well as

real-world applied research, and the value of efficacy and effectiveness studies. Remaining topics include stimulus control, transitions, choice-making, conditioned reinforcement and token economies, preference and demand characteristics of reinforcement, behavioral momentum, tolerance for delay to reinforcement, and staff preparation and performance management. We have two goals: (1) to summarize recent and relevant basic and applied research on topics that benefit practitioners and consumers and (2) to stimulate research that addresses the full range of the basic-to-applied continuum on topics of great social importance. In doing so, we hope to influence the development of competent and well-informed scientist-practitioners (both basic and applied)—a la Sidman's vision (2011)—in the behavioral study of autism spectrum disorders.

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Florence D. DiGennaro Reed

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Derek D. Reed

Contents

1 **Origins of Clinical Innovations: Why Practice Needs Science and How Science Reaches Practice** 1
Thomas S. Critchfield, Karla J. Doepke, and Rebecca L. Campbell

2 **Stimulus Control and Generalization** 25
Carol Pilgrim

3 **Stimulus Salience in Autism: A Social Learning Disorder** 75
William H. Ahearn, Diana Parry-Cruwys, Tracey Toran, and Jacquelyn MacDonald

4 **Challenging Behaviors and Task Transitions in Autism: Translating Clinical Phenomenology and Basic Behavioral Process** 113
Dean Williams

5 **Assessment and Treatment of Problem Behavior Associated with Transitions** 151
Kevin C. Luczynski and Nicole M. Rodriguez

6 **Basic Choice Research** 175
Derek D. Reed and Jeffrey H. Tiger

7 **Translational and Applied Choice Research** 193
Jeffrey H. Tiger and Derek D. Reed

8 **Basic Research Informing the Use of Token Economies in Applied Settings** 209
Carla H. Lagorio and Bryan T. Yanagita

9 **Token Economy for Individuals with Autism Spectrum Disorder** 257
Jennifer M. Gillis and Sacha T. Pence

10 Basic Research on the Behavioral Economics of Reinforcer Value 279
Derek D. Reed, Brent A. Kaplan, and Amel Becirevic

11 Preference and Demand Characteristics of Reinforcement: Practical Extensions 307
Nicole M. DeRosa and Henry S. Roane

12 Behavioral Momentum Theory: Understanding Persistence and Improving Treatment 327
Christopher A. Podlesnik and Iser G. DeLeon

13 Implications of Behavioral Momentum Theory for Intervention in Autism Spectrum Disorder 353
Iser G. DeLeon, Christopher A. Podlesnik, and Jonathan R. Miller

14 Delay of Reinforcement: Current Status and Future Directions 375
David P. Jarmolowicz, Jennifer L. Hudnall and Shea M. Lemley

15 Delayed Reinforcement and Self-Control: Increasing Tolerance for Delay with Children and Adults with Autism 407
Seth W. Whiting and Mark R. Dixon

16 Basic Research Considerations for Performance Management of Staff 437
Florence D. DiGennaro Reed, Amy J. Henley, Jason M. Hirst, Jessica L. Doucette, and Sarah R. Jenkins

17 Staff Preparation and Performance Management: Applied 465
James K. Luiselli

Bibliography 491

Index 493

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Chapter 1
Origins of Clinical Innovations: Why Practice Needs Science and How Science Reaches Practice

Thomas S. Critchfield, Karla J. Doepke, and Rebecca L. Campbell

1.1 Introduction

Let us begin by accentuating the positive: Interventions for problems associated with autism have come a very long way. Within living memory, it was common for professionals to tell loved ones of persons with autism that there was no treatment for the disorder (e.g., Bettelheim 1967; Maurice 1993), and not without some justification. Until fairly recently, it could not be claimed on any objective basis that intervening on problems associated with autism was more beneficial than not intervening. At the time two of us (TC and KD) started our careers, many individuals with autism were still being warehoused in soulless institutions where seclusion from society passed for a treatment plan.

Quite obviously, the contemporary world of autism has been shaped—no, defined—by treatment innovation, particularly in the specialty area called applied behavior analysis (ABA). As a recent report documents, among autism interventions that have scientific evidence of effectiveness, the large majority are grounded in ABA (National Autism Center 2009). Yet, as will be explained momentarily, recent advances in ABA do not eliminate a pressing need for improved autism interventions, and those on the front lines of autism service delivery must remain vigilant for behavioral science breakthroughs of relevance to autism.

Treatment innovations can arise from many sources, and we will explain why some sources are more worthy of attention than others. The main purpose of this chapter is to introduce the concept of *translational scholarship* and explain why it should be of intense interest to professionals working in the delivery of autism

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services. Rather than simply define translation, we seek to explore, in some depth, the practical and scientific context into which translation fits as a means of explaining its vital role in helping clinical innovations arise as rapidly as possible. We will conclude by discussing what practitioners can do, beyond simply monitoring others' breakthroughs, to assure that the translation process proceeds at a pace that respects the need for treatment innovation in autism services.

1.2 An Urgent Need for Treatment Innovation

These are good times for autism service delivery. In stark contrast to the days when an autism diagnosis triggered only confusion and hopelessness (Maurice 1993), today's empirically validated autism interventions (National Autism Center 2009) have the potential to radically enhance lives (e.g., Lovaas 1987). The real issue, however, is not whether today's interventions are better than those of the past, but rather whether behavior analytic services are *as good as they can be* (or, at least, as good as they must be to assure acceptable outcomes for every person in need). This is most certainly not the case. Even today's most promising interventions produce different levels of benefits for different individuals, and few individuals in treatment are "cured" of autism (e.g., Lovaas 1987).

Clearly, more remains to be learned about autism and how to devise optimal interventions for persons with this disorder. The heady recent successes of ABA should lead no one to recapitulate the perspective of Physicist A. M. Michelson who, shortly before physics was revolutionized by general relativity and quantum theories, suggested in 1903 that nothing of consequence remained to be learned in his discipline (Coveney and Highfield 1991). Indeed, history teaches that much of what we currently hold as fact will be modified or overturned by advancing science (Arbesman 2012). In autism service delivery, it is reasonable to assume that today's best practices will one day appear antiquated.

This is more than a philosophical point because, in practical terms, *imperfect services do harm*. Any benefits that they confer are partially outweighed by at least four kinds of adverse effects. First and foremost, suboptimal services harm clients by squandering opportunity cost. Lilienfeld (2002) has noted that for every individual there is limited time, energy, and money to support treatment. Services that do not work, or that work incompletely, waste all or part of these finite resources, leaving less to invest in other (possibly better) interventions. The problem is exacerbated with autism because research suggests that treatment outcomes tend to be enhanced when intervention starts during the first few years of life (e.g., Lovaas 1987). Any slippage in intervention effectiveness wastes part of this precious window of opportunity.

Second, suboptimal services harm caretakers of persons with autism. Parents of children with autism tend to have some of the highest stress levels that have been measured (e.g., Estes et al. 2009), and imperfect interventions fail to resolve some of this stress. For example, autism services are expensive and place great pressure

on family finances (James 2013). Relative to better interventions, imperfect ones extend this pressure because they create fewer benefits per dollar spent and may need to remain in place longer to generate benefits.

Third, suboptimal services harm society. It has been estimated that each case of untreated autism costs several million dollars in custodial care, lost work productivity among family members, and so forth (Ganz 2007). Imperfect interventions leave at least some of these costs in place. Moreover, because imperfect interventions are pricey, they may create resentment from a society that experiences many demands on its limited financial resources. Intensive early intervention can cost US$60,000 per year or more per child, leading some observers to object to health insurance coverage of this treatment on the grounds that it will drive up premiums for everyone (Vestal 2013). More effective services, presumably, would offer increased appeal to third-party payers because of their cost efficiency.

Finally, suboptimal services harm service delivery professionals, in part by being bad for business. Consumers who fail to see adequate progress may not return for additional services and probably will not recommend the provider to other consumers. Every treatment failure also is a strike against public perceptions of ABA generally. Given enough failures, any treatment approach gains an unfavorable reputation and may have trouble persuading a skeptical public that it is worthy of trust (Lilienfeld 2002).

Other costs to practitioners are less easily quantified. We have seen many novice service providers wilt under the realization that even their best efforts could not save every client. To state the problem in a more technical way, people get into autism service delivery because they find client progress to be reinforcing. Suboptimal interventions do not offer the richest possible schedule of reinforcement. Moreover, suboptimal services may place practitioners on the wrong side of ethical principles that they generally endorse. For example, some decades ago, prominent applied behavior analysts began asserting that service delivery must respect a client's "right to effective treatment" (e.g., Van Houten et al. 1988). This right is formalized in the 2010 Guidelines for Responsible Conduct (used by the Behavior Analyst Certification Board® and the Association of Professional Behavior Analysts), which emphasize the use of "scientifically supported most effective treatment procedures" (http://www.apbahome.net/ethical_guidelines.php). The "most effective practices" clause implies a preference for effective services over ineffective ones, but in ethical terms how does one categorize an intervention that works for some individuals but not others? Is this intervention ethical when effective, but unethical when ineffective? If an intervention is an unknown mixture of effective and ineffective components, is it ethical or unethical to employ?

Overall, it is reasonable to assert that interventions should be held accountable not simply for being the best *available* but rather for being the best *possible*. Given the costs associated with suboptimal services, and the near certainty that better interventions are possible, every autism service provider should be deeply dissatisfied with the current state of ABA services and hungry for the rapid development of better alternatives. It is of paramount importance, therefore, to determine where better interventions come from so that they may be identified and embraced as quickly as they become available. Below we discuss some possibilities.

1.3 Clinical Origins of Clinical Innovations

One popular view holds that innovations arise naturally from clinical insight (also called clinical intuition), which in turn is thought to emerge from the accumulated field experience of service delivery professionals (Welsh and Lyons 2001). According to this perspective, individuals who most often interact clinically with a particular type of client are in the best position to devise new interventions for them. One aspect of the clinical insight model is not controversial. Clinical experience is essential to effective service delivery, and it is no accident that all major credentialing bodies, including the Behavior Analysis Certification Board®, require persons in training to become service providers to obtain large amounts of supervised experience. Yet to say that service providers are experienced in delivering established interventions is not the same as trusting in their capacity to devise novel interventions that outperform existing ones.

A cautionary tale of clinical intuition comes from the Physician Benjamin Rush, a giant of early American history who is remembered as a signer of the Declaration of Independence, an ardent abolitionist, a penal reform advocate, and an early advocate of public education (Brodsky 2004). More to the current point, Rush was instrumental in professionalizing American medicine and is often regarded as the father of American psychiatry for authoring the first textbook on mental disorders published in the USA (Rush 1812). Unfortunately, Rush also is remembered for the brutal treatments that he administered to victims of a yellow fever epidemic that swept through his home city of Philadelphia in 1793. Guided by a clinically derived theory of disease (see Kopperman 2004), Rush subjected his patients to repeated forced vomiting, chemically induced bowel evacuation, and bloodletting. Although Rush did not invent these therapies, he was unusually enthusiastic in extending them to yellow fever and in achieving unprecedented extremes of treatment frequency and intensity (Kopperman 2004). For example, Rush recommended the draining of up to 85 % of an infected patient's blood (North 2000).

Based on his clinical experience, Rush was convinced that his "innovative" treatments were effective, but contemporary evidence shows otherwise. Yellow fever progresses through an initial stage, marked by vomiting, nausea, fever, and muscle pain, after which about 85 % of those infected recover spontaneously (Monath 2008). The rest proceed to a toxic phase in which mortality ranges from 20 to 50 % (Tomori 2004). Some of Rush's contemporaries objected to his approach, noting (correctly) that, given the tendency of yellow fever to weaken patients through vomiting and disinterest in eating, his treatments likely contributed to mortality by further weakening them (Kopperman 2004). Ironically, as North (2000) notes, conventional treatments that Rush sought to replace included providing lots of fluids (which would have countered dehydration) and a bland diet (which might have addressed disinterest in eating).

About 46 % of Rush's yellow fever patients died (North 2000), a figure that eclipses the mortality rate expected for all yellow fever patients and matches or exceeds the mortality rate for toxic-phase patients. Thus, Rush's clinical intuition

either yielded no improvement over untreated outcomes or constituted a dramatic step backward in yellow fever treatment. To make matters worse, Rush was such a persuasive advocate that his treatments, once controversial, soon were widely adopted (Kopperman 2004). Perhaps not surprisingly, mortality increased in Philadelphia in the years following adoption of Rush's "innovations" (North 2000).

Benjamin Rush was one of the brightest lights of his generation and among the most experienced clinicians of his day, but his clinical intuitions ran contrary to how the world actually works. His story is by no means unique. In mental health services, the insights of experienced service providers have spawned such classics of clinical folly as Freudian psychotherapy, trephining (drilling holes in the skull, possibly to release evil spirits), and rebirthing therapy (which purports to cure virtually any psychological disorder by simulating the birth process). Experienced clinicians have believed deeply in all of these interventions, despite the fact that there is no objective evidence to support their effectiveness.

The world of autism services is no stranger to faulty clinical insights. For example, beginning in the 1940s, psychoanalytic therapists (e.g., Kanner 1943, and especially Bettelheim 1967) began embracing and popularizing the so-called "refrigerator mother" theory of autism, which claimed that the disorder originates in emotionally distant maternal parenting. This theory arose through casual clinical observations and, over the course of many decades, spawned treatments that did not work and caused parents to be subjected to painful and unwarranted blame (see Maurice 1993) for a disorder that, according to current understanding of autism as a neurological disorder, could not have been caused by parenting.

Autism professionals also will be familiar with the travesty that is facilitated communication (Biklen 1992), a purported breakthrough in promoting communication among nonverbal individuals with autism. Facilitated communication arose through the clinical insights of an Australian hospital worker and spread widely in autism service delivery in the early 1990s. Despite overwhelming empirical evidence that facilitated communication does not work (e.g., Jacobson et al. 1995), it continues to be promoted by an academic institute and, apparently, employed widely by enthusiastic adherents.

1.3.1 Vulnerabilities of Clinical Insight

Insight has been defined as a sudden flash of understanding (e.g., Kohler 1925), and it may well be the basis of some important solutions (Metcalfe and Weibe 1987; Root-Bernstein 1989). Yet insight is an unreliable basis for advancing understanding for three reasons. The first reason is that insights arise unpredictably and sporadically (see Critchfield and Twyman 2014), and their origins are not well understood (Metcalfe and Weibe 1987). Even if all insights were brilliantly accurate, there would be no means of assuring that they would arise each time a practical problem required a solution.

The second problem is that not all insights are brilliantly accurate. The psychological processes that generate accurate insights appear to be equally capable of generating erroneous ones (Adcock 1995; Waller 1934). Both accurate and inaccurate insights tend to be accompanied by powerful positive emotions and feelings of certainty (the "ah-ha!" sensation; Metcalfe and Weibe 1987), and nothing in the experience of insight necessarily engages critical thinking about the experience (Adcock 1995).

If insights can be faulty, then of paramount importance is some mechanism for distinguishing between those that are useful and those that are not. A third limitation of clinical insights is that clinical situations rarely provide clear feedback about their accuracy. Insights of clinical interest identify potential cause–effect relationships between clinical problems and factors that may cause or remediate them. As Lilienfeld (2002) has observed, however, human services settings typically make a poor proving ground for cause–effect judgments, in part because interventions can take considerable time to implement and create beneficial changes. Delays intervening between cause (here, the onset of treatment) and effect (the possible emergence of therapeutic gains) are known to impair cause–effect reasoning (Matute and Miller 1998). In the case of an insightfully designed intervention, if a client has not experienced benefits, is this a sign that the intervention does not work, or simply that it has not worked *yet*?

To complicate matters, therapeutic effects, once they occur, are variable. Treatments usually help some individuals more than others (e.g., Lovaas 1987), and even a client on the mend has better and worse days. For some problems, improvement can sometimes occur without treatment. Thus, therapeutic progress is an inherently ambiguous stimulus in the sense that treatment and outcome are imperfectly correlated and this, too, is known to impair cause–effect reasoning (Matute and Miller 1998). In the case of an insightfully designed intervention, if a client experiences benefits, is this the effect of an intervention, or simply a case of spontaneous remission? If a client does not experience benefits, does this mean the treatment does not work for anyone, or that the client is among a minority for whom it is not helpful?

In a nutshell, the problem with clinical insights is not just that they *can* be wrong but also that it is difficult to *tell* whether they are wrong. The powerful emotional responses that accompany insights (Adcock 1995), coupled with the ambiguous circumstances in which interventions are implemented, open the door to illogical tendencies such as the confirmation bias, which involves selectively attending to evidence that fits preconceptions and cherished beliefs (Garb and Boyle 2003). Benjamin Rush certainly fell victim to this bias, seeing significance in patients who recovered following his treatments, and finding reasons to dismiss deceased patients as uninformative about the treatments. Overall, it may be said that, unfettered by external constraints, clinical insight is a breeding ground for illusory and wishful thinking. Services that arise strictly through clinical insight and are supported mainly via clinical anecdote are suspect and should be avoided.

1.4 Research: Insight with Oversight

> If a new drug had just been discovered, it wouldn't be something that would be just thrown out into the market. It would take years of studies before this medication would be marketed. It's the same way facilitated communication should be treated. I mean, why should … people's lives [be] devastated because they're trying it out on us guinea pigs?—Parent of a child with autism, recorded in the film *Prisoners of Silence*. (Palfreman 1993)

Historical experience links a heavy reliance on clinical intuition to stagnation in service delivery fields. In medicine, thousands of years of accumulated clinical experience produced limited cumulative progress until the Renaissance, when early scientific methods first were applied to the study of disease (Siraisi 2012). Medicine began to assume its modern form only in the nineteenth century, when better developed scientific methods could guide its evolution (Fissell 1991). Rapid medical progress in the twentieth century accompanied rapid growth in medical science.

Research thus can be an engine of practical innovation. This is true in no small part because science relies on "insight with oversight."[1] Like clinicians, scientists acquire years of experience interacting with their subject matter and become inclined to draw intuitive conclusions about it (Root-Bernstein 1989). Scientists, however, subject their intuitions to formal tests with the potential to weed out incorrect assumptions about how the world works. Research "oversight," therefore, provides a means of distinguishing between faulty and informative insights. What follows is a discussion of several types of research with the potential to generate clinical innovations.

1.4.1 Clinical R&D: "Pure Applied" Research

Some innovations come from systematic, though relatively atheoretical, efforts to improve on existing technology that, in many cases, has already shown evidence of effectiveness. A familiar example comes from Thomas Edison's laborious work at constructing a commercially viable light bulb. The principles behind creating light from electricity were well understood, and the basic plan for a light bulb already had been worked out. In fact, at least 22 people had devised incandescent light technology before Edison filed his first patent (Friedel and Israel 1986). What remained for Edison was to identify—often through trial and error—materials that were inexpensive and durable enough to make light bulbs that were practical for everyday use. The resulting Edison light bulb was sufficiently derivative of existing technology that Edison became the subject of multiple patent infringement lawsuits (Lemley 2012).

[1] We adapt this phrase from Root-Bernstein (1989), who had a slightly different emphasis when coining it.

Work like Edison's is sometimes called "research and development" (R&D), and may be regarded as a "pure applied" enterprise because it targets "dependable ways of ameliorating social problems," rather than seeking to illuminate fundamental principles (Johnston 2000, pp. 143–144). R&D may consist of formal research (employing experiments to determine whether technologies are effective), but it can also consist of "clinical tinkering" similar to Edison's lengthy process of trial and error. Either way, the motivation behind R&D is to bridge the "distance between a principle or technique that has practical potential and the routine delivery of a consistently effective technology in the marketplace" (Johnston 2000, p. 142).

One purpose of R&D—in line with the quest for ever-better services—is to increase the efficacy of an existing technology. For example, in the decades since functional analysis (e.g., Iwata et al. 1982) was introduced, hundreds of studies have explored its parameters (e.g., duration of assessment, types of assessment conditions) and tested its use with new types of clients and behavior problems and in new settings (Beavers et al. 2013). This type of R&D seeks to maximize the benefits that could be achieved if a technology were widely disseminated.

Other R&D efforts—known variously as "transportability research" (Schoenwald and Hoagwood 2001) or "implementation research" (Fixsen et al. 2005)—seek to promote the dissemination of existing technology. In the case of human services, this can involve modifying an intervention so that it requires no special resources (e.g., staffing, expertise, and materials) beyond what are commonly available in field settings (Schoenwald and Hoagwood 2001).

Although R&D plays an important role in all practical fields, it is intended to refine innovations rather than to spawn them, and truly new technology arises unreliably from this process. No statistics are available on how often R&D leads to genuine innovation in ABA, but Comroe and Dripps (1976) have estimated, based on a study of medical innovations, that only about 17 % of clinical innovations arise through R&D. This means that most R&D does *not* innovate. To illustrate, according to one assessment of military technology, in approximately the past 2800 years, only 11 weapon innovations (about one every 255 years) have emerged that might be called genuinely revolutionary (Herr 2013). Everything else that has been developed in weaponry may be regarded as derivative, that is, as variations on established technological themes.

This is not to disparage "derivative" technology development because innovations in concept rarely change the world. Automobiles existed before Henry Ford got involved in that industry, but they had negligible impact on society until the affordable and (relatively) reliable Model T placed automobile technology in the hands of the masses. Similarly, hand-carried devices to launch gunpowder-propelled projectiles (guns) have existed for centuries, but not until the 1800s were these weapons made accurate and user-friendly enough to be useful in battle (Herr 2013). R&D matters because to change society requires the right variation on an innovation. But with the present focus on the origins, not the perfection, of innovations, we shift attention away from R&D and onto other kinds of research.

1.4.2 Research That Harnesses "Theoretical Oversight"

Comroe and Dripps (1976) suggested that up to four fifths of practical innovations trace to developments in types of research that are driven by theory. We suggest that this is true because these types of research not only exert oversight by empirically evaluating the validity of insights but also place constraints on where insights come from in the first place. To understand this point, it is useful to briefly review what theories are, which can be defined in two clauses.

First, theories are a parsimonious way to make sense of a variety of facts. For example, the theoretical concept of behavioral momentum holds that behavior persistence derives from several factors including recent reinforcement history (Nevin and Grace 2000; for more on behavioral momentum, see Chaps. 12 and 13 in the present volume). Behavioral momentum theory makes it possible to think similarly about animal responses under various laboratory reinforcement schedules, child compliance with requests, addict responses to certain situations associated with drug abuse, responses of basketball players to in-game adversities, and possibly the persistence of resource-intensive personal and cultural habits in the USA (Nevin 1995; Mace et al. 1988; Mace et al. 1992).

Second, theories predict what should be seen in observations not yet conducted: That is, *if* a particular working idea is true, then in a specific set of circumstances certain behavioral effects should be observed. For instance, Mace et al. (1988) wanted to construct interventions to improve child noncompliance with caretaker requests. Noting that many requests that end in noncompliance involve asking children to do difficult things, Mace and colleagues thought of the behavioral momentum concept of a disrupter, which is any factor with the potential to change ongoing rates of some behavior. Examples of disrupters include punishment, changes in physiological state, such as drug intoxication, and, as with tasks that children are often requested to complete (e.g., cleaning one's room), effort. Behavioral momentum theory states that the effects of disrupters on behavior are negatively correlated with the behavior's recent reinforcement history. Mace et al. (1988) reasoned that noncompliance occurs when recent reinforcement of compliance is too lean to counteract the effects of effort-related disruption. They therefore sought to increase the frequency of reinforcement for compliance, but there was a practical constraint: Compliance cannot be reinforced unless compliance first occurs. Based on behavioral momentum theory, they expected that compliance would follow requests for low-effort behaviors (e.g., "Give me five."). A number of these low-effort requests were made, and compliance to them reinforced, before introducing the type of request that tended to have been met with noncompliance. Compliance with high effort increased, as behavioral momentum theory suggests.

As the preceding example illustrates, research that is informed by theory does more than use empirical methods to validate random insights. Theory itself is a source of new insights and, importantly, the deductive process of deriving predictions from theory provides a sort of preemptory "oversight." Theory specifies the

premises on which insights may be based, and therefore limits the range of insights that should arise in the first place. To the extent that a theory is well defined and grounded in credible research, this "oversight" process may reduce the frequency of faulty insights that need to be empirically "weeded out."

A connection between research and theory is most obvious in *pure-basic research,* which seeks to reveal fundamental principles about the world. Basic research is, by definition, driven by and designed to advance theory. Behavior is studied under conditions that promote convenient and precise observation. In operant learning research, a familiar scenario involves an organism (often a rat or a pigeon), housed in a distraction-free chamber in which manipulating a metal lever or depressible disk produces food reinforcers. Such laboratory arrangements are an attempt to isolate, in relatively pure form, behavioral processes that presumably operate in everyday circumstances. The questions posed in pure-basic research focus on identifying core elements of behavior control, not on modeling everyday circumstances or resolving specific everyday problems. Pure-basic research like that of Thorndike (1898) and Skinner (1938) gave behavior analysis its start and continues to serve as its theoretical backbone.

If, as Skinner (e.g., 1938, 1953) always asserted, laboratory-based principles of behavior are potent and highly general, then they should provide essential guidance for analyses of behavior in the everyday world. Indeed, a considerable amount of good has been accomplished in the handful of decades since these principles began making their way out of laboratories and into field settings (Madden 2012; Miller 1985; Rutherford 2009). Comroe and Dripps (1976) estimated that about 36 % of practical innovations trace to basic research, but connections between pure-basic research and practice are more tenuous than might be desired. It appears that only rarely do discoveries make a direct leap from the laboratory to the field, and so it is reasonable to explore just how principles that are revealed in pure-basic research become connected to problems in the field.

The term *translation* describes activities that allow basic research discoveries to inform applied efforts. In due course, we will discuss some of these activities specifically. For now, in order to emphasize the importance of translation, we address the uncertain bench-to-bedside journey of basic principles insights. Speaking of journeys, in 1747, British Royal Navy Surgeon James Lind, addressing the scourge of scurvy that long ravaged the crews of sailing vessels, determined that eating citrus fruits prevented and cured the disease. Unfortunately, it was nearly 50 years before the Royal Navy acted to prevent scurvy by routinely stocking citrus fruits on its ships (Rogers 2004). In 1854, Physician John Snow produced compelling evidence that cholera was caused by poor sanitation, but it took many years for London health officials to authorize construction of modern sewers (Johnson 2007). Such lags between discovery and implementation are not unique to medicine. The concept of reinforcement got its first scientific support from Edward Thorndike (1898) and had been suggested even earlier (see Boakes 1984). Forty years later, Skinner (1938; *The Behavior of Organisms*) detailed the principles of operant learning with much greater precision. Yet a further three decades were required for effective reinforcement-based interventions for clinical disorders to emerge (Rutherford 2009).

Some of the reasons why translation tends to occur grudgingly are not mysterious. The basic and applied wings of a field-like behavior analysis, though linked by a common conceptual system, are for all practical purposes separate professions, with different everyday concerns, different contingencies of survival, and, most important, different social networks (Critchfield 2011c). Basic and applied behavior analysts inspire one another only occasionally because they too rarely engage with each other's work. Historically, basic behavior science articles have infrequently cited applied articles and vice versa (see Critchfield and Reed 2004; Hayes et al. 1980; Poling et al. 1994). When preparing this chapter, we found that historical trends continue. For 2012, 22 % of full-length research articles in *Journal of Applied Behavior Analysis* cited basic research, and 19 % of pure-basic articles in *Journal of the Experimental Analysis of Behavior* cited applied research. Below we elaborate on some reasons for this limited cross talk.

1.5 Impediments to Spontaneous Translation

1.5.1 Limited Attention in Basic Research to Clinical Problems

Although basic science aims to illuminate fundamental principles about how the world works, there is no guarantee that basic researchers will choose to study principles of great everyday importance. In recent generations, basic scientists have argued that the pursuit of knowledge is valuable in its own right, and thus basic science owes nothing directly to application; however, basic science is said to be worthy of societal support because eventually it will become obvious how to better society using the discoveries of basic science (Stokes 1997). Critchfield (2011a, b) suggested that this "Someone, Someday" perspective is self-contradictory: The belief that basic scientists bear no responsibility for addressing practical problems may reduce the chances that basic scientists will choose to study topics that "Someone, Someday" finds useful. Too often, basic researchers fulfill the stereotype of the curmudgeon, holed up in a laboratory, passionately exploring minutia that interest few people other than the researcher.[2]

[2] This is not to imply that broad public appeal is a good index of research importance. In the early 1980s, retrovirology was considered a rather esoteric area of specialization in virology basic research. Only one retrovirus was known to exist, and it was unclear how its study could benefit medical practice generally. When the human immunodeficiency virus (HIV) epidemic emerged, however, and a retrovirus was found to be responsible, retrovirology became the focus of considerable public and scientific interest (Gallo 2006). A tenet of the "Someone, Someday" perspective with which we agree is that it is impossible to prejudge the importance of basic research. It remains true, however, that a considerable amount of basic research appears not to stimulate practical innovations.

Even when basic researchers study phenomena of obvious relevance to the clinical world, the experiments they design may not address the primary challenges of service delivery. Consider stimulus control. In field settings, a central challenge involves programming for generalization of intervention effects to new contexts (Stokes and Baer 1977). Numerous laboratory studies show that probability of generalization positively correlates with the degree of physical similarity between training and test environments (Mostofsky 1965). Practical implications of this research would appear to be straightforward: Make the training setting as similar as possible to generalization settings (e.g., Miltenberger 2004). A disconnect arises, however, due to the fact that most laboratory studies, which were devised to answer theoretical questions, have employed streamlined experimental procedures in which training and test stimuli vary along just one stimulus dimension (e.g., Harrison 1991). By contrast, the setting in which an autism intervention is first employed (a clinic, perhaps, or a child's home), which is likely to become a discriminative stimulus for treatment effects, has numerous salient features (e.g., appearance of the building, type of furniture in a room, common background sounds, people who are present). It thus may differ from other settings along many dimensions simultaneously. Moreover, basic research shows that when a discriminative stimulus has multiple features, it is difficult to predict which feature(s) will acquire discriminative control (Reynolds 1961), and other research suggests that persons with autism are especially prone to restricted stimulus control in which only selected features of a putative discriminative stimulus come to acquire discriminative control (Lovaas et al. 1979). Taken together, these factors make it difficult to apply the "simple" maxim that training and generalization settings should be similar. Programming for generalization remains more art than science (Stokes and Baer 1977; Stokes and Osnes 1989), in part because basic scientists have not asked enough questions about generalization that people in service delivery want answered.

1.5.2 Limited Awareness of Basic Science Among Applied Professionals

Even when basic researchers ask questions that are directly relevant to the everyday world, there is no guarantee that individuals who work in practical settings will be aware of their findings. Basic research usually is published in specialized journals that are read mainly by basic researchers. It uses specialized experimental techniques and is described with specialized technical language. Unfortunately, basic scientists are not renowned for their capacity to simplify basic science for a nontechnical science (e.g., Critchfield and Reed 2009) or for their proclivity for attempting to do this.

In the early days of ABA, there was a high probability that practitioners would gain familiarity with basic science as a routine part of their university training (e.g., Rutherford 2009) because there were no ABA-specific training programs at the time. Many of ABA's pioneers thus had personal experience conducting basic research and were not daunted by reports of basic research. Even those who did not have basic research experience held doctoral degrees, and thus, presumably, had generic skills for teasing apart the details of technical reports.

Things are different now. The modal ABA practitioner holds a master's degree from a mostly applied graduate program that lacks the staff and facilities required to sponsor (or teach) basic research. ABA certification standards at the master's level do not require experience in conducting basic research or even expertise in reading basic research reports (see http://www.bacb.com). Another sizeable group of ABA practitioners hold only a bachelor's degree and are even less likely to have learned how to digest basic research. The overall picture that emerges is of a community of ABA practitioners that is not well positioned to access the fruits of basic research.

It should be obvious from the present section that knowledge produced by basic behavioral science, however valuable in principle to clinical innovation, is not necessarily influential in the clinical realm. Without special assistance, basic research can be the metaphorical equivalent of Gregor Mendel's pioneering research on plant inheritance, bricked up in the wall of an Austrian abbey, informing no one.[3] Special assistance comes in the form of *translational scholarship,* which is a conscious effort to break down walls between basic science and practice by consciously exploring the clinical relevance of laboratory-derived principles.

1.6 Varieties of Translational Scholarship

There are several varieties of translational scholarship, as summarized in Fig. 1.1.

1.6.1 Nonexperimental Approaches

1.6.1.1 Narrative Interpretation

Behavior analysts are familiar with the tradition, popularized by B. F. Skinner, of extrapolating from basic behavioral principles to interpret everyday behavior. Through works like *Science and Human Behavior* (1953), Skinner inspired many to think about how laboratory principles could inform an everyday technology of behavior. Some interpretive accounts suggest behavioral processes that may underpin specific behavior problems such as terrorism (Dixon et al. 2003), alcoholism (Vuchinich and Tucker 1988), pornography (Mawhinney 1998), and conduct disorder (Strand 2000). Others begin with fundamental behavioral processes such as those described in behavioral choice theory and explore the everyday phenomena to which they may be relevant (e.g., McDowell 1982). Narrative interpretation, as a form of translational scholarship, is fuel for the imagination. It proposes a correspondence between what is known from the laboratory and what is observed in the

[3] We take creative license here in presenting a popular but apocryphal version of Mendel's story, the true version of which retains the image of science lost in obscurity. The records of Mendel's experiments actually were burned upon his death, rather than bricked up in the walls of the abbey in which he had worked. During his lifetime, Mendel published just one scientific paper in an obscure journal. Consequently, his work was largely ignored for about 35 years (Carlson 2004).

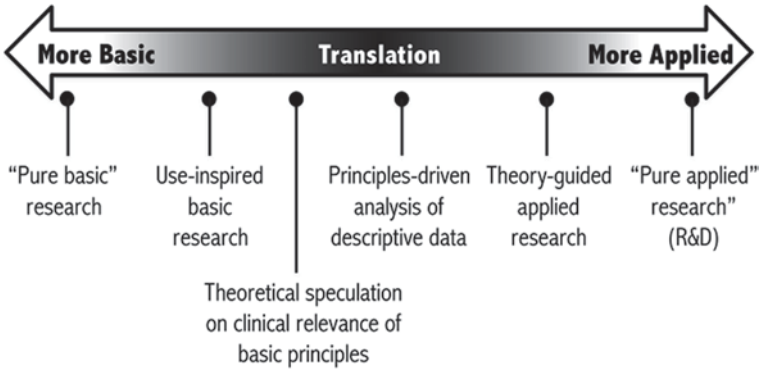

Fig. 1.1 Some varieties of translational scholarship

everyday world, although without empirical evidence there is no certainty that the correspondence is genuine (Baron et al. 1991; Mace and Critchfield 2010).

1.6.1.2 Descriptive Interpretation

In some cases, the relevance of behavioral principles to everyday affairs is examined by exploring formal descriptive evidence from everyday situations. The goal is to see whether naturally occurring behavior conforms to the empirical predictions of laboratory-derived principles—which it often does. For example, descriptive data show that bill-passing legislators follow a pattern that is familiar in laboratory schedules of reinforcement (Critchfield et al. 2001, Critchfield et al. 2015; Weisberg and Waldrop 1977); basketball players divide their offensive efforts between two-point and three-point field goal attempts in ways that are predicted by the model of choice known as the generalized matching law (Alferink et al. 2009; Vollmer and Bourret 2000); and public consumption of energy resources conforms to predictions of behavioral economic theory (Reed et al. 2013). This kind of translation reveals an empirical correlation between patterns of behavior seen in the laboratory and the everyday world, although, in the absence of experimental analysis, there is no guarantee that similar-looking behavior patterns really trace to identical behavior processes (e.g., St. Peter et al. 2005).

1.6.2 Experimental Approaches

1.6.2.1 Use-Inspired Basic Research

It is possible to utilize methods familiar in laboratory science to answer research questions that practical problems suggest. Although the goal remains to shed light

on fundamental principles, the applied problem of interest determines *which* principles are selected for study and *which* aspects of those principles receive attention (Critchfield 2011a, b; Mace and Critchfield 2010). As D. Stokes (1997) has observed, the Biologist Louis Pasteur was a frequent practitioner of use-inspired basic research. Some of Pasteur's work was "pure basic," but some was driven by an interest in such practical matters as industrial beet–sugar fermentation. In behavior analysis, not surprisingly, some of the earliest use-inspired basic research came from B. F. Skinner, including laboratory studies on how drugs of everyday importance affect behavior (Skinner and Heron 1937; Skinner 1959a) and on how behavioral processes result in emotional responses of potential everyday relevance (Estes and Skinner 1941; Skinner 1959b). Practical interests also helped to shape seminal laboratory research on stimulus equivalence (Sidman 1971), delay discounting (Madden and Bickel 2009), and behavioral economics (Kagel et al. 1980). More recently, inspired by clinical concerns, Mace et al. (2010) devised laboratory experiments to explore novel effects of differential reinforcement of alternative behaviors.

Today, it is common to build laboratory models of everyday behavior problems (Davey 1983). Laboratory models have been developed to analyze phenomena as diverse as false memory (Guinther and Dougher 2010), gambling (Habib and Dixon 2010), say–do correspondence (Lattal and Doepke 2001), and analogical reasoning (Stewart et al. 2002). Perhaps the most widely employed laboratory model involves the simulation of drug abuse through drug self-administration procedures (Ator and Griffiths 1987). Overall, the primary contribution of use-inspired basic research is to improve the understanding of behavior principles that are especially relevant to everyday problems. A limitation of this kind of research, from a service delivery perspective, is that revealing important behavior principles is but one building block of innovative interventions. Those interventions still must be created and validated in field settings (e.g., Mace et al. 2010).

1.6.2.2 Theory-Guided Applied Research

Applied research can spawn clinical innovations by drawing upon the fruits of basic science. For example, functional analysis was made possible by a series of early ABA experiments that revealed environmental determinants of problem behavior. These studies were anticipated by Skinner's (e.g., 1953) theoretical interpretations of everyday problems, which in turn were based heavily on basic, laboratory research on how consequences affect behavior (Hanley et al. 2003). We mentioned previously that interventions to increase compliance with requests have been grounded in behavioral momentum theory (e.g., Mace et al. 1988). Similarly, behavior-decelerating interventions employing noncontingent reinforcement have been developed with various aspects of behavior theory in mind (e.g., Virues-Ortega et al. 2013). As these examples suggest, some applied research connects fairly explicitly to basic research. Yet expertise is a constraint on the proliferation of this type of research, as many applied researchers are not well versed in basic research, and thus they are unable to consider the latest laboratory advances when developing interventions.

1.7 The Role of Practitioners in Translation

Takeaway points from the present essay are as follows. (1) Anyone interested in better interventions—and this should include *everyone* involved with autism service delivery—must look to research for inspiration. (2) Basic research reveals the fundamental behavior processes on which effective interventions are founded. (3) However, stakeholders in the service delivery process may not be equipped to digest reports of basic research, and basic researchers are unlikely to provide guidance regarding the everyday applicability of the processes they study. (4) Translational scholarship, in several varieties, takes up the gauntlet of linking basic science to everyday behavior and practical interventions, and it is therefore a valuable source of inspiration to those seeking treatment innovations.

Although translational scholarship ranges from more basic to more applied in scope (Fig. 1.1), all types of translational scholarship are fueled in some way by insights from basic research. The existence of many kinds of translational scholarship indicates that there is no single pathway for these insights to find their way into the field. This means that there are multiple ways for practitioners to be informed by discoveries of basic science without having to conduct or study pure-basic research. A service delivery professional with limited time to read about research—and everyone has limited time—would be well served by seeking out translational work specifically.

This may be easier said than done, because translational work may not be clearly designated as such. It can appear in basic or applied publications and, because academic writers (like this chapter's authors) sometimes have difficulty explaining clearly and succinctly, article titles and abstracts can be an unreliable guide to translational content. Although we can offer no foolproof advice on how to quickly identify the most promising translational sources, we believe that the "urgent need for treatment innovation," mentioned earlier in the chapter, provides sufficient motivation to slog through the needle-and-haystack process of scanning scholarly journals for translational insights.

Another point of concern is that there are too few needles out there: Translational work emerges more rarely than is optimal (Mace and Critchfield 2010; Critchfield 2011a). This is not surprising, as translating requires expertise in both basic and applied domains that simply is not combined in very many people (Mace and Critchfield 2010). To illustrate, Critchfield and Reed (2004) reported that only five individuals accounted for a large proportion of translational articles published in *Journal of Applied Behavior Analysis* during a recent span of years. With relatively few individuals doing the translating for behavior analysis, patience may be the buzzword for practitioners in search of treatment innovations.

And yet, with autism, patience is an expensive luxury. Available evidence suggests that treatment is most effective when initiated at a young age, with therapeutic benefits possibly less reliable and robust for those who begin treatment after a critical window for early intervention (e.g., Fenske et al. 1985). For each newly diagnosed case of autism, there is only so much time for treatment innovations to be developed. Those who provide autism services may therefore wish to consider

adopting a more aggressive approach than simply waiting for others to bring the seeds of innovation to their attention.

Mace and Critchfield (2010; see also Critchfield 2011a, b; Critchfield and Reed 2004) pointed the way to accelerating translation by stressing the value of translational collaboration that brings together in teams individuals who separately represent the basic and applied wings of behavior analysis. In translational collaborations, no one individual must provide all of the needed expertise. Collaborative teams of more basic and more applied experts constitute the standard model of innovation in many domains (e.g., Gregerman 2013). Even the prototypical R&D tinkerer, Edison, was not immune to this kind of collaboration. Although Edison received most of the public credit for many inventions, he regularly interacted with a team of more than a dozen engineers, machinists, and physicists (basic scientists). Edison, therefore, was more the face of a collaborative team than a lone inventor (Burkus 2014).

We do not expect that the modal practitioner will be positioned, by virtue of interest and training, to participate directly in research collaborations of translational import. Those with the right training and skills, however, have the opportunity to recruit scientific expertise into teams that aim for treatment innovation. Service-delivery professionals know the everyday problems that need to be solved and the limitations of existing treatments. Basic scientists may not wish to be directly involved in service delivery but, when informed about the problems of the field, have a strong analytic bent that may aid in matching everyday problems to the most relevant laboratory discoveries. Practitioners and applied researchers have the skills to develop workable interventions based on the match, and applied researchers have the skills to objectively evaluate their efficacy.

Translational teams do not coalesce by accident, however. Someone must bring the relevant professionals together, and historically the bulk of collaborative translation in behavior analysis has been initiated from the applied sector (Mace and Critchfield 2010). There is no reason why at least some practitioners cannot fill this important role.

Practitioners who are not able to collaborate directly with translational teams can make a difference in other ways. Collaborations begin with conversations, and, collectively, members of the burgeoning practitioner community may be able to catalyze important translational conversations. A model for this catalytic role was pioneered beginning in the early 1990s by *Journal of Applied Behavior Analysis,* which invited teams of scholars, often one basic and one applied, to coauthor translational essays aimed at illuminating the applied significance of research that had appeared recently in basic science journals. Many of these authors had never worked together previously, but many of the essays that they produced were frequently read and cited (e.g., Critchfield and Kollins 2001; Fisher and Mazur 1997; Stromer et al. 2000). By using its leverage to force together people with diverse skills, the journal stimulated translational scholarship that might not have emerged otherwise.

The community of practitioners can employ similar leverage. Because practitioners are many, they represent a variety of professional organizations. Because practitioners are linked to considerable fee-for-services dollars, their organizations