

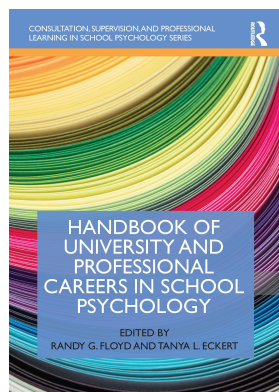
This article was downloaded by: 10.3.97.143

On: 01 Dec 2023

Access details: *subscription number*

Publisher: *Routledge*

Informa Ltd Registered in England and Wales Registered Number: 1072954 Registered office: 5 Howick Place, London SW1P 1WG, UK



Handbook of University and Professional Careers in School Psychology

Randy G. Floyd, Tanya L. Eckert

Developing Programs of Research in School Psychology

Publication details

<https://www.routledgehandbooks.com/doi/10.4324/9780429330964-26>

John H. Kranzler, Jamilia L. Blake, Ethan R. Van Norman

Published online on: 30 Dec 2020

How to cite :- John H. Kranzler, Jamilia L. Blake, Ethan R. Van Norman. 30 Dec 2020, *Developing Programs of Research in School Psychology from: Handbook of University and Professional Careers in School Psychology* Routledge

Accessed on: 01 Dec 2023

<https://www.routledgehandbooks.com/doi/10.4324/9780429330964-26>

PLEASE SCROLL DOWN FOR DOCUMENT

Full terms and conditions of use: <https://www.routledgehandbooks.com/legal-notices/terms>

This Document PDF may be used for research, teaching and private study purposes. Any substantial or systematic reproductions, re-distribution, re-selling, loan or sub-licensing, systematic supply or distribution in any form to anyone is expressly forbidden.

The publisher does not give any warranty express or implied or make any representation that the contents will be complete or accurate or up to date. The publisher shall not be liable for an loss, actions, claims, proceedings, demand or costs or damages whatsoever or howsoever caused arising directly or indirectly in connection with or arising out of the use of this material.

17 Developing Programs of Research in School Psychology

*John H. Kranzler, Jamilya L. Blake, and
Ethan R. Van Norman*

Tenured and tenure-track professors in school psychology training programs are competitively selected and typically have an appointment that is some combination of research, teaching, and service. Although expectations for these duties range from institution to institution, the production of scholarly research is weighted heavily in virtually every important personnel decision, but especially at research universities. “Publish or perish” is the well-known maxim that refers to the pressure experienced by academics to produce a sufficient quantity of scholarly work to sustain and advance in their careers. Tenure-track assistant professors who do not publish a sufficient amount of scholarship are denied tenure and promotion to the rank of associate professor, and tenured associate professors are denied promotion to the rank of full professor. Unproductive faculty also typically compete poorly with their peers for annual merit and other pay increases and tend to have salaries that become compressed.

Over the past three decades, a number of investigations have been conducted on scholarship in school psychology. The main focus of these studies has been the identification of the most productive scholars and training programs in school psychology and the impact of their research (e.g., Grapin, Kranzler, & Daley, 2013; Kranzler, Grapin, & Daley, 2011). It is important to note, however, that the sheer *number* of publications is not the main determinant of being hired into a tenure-track position at a research university, getting tenured or promoted, or establishing a national reputation. Although quantity of scholarly productivity is not unimportant, more significant is the quality, visibility, and impact of a scholar’s *program of research*.

What is a program of research? Faculty with a defined program of research have a coherent and compelling vision and agenda for their scholarship that focuses on a narrow range of core themes. Their scholarship tends to build toward one goal, furthering knowledge in a particular area of research. Developing a program of research is very important, mainly because few studies have a profound impact on the field on their own. Almost always, the results of a number of investigations with a unifying theme are more influential than any single study. In addition to being the most important factor in getting tenured and promoted at research universities, programmatic scholarly productivity is also the primary means by which an academic’s status or national reputation is established among peers in their discipline. Over time, the publication of a number of studies with the same core research themes becomes a sort of personal “brand” upon which a scholar’s reputation is based. It defines them as an academic. The visibility and impact of one’s work is also mitigated by its dispersion. Researchers who conduct a series of projects with no shared themes and whose work does not build on itself tend to have difficulty establishing a national reputation as a leading scholar in any area in which they have published, because they do not become known for any particular line of research.

Each year, applicants for tenure-track faculty positions in school psychology training programs are asked to articulate their program of research during interviews; and tenured and tenure-track faculty are evaluated in large part on the programmatic nature of their research, in addition to its quantity and quality. The absence of a defined program of research is a serious shortcoming in these circumstances. Despite its importance, at the current time, little, if anything, has been written for prospective and new scholars on how to develop a program of research in the field of school psychology. In a qualitative study, Martínez, Floyd, and Erichsen (2011) interviewed a number of the most productive scholars in school psychology. Although all of the respondents stressed the importance of establishing a program of research, they did not discuss how they developed their program of research, how they have sustained it, or how it may have evolved over time.

The purpose of this chapter, therefore, is for us—three scholars who are at different stages of their career—to provide our personal thoughts on the development and evolution of our respective programs of research. We do not intend these to be exemplars, but examples of how three different programs of research were developed by school psychology faculty. Each of us is a tenured or tenure-track faculty member in an American Psychological Association (APA)-accredited school psychology training program at a doctoral research university with high or very high research activity according to the Carnegie Classification of Institutions of Higher Education (Carnegie, 2018). Dr. Ethan Van Norman is an assistant professor of school psychology at Lehigh University; Dr. Jamilia Blake is an associate professor at Texas A & M University; and Dr. John Kranzler is a full professor at the University of Florida. In the next section, each of us addresses the same set of topics related to developing a program of research. After this, we compare and contrast our responses and provide specific recommendations and words of advice for new scholars who wish to establish their own program of program.

Developing a Program of Research in School Psychology

Selecting a Research Focus

Van Norman. My research broadly addresses issues associated with psychoeducational assessment within tiered systems of support. I became interested in assessment research during my third year of undergraduate studies. After taking an introduction to assessment and measurement course, I investigated subfields that leveraged assessment in an applied way. I wavered between the fields of industrial-organizational psychology and school psychology. The potential to help children ultimately won out. I then sought advice from my undergraduate professors regarding people in the field who were doing cutting-edge assessment research. This led me to apply to the University of Minnesota.

At that time, I did not believe I had the capacity to be a successful faculty member, nor did I have ambitions to pursue a career in research. Instead, I viewed the PhD degree as a means to ultimately serve in a leadership role in schools. After 2 years of graduate school conducting assessment research, however, I realized that I might have what it takes to be a faculty member. As I continue to conduct research, my work has evolved to have more direct implications for practice. At first, I was heavily involved in development of psychological assessment instruments. After my first practicum experience and internship, I realized that the field has no shortage of measures or sources of data. I also became aware that the development of new measures is not always an altruistic endeavor and is sometimes at odds with ethical research practices due to the inherent financial conflicts of interest for test developers. It became clear to me that there is a critical gap in the school psychology literature regarding the use of data to make decisions. Much of my research now investigates optimal ways to

collect and interpret data to help school psychologists make good decisions in the schools. For example, a recent line of research I have been engaged in has explored the viability of existing and novel decision-making frameworks to interpret academic progress monitoring data (e.g., Van Norman & Nelson, 2019a, 2019b). Related to universal screening, I have also investigated how best to combine multiple sources of information to identify students at-risk for academic difficulties (Van Norman, Nelson, & Klingbeil, 2017; Van Norman, Nelson, & Parker, 2018).

Blake. My research broadly focuses on how the display of aggression and alienating school experiences, such as school discipline, can impact the educational trajectory and psychological functioning of diverse students. One of my primary lines of research concerns investigating the various ways in which youth aggress and the implications of aggression for both perpetrators and victims from racially and ethnically diverse and socially marginalized backgrounds. I am especially interested in researching the various ways in which children use aggression to attain social status and to achieve their desired social goals and the degree to which their display of aggression results in clinically meaningful levels of social and psychological maladjustment, not only within their peer network but also within the school context as it relates to their academic and behavioral functioning.

My enthusiasm for the study of peer relations initially stemmed from the field's rich social psychological and sociological theoretical traditions, which I find salient to understanding human interactions. However, as I became more familiar with the peer relations literature, I realized that, with few exceptions, many studies failed to consider the significance of race and ethnicity in understanding peer processes, especially with respect to peer-directed aggression and victimization among girls and women. The absence of such studies fueled my early programmatic line of research to examine the causes and consequences of aggression and victimization in youth, the various manifestations of aggression (i.e., social versus nonverbal aggression), and the role of race and ethnicity and gender in understanding these behaviors (Blake, Lease, Olejnik, & Turner, 2010; Blake, Lease, Turner, & Outley, 2012). This line of research led me to broaden my focus to include other groups of students who are at risk for aggression or victimization, with a particular emphasis on peer victimization of students with disabilities (Blake, Kim et al., 2016; Blake, Zhou, Kwok, & Benz, 2016). Through these studies, I learned that, irrespective of their engagement in aggression and victimization, racially and ethnically diverse students and students with disabilities are at increased risk for poor standing on a range of health and educational outcomes. These findings, although not surprising, led me to expand my research to include the examination of disparities in socially marginalized youth's educational attainment and school functioning.

My second line of research examines the disparate impact of school discipline on Black youth and is an outgrowth of my work on aggression. The more I investigated aggressive behavior in Black youth, the more aware I became of the disproportionate discipline practices directed at them in the public schools to manage troublesome behaviors. What I found problematic about the school discipline disparity literature was that the majority of this research centered on the discipline experiences of Black boys, ignoring the experiences of Black girls (Darensbourg, Perez, & Blake, 2010). To that end, my work sought to challenge gender stereotypes regarding student behavior in K-12 settings by questioning whether the protective nature of being a girl is equally afforded to girls of all races in school systems (Blake, Butler, Lewis, & Darensbourg, 2011). My scholarship in this area attempts to unpack the notion that Black girls are shielded from racism in educational settings in order to raise awareness about the vulnerability of Black girls to discrimination in K-12 classrooms, despite their less threatening nature and academic prowess (Blake, Butler, & Smith, 2015). Within this vein, I also investigate how racial bias, whether it be implicit or explicit, may explain discipline disparities from a gendered lens.

Kranzler. My primary area of scholarly interest concerns the nature, development, and assessment of intelligence (“IQ”). Standardized IQ tests have been called psychology’s greatest contribution to society and have long been a cornerstone of practice in school psychology (Benson et al., 2019). In the schools today, individually administered intelligence tests are widely used for the identification of intellectual disabilities, giftedness, and specific learning disability (SLD; Kranzler, Benson, & Floyd, 2016). Despite their prevalence, controversy and disagreement have long surrounded their use and interpretation. Much of my work in recent years has focused on examining the utility of new approaches for determining SLD eligibility, such as those based on the interpretation of patterns of strengths and weaknesses on IQ tests (Kranzler, Floyd, Benson, Zaboski, & Thibodaux, 2016a, 2016b; Kranzler, Gilbert, Robert, Floyd, & Benson, 2019).

When I began my graduate studies at the University of Arizona (UA) in 1983, I did not have a clear research focus. In the early 1980s, however, there was a great deal of heated debate surrounding intelligence and its assessment, particularly concerning its heritability, the so-called nature-nurture issue, and test bias. The question of how and why individuals differ in their intellectual abilities and whether we can assess them fairly for all groups fascinated me. I was also taking a course on the administration of intelligence tests, which only reinforced my interests in the area. Unfortunately, no one at the UA was doing research on intelligence at the time. So, after completing my master’s degree, I transferred to the University of California at Berkeley, which had both an APA-accredited school psychology program and one of the world’s leading researchers in the field of intelligence, Arthur Jensen. By choosing intelligence and its assessment as a focus of research, I have been able to conduct research in an area I find inherently interesting and also pertinent to the practice of school psychology.

Although almost all of my research throughout my career has involved the construct of intelligence or its assessment, my program of research has evolved over time. While at Berkeley, I worked as a research assistant on a number of research projects under the guidance of Dr. Jensen related to the theory that individual differences in intelligence are related to the speed and efficiency of elementary information processing (e.g., Jensen, 2006). My dissertation examined whether individual differences on the general factor that underlies individual differences on all tests of intelligence—called psychometric g —are related to a unitary process or a number of independent processes (Kranzler & Jensen, 1991). Over the next few years, my scholarship primarily consisted of basic research on the nature of individual differences in intelligence, but I also examined the extension of my research on the speed and efficiency of elementary processes to special populations, such as giftedness and SLD (e.g., Kranzler, 1994; Kranzler, Whang, & Jensen, 1994).

Up until this point in my career, I had mainly published the results of my research in journals related to the field of intelligence, such as *Intelligence* and *Personality and Individual Differences*. Although I found this work to be interesting and rewarding, I realized that I was not establishing a national reputation as a researcher in the field of school psychology. Since the mid-1990s to the present, therefore, I have focused my research on more practical issues related to the use of IQ tests in the schools and other advances in psychoeducational assessment (e.g., curriculum-based measures of achievement) that are directly relevant to the field of school psychology (e.g., Kranzler, Yaragchi, Matthews, & Otero-Valles, 2020).

Developing an Overall Plan

Van Norman. I primarily draw upon contemporary validity theory, or more specifically, argument-based approaches to validation as a conceptual framework for my research (Kane, 2016). An over-simplification of the framework dictates that interpretations and uses of test scores to make decisions need to be accompanied by appropriate evidence. The degree to which

the research evidence supports the inferences and assumptions upon which those interpretations are based speaks to the validity of a test to inform a given decision. Before undergoing any research project, I try to ascertain what inference or assumption a potential research question addresses. Doing so can shed light on (a) the necessity for another study to speak to that inference and (b) whether the conclusions from the investigation would contribute meaningful evidence to the claim. The complexity of the inference determines whether a one-off study will be sufficient (e.g., Van Norman et al., 2018) or if a more systematic line of inquiry is warranted (e.g., Van Norman, 2016). I have also found that balancing between the two approaches to be helpful.

At the current time, I am very interested in methods to monitor response-to-intervention. Conducting studies to identify new and optimal ways to measure growth can be a risky endeavor in that results more often than not do not pan out. At the same time, I also conduct research regarding universal screening practices. Although I seek to identify novel practices in relation to universal screening, there are often more specific or narrower lines of inquiry that are understudied but much less risky to spend my time on for the hopes of the project turning into an eventual publication. For example, it may be easier to directly compare the technical adequacy of two instruments that have not been investigated previously (e.g., Klingbeil et al., 2019), as opposed to an article that is more conceptual in nature or discusses topics that require higher levels of quantitative exposition (e.g., Van Norman et al., 2018). I have found that researchers and companies are always developing new measures, and as long as that trend continues, independent evaluations of those measures from individuals without financial conflicts of interest, such as myself, will be warranted.

As a relatively new investigator, managing projects is still a consistent struggle for me. In an ideal world I would act as the head of a laboratory with a number of graduate students under my direction. I have many Excel tables outlining studies created with the best of intentions. The reality, however, is that much of my work is driven by collaborations with school sites, other faculty colleagues, and non-profit organizations. Although imperfect, I tend to compartmentalize the immediate demands of a collaboration and think ahead into the future for 1, 3, 5, or even 10 years about what investigations may be possible. From these potential research opportunities, I will ask graduate students if they would be interested in working with me on the project, given what I know about their interests and ultimate career ambitions. Making myself available to educators seeking assistance has allowed me to serve individuals who need help meeting the needs of their students, as well as to conduct research that addresses broader issues in the field of school psychology.

Blake. My programmatic lines of research are interdisciplinary in nature; thus, I draw from various theoretical traditions that emanate from the fields of diversity science (within social psychology), educational sociology, special education, developmental psychology (e.g., peer relations), and legal studies (e.g., intersectionality). Across my work, race and ethnicity, gender, and disability status are cross-cutting foci and the primary lenses through which I investigate peer and educational processes. I do not adopt a single theoretical or conceptual framework to do so, however. Instead, I let the nature of the research questions under investigation guide the theoretical lens I assume. My selection of research questions is driven by gaps in the literature and my passion for understanding why these gaps exist. I am most interested in conducting studies that answer very nuanced research questions that seek to uncover the mechanisms for racial, ethnic, and disability-based disparities in aggression and education. My formulation of research studies is influenced by the potential of my research to advance understanding of the experiences and issues affecting racially and ethnically diverse and socially marginalized youth, as well as the feasibility of implementing such studies (e.g., access to resources in terms of populations of interest and methods of data collection methods).

I usually spend the summer months developing a set of research goals that I hope to achieve during the upcoming academic year. My project plan consists of developing the research

questions that I am most interested in answering and explaining why they interest me, but perhaps more important, others as well. I use this process to help me evaluate the potential impact of my work and to prioritize the questions that are going to contribute most to advancing the field, even if these advancements are incremental. Once I have settled on what projects I want to do and why, I draft timelines for these projects while taking into consideration my current workload. I weigh the feasibility of projects for implementation and those that are beyond the scope of my current capacity (e.g., limitations in time, funding, or personnel). The most feasible and engaging questions become the final research project(s) that I undertake during the academic year. At the end of each academic year, I repeat this self-reflection and project-planning process. In terms of project management, given my range of research interests, I often have multiple small projects running concurrently that explore related themes and culminate to answer larger research questions surrounding issues of equity. In order to manage these projects—and protect my sanity—I stagger the implementation of discrete research projects to maximize resources so that I am rarely conducting studies that are in the same phase of the research process simultaneously, unless doing so preserves resources (e.g., collecting similar measures from differing samples for two different study purposes).

Kranzler. The theoretical framework for much of my current work is the Cattell-Horn-Carroll (CHC) theory of human cognitive abilities (e.g., see Schneider & McGrew, 2018). Since their inception and until only recently, standardized tests of intelligence had been criticized on the grounds that they were largely atheoretical. Over the past decade, however, researchers and test developers have increasingly used this theory as a framework for both designing and tests and their interpretation. The CHC theory is primarily useful because it provides a common nomenclature for those who conduct research on the constructs measured by tests of intelligence, which facilitates investigations of their clinical utility.

To develop discreet research projects, the most important thing I try to do is to stay current in the field, which means regularly reading the research literature and reviewing newly developed instruments. This is easier than ever with websites like ResearchGate (www.researchgate.net), which allows you to “follow” other researchers and get notified whenever they publish something new. When reviewing the literature, I try to read critically and evaluate the quality, robustness, and validity of the scientific evidence supporting new advances in the field. I am committed to the evidence-based practice movement in school psychology, which aims to “identify, disseminate, and promote the adoption of practices with demonstrated research support” (Kratochwill, 2007, p. 829). Professional practices that are innovative should be substantiated with scientific evidence before they are broadly implemented. When I have concerns or questions about the empirical evidence supporting a new practice or instrument, I develop research projects that are aimed at resolving crucial uncertainties concerning their veracity, often by postulating plausible rival hypotheses to the constructs that are purportedly measured or their clinical utility.

When I was a new assistant professor, my biggest fear was not being able to generate enough ideas for research projects so that I could get tenured and promoted. It soon became clear that the biggest problem is not generating enough ideas for research, but deciding upon which studies to conduct and how to prioritize them. Managing more than one research project at a time is a big challenge. One thing that is important to do is not juggle too many projects at the same time. To do this, I try to have discreet on-going research projects at different stages of completion. That means having at least one research project in the early stages (e.g., developing hypotheses and gathering data), another in the middle stages (e.g., analyzing data and writing papers), and one in late stages (e.g., having a paper under review or revision) at the same time. This can be especially difficult to manage when a worthwhile project serendipitously drops into your lap (e.g., you are invited to write a chapter or contribute to a special journal issue), so it is necessary to work effectively and efficiently with other researchers and students.

Collaboration

Van Norman. I was fortunate to attend a graduate training program with many talented researchers and practitioners. I also carved out somewhat of a niche within my cohort by taking a number of additional statistics and measurement courses. As such, my graduate school colleagues and others I have met since entering the professoriate often reach out to me to collaborate on research projects. In addition, I am usually happy to consult even if it does not lead directly to a publication, with an understanding that other opportunities may arise in the future that overlap with my broader research area. Doing this has led to multiple investigations that arose after an initial collaboration, including some that necessitated collecting new data, on many of which I served as the lead author. I am also affiliated with the Center for Promoting Research to Practice at Lehigh. Several colleagues and I have been intentional in reaching out to school districts regarding the type of research we are engaged in and offering our support. I have worked on multiple projects to help schools better understand their assessment practices as well as assist them in evaluating the impact of their instruction. After establishing a productive relationship and demonstrating our value to the schools, my colleagues and I will often discuss research projects that are congruent with their broader initiatives. Although this approach has taken time, I have found it much more successful than cold-calling principals in neighboring districts.

Mentoring was critical during my early years in graduate school and I continue to seek out guidance from people I have grown to respect and trust. Much of the mentoring I have sought has dealt with advice unrelated to research, however. Regarding research mentorship with my students, I try to take a developmental approach. I attempt to match the level of support I provide with their ultimate career ambitions and the level of pre-existing knowledge they bring to graduate school. Depending on both factors, I initially provide a high level of structure and limit the number of independent tasks I request (e.g., helping with literature reviews and data cleaning). As they progress through the program, however, I gradually increase the number of independent tasks and use less directive requests (e.g., help me with addressing reviewer comments for a revision). By the end of their training, I expect them to be fairly independent researchers and developing their own ideas and conducting their own research investigations with me serving in a primarily supervisory role.

Blake. The interdisciplinary focus of my work facilitates natural collaborations with colleagues both within and outside the field of school psychology. I see the development of collaborative relationships as a courting process that occurs organically, akin to romantic dating. When starting a new collaboration, I usually have a few conversations with potential collaborators to gauge mutual interests and level of commitment in terms of time. I view research collaborations to be very similar to marriage or other long-term relationships in terms of the time and level of commitment required for successfully completing research projects. I learned it is best to start slowly with a small, low-risk project to determine whether the collaboration is a good match before moving forward with larger projects that require significant time and result in multi-year commitments. The colleagues with whom I work the most share my passion for certain topics, have complimentary expertise and the same level of research metabolism that I do (e.g., the pace with which they draft and complete projects and manuscripts), and, frankly, are fun and enjoyable to work with.

I have found collaborations, particularly those that are interdisciplinary, to be highly rewarding, as they have considerably expanded the scope and impact of my work. Interdisciplinary collaborations can reduce the distribution of labor by having more individuals who can take the lead on different portions of the project. By doing so, time can be recaptured in the design and planning of the project and the interpretation of the findings. With that said, interdisciplinary collaborations sometimes present unique challenges. It can take considerable time to

operationalize key concepts when working across disciplinary boundaries, requiring frequent conversations to develop and clarify terminology and conclusions that are meaningful across disciplines. Nonetheless, the challenge of interdisciplinary collaboration has not discouraged me, but it has forced me to reconsider and develop more realistic timelines for the interdisciplinary research projects I undertake.

Kranzler. It is important to build relationships with potential collaborators who compliment my skill set and are easy to get along with. For me, that usually means finding collaborators who have similar interests and more advanced skills in research methods or statistics than my own. I have built effective collaborative relationships with prior students and with other researchers I have come to know over time. Once, at a conference, I pitched an idea to someone whose work I was familiar with, but had never met, about a collaborative project that required a sophisticated understanding of confirmatory factor analysis that he was a noted expert in. He agreed, and we went on to have a highly effective collaboration on several projects that resulted in a number of publications, including one that won article of the year in a leading journal, and along the way developed a friendship that has lasted to this day. I have also found that by working with collaborators who complement each other, different people can take the lead on separate on-going research projects at the same time. Moreover, when writing manuscripts for publication, different people can focus on different parts of the paper that play to their strengths. Consequently, the scholarly work that I have published in collaboration with other researchers is always better than I could have done alone.

In addition, I do a lot of collaborating with doctoral students. Many doctoral programs do not do an adequate job of socializing students for careers in academia. Although most, if not all, provide an adequate amount of training in the requisite declarative knowledge (i.e., ideas, concepts, and theories), some do not do a good job teaching the procedural knowledge (i.e., how professors work) needed by an aspiring academic to succeed. For example, there is seldom instruction in writing grant proposals or in subtle nuances of writing (and re-writing) manuscripts for publication. When I was a graduate student, my mentor was instrumental in socializing me for life as an assistant professor by establishing an apprentice-like relationship, and I try to do the same for my students. I try to provide only as much guidance as needed throughout all phases of the research process, from developing hypotheses, to analyzing data, and ultimately submitting (and re-submitting) articles for publication. Most important, I try to model how to ask good research questions, think critically and independently, and keep an open mind.

Supporting a Program of Research

Van Norman. Given the nature of my research I have found that funding is nice but not critical for my productivity. This may be in part because I have gotten into the (probably bad) habit of being willing to use my own money for things like participant reimbursement and materials cost. I tend to use research funding as a mechanism to fund students (rather than supplement my pay), which then further improves my capacity to conduct research. Being able to offer students funding is an extremely powerful mechanism to recruit highly qualified candidates.

I have been more successful in obtaining external funding for my research than internal funding from my university. Although it may be atypical, this has inadvertently reinforced me to pursue external funding at a greater rate than internal funding. I believe there is merit to the idea of using internal funds to conduct pilot studies to leverage a larger, more competitive grant application to an external funding agency. Alternatively, I have found that if an investigator's idea is good enough and they have a track record of conducting the research they propose, their submission may be viewed more favorably by reviewers who are familiar with their area of research. At Lehigh, the center with which I am affiliated provides support for my research, including workspace for graduate students, technology for teleconferencing, and administrative

support. In addition, I am fortunate to work with colleagues who have an extensive portfolio of externally funded research. They have been instrumental in providing feedback on grant applications and offering advice regarding strategies to pursue external funds. I would encourage anyone pursuing a faculty position to carefully consider the amount and kind of institutional resources that are available to support a program of research prior to accepting any position.

Blake. I have supported my research with both external and internal funding. Although it is definitely possible to conduct the research I do without grant funding, having internal and external grants has increased my capacity to obtain larger and more robust samples to explore my research questions with diverse populations. I use internal grant funding primarily to conduct a small scale preliminary study. For most external funding agencies, pilot data are beneficial for demonstrating the feasibility a full-scale research project and can greatly facilitate securing grant support. I am fortunate that there are a number of resources within my college and institution that support my ability to obtain external funding. My institution offers grant writing workshops regularly, and my college has a research development office that provides training on how to target specific funding agencies for particular projects. One unique program offered in my department was a workshop series for assistant professors that was led by a full professor who was highly successful in securing external grants to teach how to develop funding proposals for particular agencies. These experiences encouraged my pursuit of and later success in securing internal and external research funding.

Kranzler. My program of research is, at the current time, largely conducted without the support of funding. Although I have received a number of external and internal grants over the years to support my research, this was mainly when I was a junior faculty member and expected to bring in some external funding. To this end, in the 1990s, I collaborated with pediatric endocrinologists on a number of research projects related to the psycho-social functioning of children and youth with short stature, mainly because there was external funding available (e.g., Kranzler, Rosenbloom, Proctor, Diamond, & Watson, 2000). Although I found this research to be very interesting and worthwhile, it was never my primary interest or focus of my research. Over the past decade or so, my research has been in an area that is difficult, if not impossible, to support with external funding. I am not aware of any federal sources of support for research in the field of intelligence. In fact, some researchers have said that external funding agencies are biased *against* research on intelligence (e.g., see Haier, 2016).

How does one conduct research that is largely unfunded? One avenue for research is to use standardization data from test publishers. Although not all publishing companies are amenable to sharing data, some are. Standardization datasets are large and usually nationally representative, so they tend to be excellent sources of data for research. Another method is to locate archival data (e.g., in schools) that can be for research projects. Sometimes this can be frustrating, however, because you have to alter the question you want to ask depending on the data available. When data need to be gathered for a project, however, I tend to rely on graduate research assistants who are either assigned to me (e.g., as a requirement for a fellowship), in need of early research experience, or in search of dissertation topics. Through the use of these methods I have been able to focus on conducting the research that I am most interested in and not be forced to change the aims of my research to fit fundable topics that are not of central interest to me. That said, I am very fortunate to work in a university that provides me with most of the resources I need to conduct research in terms of time, space, and materials. Nonetheless, there are times when I have to be creative to find possible sources of data or ways to gather them.

Important Lessons Learned

Van Norman. I have found that the only way to conduct research is to spend sufficient time doing it. Following the lead of my colleagues at Lehigh, I have been explicit in prioritizing and

protecting my time to work on projects. I have found that forcing myself to work on research in part by meeting explicit deadlines (often via collaboration with colleagues) has helped in holding myself accountable. Related to competing demands, I have learned to allocate a finite amount of time to things like course preparation and grading. Without compromising overall quality, I have found that being willing to live with “good enough” is important to protect time dedicated to research.

Reflecting back on my career thus far, I realize that I did not appreciate much of the advice I received when it was first given. Once, for example, when I was particularly agitated by the outcome of an editorial decision, a more senior colleague told me to relax and said that the race to tenure is not a sprint, it is a marathon. He also quipped that none of us in the department are B. F. Skinner. At first I shrugged off his comments, perhaps because I was still angry and wanted to wallow in self-pity, convinced that I would not publish a sufficient quantity or programmatic research to get tenure. Looking back, I realize how ridiculous this sequence of events was and have since taken that colleague’s advice to heart. I would recommend to anyone who is in the process of developing a program of research to critically ask themselves what they are interested in researching and how they realistically would like to move the field forward. Rather than chasing any “shiny new” project that may or may not be related to that topic, be selective in the endeavors you undertake and ensure they align with your goals. Not all projects are equally meaningful for developing your program of research. I would also encourage any aspiring academic to think critically about whether to become involved with collaborations proposed by colleagues that do not directly align with their intended program of research, even if the prospect of a course release from teaching or additional salary is being offered.

My colleague also pointed out that the same advice applies to obligations neatly rebranded as “opportunities” that detract from your time to conduct research. For example, serving as a co-investigator on a funded project seems to be a wise decision early on in one’s career. However, it is critical to ensure that the amount of work required for the project matches your compensated effort and that an outside evaluator can make some reasonable connection between your program of research and the project. In addition, it is also critical to ensure that there are some demonstrable pieces of scholarship (i.e., manuscripts) that reflect your time and effort from the endeavor.

Another recommendation I would offer to individuals seeking a tenure-track job is to think critically about the colleagues with whom you will be working, and the overall climate of the department in which you will be working, with the understanding that no place is perfect. At the very least, a position should actively protect you from politically contentious and time-consuming service requirements early in your career, as well as limit the number of new course preparations you are asked to complete. Both, I have found, are the biggest negative influences on productivity.

Blake. I do not believe that there is a formula for developing a successful program of research. We all have different definitions of what a successful program of research means. For some, success is measured by productivity, as reflected in the number of publications; for others, it is the satisfaction and prestige that comes with publishing in top-tier journals; and for yet another segment of scholars, success is based on the impact of their work, the degree to which their research advances the field or positively promotes scholarship, policy, or practices for a particular population. With that said, my advice for building a successful program of research is for early career scholars to define what success means to them beyond the requirements for promotion and tenure or merit at their respective institutions. Although there is more freedom to fully explore this question post-tenure, they should ask themselves what success means at various points throughout their professional careers.

I define success in terms of (a) the potential impact my work has on the individuals I recruit to be part of a research study, my students, and the colleagues with whom I collaborate; and (b)

the ability of my research to advance science, even incrementally, to increase awareness, change beliefs, and influence policy and practice. Once a scholar creates their own definition of what a successful program of research entails, to achieve and maintain that success, I recommend early career scholars persist through rejection of work submitted for publication, learn to distinguish between constructive criticism (which improves the quality of your work and clarity of your messaging) and non-specific and negative criticism from reviewers, and seek out publication outlets in fields that are receptive to the type of work that you conduct.

Early in my career, I experienced a number of failures with respect to publishing. When I first received rejection notices and feedback from reviewers, I felt defeated. I wrestled with the thought of not publishing my work in major journals in school psychology, because I viewed the reviewers for these journals as experts. However, each time I received a rejection, I would push myself to revisit the reviewers' comments and deconstruct their criticisms to identify what was constructive and consisted of specific feedback that I could use to revise the manuscript (e.g., voice, clarity and communication of wording, and clearer connections), what was backed by science versus opinion (e.g., comments that pointed to published scholarship or theory that I could read versus comments that reflected the reviewer's personal views about the work that were not rooted in empirical evidence or theory), and what was non-specific. I focused only on the constructive comments, as well as comments and questions that were connected to the literature. If the reviewers had more non-specific than constructive criticisms, I decided quickly that my work was not a good fit for that outlet and I submitted future manuscripts to a different journal. Moreover, I focused on developing high-quality work that could have an impact on the field and shed light on the experience of disenfranchised groups. That persistence paid off for me. Although my work was not widely recognized initially, it has eventually had the level of impact on the field that I had hoped for.

Kranzler. The most important lesson I have learned is to try to manage the competing demands on my time. To do so, I organize my schedule so that I have blocks of time for different tasks related to research, teaching, and service, as much as possible. Ideally, I like to have full days for writing so that I can get completely submerged into what I am working on (both reading and writing). It does not always work out, but I try to arrange to teach my classes on the same day. I also try, whenever possible, to complete any course-, program-, and service-related (e.g., journal reviews) work on the same day. In addition, another key factor related to productivity is that I can teach in my area of research, as I am the instructor of the intellectual assessment course in the program. This way, my research informs my teaching and vice-versa. Students also benefit from having an instructor who does research in the area, because they tend to have a greater depth of understanding of the literature. Over the course of my career, I have to limit my professional service to serving on journal editorial boards or as associate editor. Also, as a reviewer, I try to review articles that are related to my areas of research interest as much as possible. Last, with regard to student mentoring, so I work mainly with those students whose research interests overlap with mine. If I am not a content expert in their area of research, I ask them to find a co-chair for their committee who is.

Recommendations for Developing a Program of Research

Selecting a Research Focus

Selecting an area of research is an extremely important decision for any prospective academic. One of the foremost things that all three of us have in common when it came to selecting a research focus is that we were intrinsically interested in the topic and work. Whatever area of research you choose to work in, you will spend a lot of time and energy on it, and if that area does not interest you and you do not enjoy it, or at least care about it, your career will be more

difficult, because you will not be passionate about your program of research. When it came to defining our programs of research, each of us was initially interested in a more general area of psychology that was subsequently refined to address contemporary issues in the field of school psychology. Van Norman was initially interested in assessment in psychology in general, which led him to school psychology because of its prominent role in practice. Although he was initially interested in test development, Van Norman narrowed his interests after realizing that there is no shortage of available or new instruments to address important issues related to the use of psychological tests for data-based decision-making in the schools. Blake's initial interests, in contrast, stemmed from research and theory in social psychology. From within that theoretical framework, she extended prior research to address critical gaps in the literature pertaining to the role of race and ethnicity and gender in aggression and victimization. Her research has evolved over time to include other groups of vulnerable children and youth, particularly those with disabilities and to focus on school discipline disparities. Last, Kranzler's program of research began with a focus on basic research on intelligence and shifted to address more practical issues related to the interpretation and use of IQ tests in the schools.

When choosing an area of research, it is important to keep in mind that just because you find something interesting does not mean that others will too. A lack of literature on any particular topic does not justify research on it. Choosing an area of research that is tangential or not directly relevant to school psychology should be weighed carefully when seeking an academic position in the field. Areas of research that are not mainstream may be difficult to get published in major journals in school psychology or accepted for presentation at national conferences. If this is the case, then it will be difficult, if not impossible, for an early career scholar to establish a national presence and have an impact on the field of school psychology. Moreover, many school psychology programs, particularly those in research universities, will not be interested in hiring someone whose research is not likely to have an impact on the field, regardless of its potential importance more generally, because it will likely not improve the stature of the program.

One way to be certain that your research will be of interest to the field is the existence of recent publications in major journals in school psychology on the topic. Research on topics that are not of interest or potentially important will not be published in the leading journals. If you are interested in a new and emerging area, however, it may be difficult to judge the general interest of the field. If this is the case, then you need to ask yourself: Is this area of research potentially important for school psychology? Will it facilitate understanding of an important school-related phenomenon? Will it improve the delivery of school psychological services? If you are unsure about the answer to one or more of these questions, talk to other students, faculty, and practitioners and get their opinion. In the end, however, it's a personal decision that you have to make.

Our recommendation: Select an area of research that is intrinsically interesting to you and one that is also interesting generally to the field of school psychology.

Developing an Overall Plan

A research plan is a thoughtful, compelling, blueprint that provides a framework for developing ideas for research. You should be able to articulate what your plan of research will be over the next half decade or so to advance knowledge in school psychology. For Van Norman and Kranzler, their approach to developing discrete research projects is related to the evidence-based practice (EBP) movement and the construct validity of psychological tests. Establishing validity involves the examination of underlying theoretical rationales and empirical evidence supporting the interpretation and use of on test scores (e.g., Messick, 1989). Although they tend to do research on different kinds of psychological instruments, both Van Norman and Kranzler use theory of test validation to develop research projects by asking critical questions of the scientific evidence supporting new tests and assessment practices. They develop research

projects to address crucial uncertainties or rival hypotheses for new tests and their proposed uses. Van Norman, however, also generates ideas for research projects through collaboration between the research center with which he is associated and nearby school systems. In contrast, because her work is largely interdisciplinary, Blake uses a number of theoretical frameworks in different fields. In other words, the research question under investigation determines the theoretical framework she uses on different projects. At all times, however, her research is focused on addressing important gaps in the literature related to aggression and education, particularly those concerning racial and ethnic minority students and children and youth with disabilities. What all of us have in common is that our respective programs of research are largely driven by the identification of unanswered questions or hypotheses that are in need of investigation, the answers to which will advance the field of school psychology.

In addition to the development of an overall research plan, another important thing for early career scholars to think through is how to manage more than one research project at the same time. Results of a recent study on research productivity found that scholars in the field of school psychology publish more than one refereed journal article per year, on average, with the most productive scholar in the field averaging more than eight publications per year (Grapin et al., 2013). In their survey, Martínez et al. (2011) found that the most productive scholars in the field tended to be particularly adept at managing multiple research projects simultaneously. As Van Norman noted, however, managing research projects can be a challenge. Both Kranzler and Blake underscored the importance of staggering research projects to make conducting their program of research more manageable. Blake's strategy is to formulate a plan for research for the upcoming academic year each summer. She develops a list of the research questions to investigate and a timeline so that discrete projects can be arranged so that the same phase of research is not occurring at the same time (e.g., data collection). Keep in mind that the number of research projects that can be done at the same time varies by researcher and the hypotheses under investigation, even among the most highly productive scholars. It is also important to note that, in the beginning stages of a career, it is not uncommon to overestimate what one can do in a certain amount of time. You have to learn over time what is manageable for you, so that you can still reach our goals and have a successful program of research. In addition, regardless of the number of research projects that can be undertaken at the same time, it is essential that your research focuses on a narrow range of core themes that reflect your program of research.

Our advice: Develop a systematic and planful program of research that is both manageable and focuses on a narrow range of core themes.

Collaboration

Results of recent research on productivity revealed that scholars in school psychology tend to collaborate extensively. Grapin et al. (2013) found that the number of authors on refereed journal articles ranged from 1 to 14 over a 5-year period, but the vast majority of articles published had between two and four authors ($M = 3.4$). During that period only 18% of faculty in APA-accredited school psychology programs published at least one single-author journal article. Thus, collaboration in school psychology is commonplace—and a key part of many of the most productive scholars' success. Each of us engages in a considerable amount of research collaboration but have gone about developing those relationships somewhat differently. Each of us also collaborates extensively with graduate students and has similar mentorship styles, which tends to involve gradually introducing them to the research process and providing opportunities to assume more responsibility and creative input on projects over time.

We would also like to share a few words of caution about collaboration, however. Being someone with whom others can collaborate can be an advantage for your program of research, but relying on others for the success of your projects is unwise. Be cautious about continuing

collaborations that you have already established (especially with your mentor), as well as starting new ones, on which you are not the lead researcher. To obtain tenure and promotion and establish a national reputation, you will need to demonstrate your own independent thinking and creativity. When collaborating, being the lead researcher on a number of different projects will make that clear to others evaluating your body of work. Second, avoid getting sidetracked. There are lots of great opportunities for collaboration, and it is tempting to say “yes” to all of them. As Van Norman stated, do not chase every “shiny new” project that presents itself. Keep in mind that if you are going to have a productive program of research, you have to be strategic. It is a critical skill to learn how to say “no” if a project does not align well with your research plan and to do it in a way that does not burn bridges for future collaboration.

That said, although some of these opportunities may be a distraction or a waste of time, others may be very appealing and might lead to an entirely new and exciting line of research. Science is continuously advancing and is often full of surprises, so it is important to both commit to a program of research but, at the same time, be open to new ideas. Don’t put on blinders and miss out on important opportunities for research. Thus, begin your career with a focused research plan, but be ready to deviate and embrace new ideas and opportunities as they present themselves. It is important to note that all areas of research go through cycles of growth and decline, and it can be challenging to understand or predict these trends. Scientists often reinvent their programs of research over the course of a 30-year career, so it is wise to assess which of these opportunities is realistic and viable, because they could be the core of your next program of research. If you do change your program of research, make sure that your new series of projects can be seen to share a common theme and to build upon itself to ensure the establishment of a national reputation in a new area.

Our advice: Collaboration can play an integral role in a successful program of research, but be sure to demonstrate your own independent thinking and creativity in your area. Also, commit to your program of research, but be open to new opportunities.

Supporting a Program of Research

The three of us vary in terms of the amount of funding that is needed to support our programs of research. Van Norman has been successful in obtaining external funding to support his program of research, although it is not crucial, given the nature of his work. He stated that external funding is more important for supporting graduate students than for carrying out his own research agenda. In addition, the research center with which he is affiliated is an excellent source of institutional support for his projects, providing space, technology, and administrative support. Similarly, Blake stated that, although she is also not overly dependent on external funding to conduct her program of research, obtaining external and internal funding has enabled her to conduct her research on a larger scale. Kranzler, in contrast, conducted a number of interdisciplinary projects early in his career with researchers in other fields, mainly because of the opportunity for external funding to meet expectations for promotion at his institution. More recently, however, he has largely conducted unfunded research in his main areas of research, because of the absence of external funding in that area, so he must be somewhat creative in designing projects that are feasible.

An important element for early career scholars is to know the amount of funding and institutional supports that are needed to conduct their program of research. To this end, it is worthwhile to have knowledge of (a) how sources of academic funding and external agencies (e.g., Institute of Education Sciences) select research proposals for funding and (b) the types of research that the various agencies, foundations, and corporations tend to fund. A good way to gain experience in writing grant proposals for external funding is engaging senior mentors, co-writing and co-reviewing proposals with them, and discussing proposal writing strategies,

proposal strengths and weaknesses, and specific funding agencies or organizations to target for one's program of research with them. When searching for an academic position, some institutions will be interested in the fundability of your line of research. In these instances, a detailed plan to fund one's work is usually not needed, but a credible, hypothetical plan is essential. So it is advisable to think through what you need to conduct your program of research before searching for academic positions.

Our advice: Be knowledgeable about the amount of external and internal support available to conduct your program, and be sure that is obtainable.

Important Lessons Learned

It is very easy to get over-committed as a professor. You can get pulled in many different directions and asked to do a number of different things that are unrelated to your program of research. Conducting a program of research requires large amounts of sustained concentration. Kranzler is fortunate in that he has been able to do service and teach one or more courses in his area of research so that there is some degree of synergy among them. In this way, his research informs his teaching and service and vice-versa. This is one of the main advantages of being a full professor—being able to fashion your work environment over time to complement your program of research to some extent. Not all faculty, and especially junior faculty, are so fortunate, however. Therefore, one of the most important lessons learned by each of us over the course of our careers is the need to carve out large blocks of uninterrupted time for research. Rather than scheduling research time around other commitments, we try to schedule other activities around our research time. Both Blake and Van Norman are particularly strategic about grouping meetings, classes, and office hours on certain days of the week to protect the chunks of time they need to focus on their research.

Another important lesson learned by Van Norman is to carefully consider the fit between what you are looking for in an institution and that institution's expectations. This includes contemplating the fit between yourself and the colleagues you could be working alongside as well as expectations for teaching and service. Preparing a new course, for example, is a tremendously time-consuming activity. Knowing in advance what the expectations will be for teaching and service is important to know in order to decide whether you will have sufficient opportunity to pursue your program of research after being hired. It is important to note that not all institutions have the same expectations for research, teaching, and service, and that expectations for research productivity at a university with very high research activity may look quite different from those at other institutions.

Both Van Norman and Blake noted that one of the most important lessons they have learned thus far is how to deal with feedback on and criticism of their research. Submitting a scholarly work for publication reflects a tremendous amount of time and energy and the stakes are high, particularly for junior faculty. It is important to keep in mind that fields of science advance through the process of conjecture and refutation. Scientists make specific claims (hypotheses) using evidence (empirical data) to justify their arguments. Other scientists then identify weaknesses and limitations in their contentions, with the ultimate goal of refining and improving scientific explanations and research methods. The peer-review process is not perfect, and sometimes reviewers may provide feedback that is arguable, at best, or not particularly constructive. As Blake stated, it is important to separate fact from opinion and objectively consider the feedback you receive during the peer-review process and not become defensive. Usually, revising a manuscript in response to reviewer feedback results in an improved product. Sometimes, however, re-submitting your original manuscript to another journal is the best option. An important factor to consider is whether revising a manuscript alters the substantive conclusions of your research and its potential impact on your program of research.

Table 17.1 Summary of Recommendations for Developing a Program of Research in School Psychology

Select an area of research that is intrinsically interesting to you and one that is also interesting generally to the field of school psychology.
Develop a systematic and planful program of research that is both manageable and focuses on a narrow range of core themes.
Collaboration can play an integral role in a successful program of research, but be sure to demonstrate your own independent thinking and creativity in your area.
Commit to your program of research, but be open to new opportunities.
Be knowledgeable about the amount of external and internal support available to conduct your program and be sure that is obtainable.
Be proactive about protecting your time devoted to research and integrating research with other responsibilities as much as possible.
Be objective when evaluating feedback on your program of research.

Our advice: Be proactive about protecting your time devoted to research and integrating research with other responsibilities as much as possible. Be objective when evaluating feedback on your program of research.

Conclusion

Developing a coherent and compelling program of research that focuses on a narrow range of core themes is extremely important, particularly for faculty at research institutions. Not only are the results of programmatic research more likely to have a profound impact on the field, but also the publication of studies with the same core research themes becomes the “brand” upon which a scholar’s reputation is based. Table 17.1 summarizes our recommendations for developing a program of research in school psychology. Following these recommendations will not only assist early career scholars in establishing their program of research but also increase the chances that the results of their research will lead to improvements in the quality of school psychological services delivered to children and youth, families, schools, and communities.

References

- Benson, N. F., Floyd, R. G., Kranzler, J. H., Eckert, T. L., Fefer, S. A., & Morgan, G. B. (2019). Test use and assessment practices of school psychologists: Findings from the 2017 National Survey of Assessment Practices in School Psychology. *Journal of School Psychology, 72*, 29–48.
- Blake, J. J., Butler, B. A., Lewis, C. L., & Darensbourg, A. (2011). Unmasking the inequitable discipline experiences of urban Black girls: Implications for urban stakeholders. *Urban Review, 43*, 90–106.
- Blake, J. J., Butler, B. R., & Smith, D. (2015). Challenging middle class notions of femininity: The cause for Black females’ disproportionate suspension rates. In D. Losen (Ed.), *Closing the school discipline gap: Research to practice*. New York: Teachers’ Press.
- Blake, J. J., Kim, E. S., Lund, E. M., Zhou, Q., Kwok, O., & Benz, M. R. (2016). Predictors of bully victimization in children with disabilities: A longitudinal examination with a national dataset. *Journal of Disability Policy Studies, 26*, 199–208.
- Blake, J. J., Lease, A. M., Olejnik, S., & Turner, T. L. (2010). Ethnic differences in parents’ attitudes toward girls’ use of aggression. *Journal of Aggression, Maltreatment, and Trauma, 19*, 393–413.
- Blake, J. J., Lease, A. M., Turner, T., & Outley, C. W. (2012). Exploring ethnic variation in the adjustment patterns of aggressive girls. *Journal of Black Psychology, 38*, 104–131.
- Blake, J. J., Zhou, Q., Kwok, O., & Benz, M. R. (2016). Predictors of bullying behavior, victimization, and bully-victim risk among high school students with disabilities. *Remedial and Special Education, 37*, 285–295.

- Carnegie (2018). *The Carnegie Classification of Institutions of Higher Education*. Retrieved from <http://carnegieclassifications.iu.edu/>.
- Darensbourg, A., Perez, E., & Blake, J. J. (2010). Overrepresentation of African American students in exclusionary discipline: The role of school-based mental health professionals in dismantling the school-to-prison pipeline. *Journal of African American Male Education, 1*, 196–211.
- Grapin, S. L., Kranzler, J. H., & Daley, M. L. (2013). Normative assessment of the research productivity and scholarly impact of faculty in APA-accredited school psychology programs: 2005–2009. *Psychology in the Schools, 50*, 87–101.
- Haier, R. J. (2016). *The neuroscience of intelligence*. New York: Cambridge University Press
- Jensen, A. R. (2006). *Clocking the mind: Mental chronometer individual differences*. New York: Elsevier.
- Kane, M. T. (2016). Explicating validity. *Assessment in Education: Principles, Policy and Practice, 23*, 198–211.
- Klingbeil, D. A., Maurice, S. A., Van Norman, E. R., Nelson, P. M., Birr, C., Hanrahan, A. R., Schramm, A. L., Copek, R. A., Carse, S. A., Koppel, R. A., & Lopez, A. L. (2019). Improving mathematics screening in middle school. *School Psychology Review, 48*, 383–398. 10.17105/SPR-2018-0084. V48-4.
- Kranzler, J. H. (1994). Application of the techniques of mental chronometry to the study of learning disabilities. *Personality and Individual Differences, 16*, 853–859.
- Kranzler, J. H., Benson, N., & Floyd, R. G. (2016). Intellectual assessment of children and youth in the United States of America: Past, present, and future. *International Journal of School and Educational Psychology, 4*, 276–282.
- Kranzler, J. H., Floyd, R. G., Benson, N., Zaboski, B., & Thibodaux, L. (2016a). Classification agreement analysis of cross-battery assessment in the identification of specific learning disorders in children and youth. *International Journal of School and Educational Psychology, 3*, 124–136.
- Kranzler, J. H., Floyd, R. G., Benson, N., Zaboski, B., & Thibodaux, L. (2016b). Cross-battery assessment pattern of strengths and weaknesses approach to the identification of specific learning disorders: Evidence-based practice or pseudoscience? *International Journal of School and Educational Psychology, 3*, 146–157.
- Kranzler, J. H., Gilbert, K., Robert, C. R., Floyd, R. G., & Benson, N. (2019). Further examination of a critical assumption underlying the dual discrepancy/consistency approach to SLD identification. *School Psychology Review, 48*, 207–221.
- Kranzler, J. H., Grapin, S. L., & Daley, M. L. (2011). Research productivity and scholarly impact of APA-accredited school psychology programs: 2005–2009. *Journal of School Psychology, 49*, 721–738.
- Kranzler, J. H., & Jensen, A. R. (1991). The nature of psychometric g: Unitary process or a number of independent processes? *Intelligence, 15*, 397–422.
- Kranzler, J. H., Rosenbloom, A. L., Proctor, B., Diamond Jr., F. B., & Watson, M. (2000). Is short stature a handicap? A comparison of the psychosocial functioning of referred children and non-referred children with normal short stature and children with normal stature. *Journal of Pediatrics, 136*, 96–102.
- Kranzler, J. H., Whang, P. A., & Jensen, A. R. (1994). Task complexity and the speed and efficiency of elemental information-processing: Another look at the nature of intellectual giftedness. *Contemporary Educational Psychology, 19*, 447–459.
- Kranzler, J. H., Yaraghchi, M., Matthews, K., & Otero-Valles, L. (2020). Does the response-to-intervention model fundamentally alter the traditional conceptualization of specific learning disability? *Contemporary School Psychology, 24*, 80–88. doi:10.1007/s40688-019-00256-x.
- Kratochwill, T. (2007). Preparing psychologists for evidence-based school practice: Lessons learned and challenges ahead. *American Psychologist, 62*, 829–843.
- Martínez, R. S., Floyd, R. G., & Erichsen, L. W. (2011). Strategies and attributes of highly productive scholars and contributors to the school psychology literature: Recommendations for increasing scholarly productivity. *Journal of School Psychology, 49*, 691–720.
- Messick, S. (1989). Validity. In R. L. Linn (Ed.), *Educational measurement* (pp. 13–103). Macmillan.
- Schneider, W. J., & McGrew, K. S. (2018). The Cattell-Horn-Carroll theory of cognitive abilities. In D. P. Flanagan & E. M. McDonough (Eds.), *Contemporary intellectual assessment: Theories, tests, and issues* (pp. 73–163). New York: Guilford Press.

- Van Norman, E. R. (2016). Curriculum-based measurement of oral reading: A preliminary investigation of confidence interval overlap to detect reliable growth. *School Psychology Quarterly, 31*, 405–418. 10.1037/spq0000146
- Van Norman, E. R., & Nelson, P. M. (2019a). An evaluation of the use of seasonal goal lines to improve the accuracy of curriculum-based measurement of reading decision rule recommendations. *Assessment for Effective Intervention*. Advance online publication. 10.1177/1534508419872249
- Van Norman, E. R., & Nelson, P. M. (2019b). The influence of trend estimation method on forecasting curriculum-based measurement of reading performance. *Journal of School Psychology, 74*, 44–57. 10.1016/j.jsp.2019.04.001
- Van Norman, E. R., Nelson, P. M., & Klingbeil, D. A. (2017). Single measure and gated screening approaches for identifying students at-risk for academic problems: Implications for sensitivity and specificity. *School Psychology Quarterly, 32*, 405–413. 10.1037/spq0000177
- Van Norman, E. R., Nelson, P. M., & Parker, D. C. (2018). A comparison of nonsense word fluency and curriculum-based measurement of reading to measure response to phonics instruction. *School Psychology Quarterly, 33*, 573–581. 10.1037/spq0000237.