The Measurement of Affect, Mood, and Emotion

The role of affective constructs in human behavior in general, and health behavior in particular, is recapturing the attention of researchers. Affect, mood, and emotion are again considered powerful motives behind dietary choices, physical activity participation, cigarette smoking, alcohol over-consumption, and drug abuse. However, researchers entering the fray must confront a vast and confusing theoretical and technical literature. The enormity of this challenge is reflected in numerous problems plaguing recent studies, from selecting measures without offering a rationale, to interchanging terms that are routinely misconstrued. *The Measurement of Affect, Mood, and Emotion* cuts through the jargon, clarifies controversies, and proposes a sound three-tiered system for selecting measures that can rectify past mistakes and accelerate future progress. Panteleimon Ekkekakis offers an accessible and comprehensive guidebook of great value to academic researchers and postgraduate students in the fields of psychology, behavioral and preventive medicine, behavioral nutrition, exercise science, and public health.

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The Measurement of Affect, Mood, and Emotion

A Guide for Health-Behavioral Research

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To the sources of all affect, mood, and emotion in my life, my wife and my son

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Foreword

Emotions, moods, passions, feelings, and other affectively charged states are now recognized as essential in the human condition. With the fading of behaviorism and then cognitivism a generation ago, emotions and the rest won their rightful status within the human sciences as a legitimate topic. It's fair to ask how the field of affective science is doing.

Panteleimon Ekkekakis provides a frank, sobering, and unfortunately accurate answer: the basic building blocks of science – clearly defined concepts and valid tools to measure them – are missing from much of the research. Ekkekakis focuses on health research, but his diagnosis applies as well to any branch of human science, including education, social welfare, and psychology in general. Fortunately, he also provides a much needed practical remedy.

Progress in affective science has been slow – as if our party won the election but then failed to form a new government. The explanation for the slow progress may be that science is not simply the accumulation of facts. Creation of a new science is not a transition from a blank slate to a scientific paradigm, but from one paradigm to another. The initial pre-scientific paradigm is the set of everyday, lay folk concepts and assumptions. We inherit concepts from angst to zeal with the preconceptions that accompany them. Astronomy, physics, and biology all show how difficult this transition is and how qualitatively different the initial folk concepts typically are from later scientific ones.

The affective domain is heterogeneous. The everyday concept of emotion is too heterogeneous, for it includes object-directed states (such as loving someone) and object-free or free-floating ones (such as malaise or anxiety) and includes long-term states (such as loving someone) and short-term ones (such as startle). To assess an emotional episode, one must therefore consider its object and its temporal dimension; simply checking a word on a list hardly suffices.

The affective domain lacks a superordinate term. For that reason, I used the cumbersome phrase "emotions, moods, passions, feelings,

and other affectively charged states." "Affect" is now sometimes used as the superordinate, but its boundaries are vague. I especially want to distinguish affect used as a superordinate from Core Affect. Core Affect is an ingredient in most (not all) emotions, moods, and so forth, but not a superordinate – much as flour is an ingredient in most (but not all) baked goods, but would not do as a substitute for the term "baked good."

The book you hold in your hands is a powerful plea for a qualitative shift in the way research is conducted. It is a wise, thoughtful, and much needed guidebook for the transition from a prescientific to a scientific paradigm. If researchers read this book, they will be convinced, they will change their behavior, and their research will advance. I'm often asked to recommend a measure for emotion or mood, and I never have a simple answer. Now I do: read Ekkekakis.

James A. Russell
Boston College

Prologue

> Simple can be harder than complex. You have to work hard to get your thinking clean to make it simple.
>
> – Steve Jobs, in an interview to *Business Week*, May 25, 1998

My goal was to write the guidebook I wish I had available to me when I was starting out as a junior researcher 25 years ago. While still an undergraduate student, I became interested in learning more about the affective changes that occur when people participate in physical activity. At that time, I had some elementary background in psychology but had not taken even a single course on emotional or affective phenomena for the simple reason that such courses did not exist back then, at least at the universities I attended. My knowledge about emotion was limited to the highly abridged summaries of the classic theories of William James and Walter Cannon contained in introductory textbooks.

Lacking background and guidance, I made every mistake imaginable. I used measures without knowing much about them and certainly without having a good grasp of their theoretical underpinnings or their relative strengths and limitations. I selected one measure over another on the basis of such profoundly naive criteria as their brevity or popularity. I measured one variable (e.g., emotion) but discussed my results as if I had measured another (e.g., mood). I measured only a small part of a domain of content (e.g., only a few discrete mood states) but my ignorance led me to generalize to the entire domain (e.g., the global domain of mood). I based my conclusions on measures that were deeply flawed although even a rudimentary psychometric analysis would have alerted me to this fact. I was in way over my head and, for many years, I did not even realize it because, frankly, I was just using the measures that "everyone" (or so it seemed) was using in the published literature, including researchers with decades of experience, great academic credentials, and worldwide name recognition. I was just emulating what I perceived as common arguments and common practices. So what could I possibly be doing wrong?

My perspective started to change when I entered graduate school. One upside to my youthful naïveté was that it allowed me to take a plunge into deep waters, a course of action I might have avoided if I had any realistic idea about the enormity of the gaps in my knowledge or the magnitude of the challenge I faced. And so my journey into the world of affect, mood, and emotion research began. I started studying about affective phenomena, taking courses from brilliant teachers, communicating with recognized leaders in the field, and, more than anything else, reading voraciously. Now, with the benefit of hindsight and 25 years of working exclusively on affective phenomena, I can recognize (and feel deeply embarrassed about) my own past mistakes. At the same time, based on what I see as a manuscript reviewer and editor on a daily basis, I have become increasingly concerned about what I perceive as signs of a storm gathering over this research area. My growing concern provided the impetus for this book. Let me explain.

First the good news

After decades of modeling humans as data processors engaged in a perpetual cycle of information gathering, cognitive analysis and appraisal, weighing pros and cons, and rational decision making, psychologists and other behavioral scientists are, once again, willing to consider the possibility that affective constructs, such as core affect, mood, and emotion, influence human behavior. This has been a long and arduous process. In the 1950s, as B. F. Skinner (1953) was characterizing "emotions" (his quotation marks) as "excellent examples of the fictional causes to which we commonly attribute behavior" (p. 160), Paul Young (1959) was trying to convince his peers that "any theory of behavior which ignores the concept of affectivity will be found inadequate as an explanation of the total facts" (p. 106). In the 1960s, during the transition from behaviorism to cognitivism, Silvan Tomkins (1962) attempted to resurrect psychological hedonism by conceptualizing affect as "the primary motivational system in human beings" (p. 108). However, these voices were simply too few and too isolated to generate momentum and shift the paradigm. As Kuhn (1962/1996) warned, ideas that go against the dominant paradigm of an era are usually ignored, marginalized, or suppressed. At least the cognitive theories of emotion by Magda Arnold, Stanley Schachter, and Jerome Singer and Richard Lazarus, as well as the exciting cross-cultural research by Paul Ekman on facial emotional expression, kept interest in the topic of emotion alive.

Then, thankfully, came a stream of "outsiders" so brilliant and creative that scientists and the general public alike had to take notice.

Nobel laureate in economics Herbert Simon, after raising questions about the true limits of human rationality, called attention to the role of emotion in judgment and decision making. Michel Cabanac in applied physiology proposed that pleasure serves as the "common currency" by which organisms, including humans, decide on appropriate courses of action in the presence of competing priorities. Antonio Damasio in neurology reignited the conversation on the deeply disruptive effects that damage to emotion-related areas of the brain have on behavioral decision making. Joseph LeDoux in basic neuroscience ruffled a few cognitivist feathers by arguing for a possible precognitive mode of eliciting emotional responses to frightening stimuli. By operating mainly outside psychology proper, and therefore the narrow confines of the cognitivist paradigm, these influential thinkers got scientists across a broad spectrum of disciplines talking about affective phenomena and their role in behavior once again.

Within the core of psychology, the debate between Robert Zajonc and Richard Lazarus that started in the early 1980s probably convinced more than a few skeptics that the area of emotion research represented an enormous pool of fascinating questions, surprising phenomena, and intriguing mysteries central to understanding human behavior. Although it would be hard to identify a single pivotal event, study, research line, or inspirational figure as the main instigator of this trend, during the 1990s and 2000s, interest in affect, mood, and emotion among psychologists rose perhaps to the highest level since the James–Cannon debates of the early twentieth century. A psychologist, Daniel Kahneman, won the Nobel Prize (in economics) in part for reminding behavioral researchers of the timeless insight encapsulated in the words of English philosopher Jeremy Bentham, the central figure of the school of utilitarianism: "nature has placed mankind under the governance of two sovereign masters, pain and pleasure; it is for them alone to point out what we ought to do, as well as to determine what we shall do" (from the opening lines of his 1789 *An Introduction to the Principles of Morals and Legislation*). Kahneman, Paul Slovic, and others initiated research on a mechanism, named the "affect heuristic," according to which pleasure guides judgments and decisions, assigns priorities to different goals, and ultimately serves as a powerful motive in human behavior. Writing in the *American Psychologist* after receiving the Nobel Prize, Kahneman (2003) described the affect heuristic as "probably the most important development in the study of judgment heuristics in the past few decades" (p. 710). Jaak Panksepp in animal research and Richard Davidson in human research led the evolution of the new field of affective neuroscience into one of the most vibrant and prolific

domains of modern psychological research. John Cacioppo and Gary Berntson put affective phenomena at the center of the budding interdisciplinary field of social neuroscience. James Gross and Kevin Ochsner pioneered the field of emotion regulation. And in a series of captivating debates that served as the foundation for most of the chapters of this book, James Russell, Lisa Feldman Barrett, and David Watson shed greatly needed light on several crucial conceptual issues long enveloped in a thick cloud of confusion and controversy.

Today, as psychology is slowly regaining a more balanced perspective after the cognitivist fascination that characterized the past few decades, interest in affective phenomena is rising rapidly. New dedicated journals, such as *Emotion* and *Emotion Review*, were created to accommodate the growing number of article submissions. Importantly, interest in affective phenomena is now also growing in a broad range of research fields investigating the driving forces behind human health behaviors. Why do humans overeat, smoke cigarettes, abuse drugs and alcohol, or avoid exercise? Until recently, the standard answers pointed to cognitive processes: because they lack education, because they have low outcome expectations, because perceived benefits do not outweigh perceived risks, because of low self-efficacy. As acknowledged by Ajzen and Fishbein (2005), "much of the research [stemming from cognitive theories of motivation and behavior] has devoted little attention to the role of emotion in the prediction of intentions and actions" (p. 203). Today, however, as the limitations of cognitivist explanations are becoming apparent to an increasing number of investigators, the role of affective and hedonic processes is attracting attention. Extensive literatures have now emerged on the links between affect, mood, and emotion with eating behaviors, obesity, drug abuse, smoking cigarettes, drinking alcohol, and exercising. Most studies in these areas were conducted within the last decade and the rate of publication continues to increase. In sum, it is clear we are now in the midst of a paradigmatic transition; affective processes are regaining their rightful place among the major motives driving human behavioral decisions. A new, more refined, more sophisticated version of psychological hedonism is emerging.

Now the not-so-good news

The nature of academe is changing dramatically. It is fair to say that, at most major academic institutions, professors are allocating their time in a substantially different manner compared to their mentors just a generation ago. They devote more work hours to applying for funding, networking, preparing budgets, hiring and firing personnel

(e.g., postdoctoral researchers, information technology professionals, study coordinators), and managing the logistics of multimillion-dollar grants. Success in these activities has become the main criterion by which applicants for faculty positions are selected, promotion and tenure decisions are made, awards are given, salaries are adjusted, startup and retention packages are offered, and the overall quality of scholarship is evaluated.

Since time is a precious and finite commodity, and institutional pressures to generate funds are constantly rising, something "has to give." Usually, that something is the time to study, reflect, and broaden and deepen one's conceptual knowledge and understanding. Who has time to educate oneself about a new area outside of one's own narrowly demarcated line of research? Who has time to delve into a century-old literature, retrace the evolution of ideas through time, carefully scrutinize methods, critically analyze assumptions, or contemplate arcane theoretical arguments? In my work as editor and reviewer for journals and granting agencies, I see that, increasingly, academics resort to choosing ideas, measures, and methods on the basis of much more superficial criteria. What is the latest trend? What is mentioned more frequently in the current literature or talked about at conferences? What are the established authority figures in the field doing these days?

Psychological measures, in particular, seem to be picked "off the shelf" and treated as "plug-and-play" devices. For many time-pressed investigators, the process of selecting a measure for a particular variable (e.g., "affect" or "mood") has been reduced to running a computer search for that keyword and selecting the measure that seems to pop up more frequently than others. Evidently there is no longer the expectation that the investigator will have a thorough, deep understanding of the developmental history, the conceptual bases, the intricate psychometric details, or the strengths and limitations of the measure.

One of my biggest pet peeves is the phrase "to measure variable X, we used measure Y because it has been used extensively in previous studies." I am not sure when or why this type of "justification" came to be viewed as adequate in the research literature. I do know, however, that researchers should not allow it to become the norm. Measurement is too important, too central to the research enterprise to be treated with such laxity and superficiality. Measurement is perhaps one of the most delicate and challenging elements of the research process because it represents the intersection of theory, method, analysis, and interpretation. As such, it requires more conceptual sophistication and technical expertise than most other components of research methodology. Unfortunately, as I read manuscripts and

research proposals, I am all too often reminded of this excerpt by Kerlinger (1979) I once read:

> Measurement can be the Achilles' heel of behavioral research. Too often investigations are carefully planned and executed with too little attention paid to the measurement of the variables of the research … All fields of human effort have their shares of mythology and nonsense. Measurement is unfortunately particularly burdened with both. Negative attitudes toward psychological measurement are part of the cause. But ignorance and misunderstanding are probably a greater part. (pp. 141–142)

Mythology, nonsense, negative attitudes toward psychological measurement, ignorance, and misunderstanding; Kerlinger said it all. And his words, unfortunately, ring as true today as they did more than three decades ago.

The goal of this book

This guidebook is about the measurement of a specific category of variables within a specific research domain; it is about the study of affective phenomena as they relate to health behaviors. Why the focus on health-behavioral research? One part of the answer is that, as I mentioned earlier, interest in affective phenomena within health-behavioral research is rising at an astonishing pace. The other part of the answer is that, at least within the English-speaking world (United States, Canada, United Kingdom, Australia) and much of Europe, research on health behaviors is among the most heavily funded areas of behavioral and biomedical research. Health behaviors pertain mainly to prevention and, as such, represent a much more cost-efficient option than treatment. In addition, a person engaging in health behaviors typically reports more satisfaction with life and better perceived quality of life than someone receiving treatment for a chronic disease. Given these economic and humanitarian reasons, it is logical that funding agencies, governmental and private alike, are interested in exploring the potential of health behaviors for reducing the personal and societal burden of chronic disease.

Therefore, in the domain of health behaviors one sees the ominous intersection of the two trends I described in this prologue. On one hand, investigators with very diverse educational backgrounds and research foci are becoming interested in affective phenomena. On the other hand, most of these investigators operate under extreme pressure within an increasingly competitive environment for a limited pot of research funds. Hence the problem. Oftentimes, no matter how noble the intentions and how deep the desire of the investigator to do top-

notch innovative research, when the quantitative performance expectations are as high as they are these days, corners must be cut. Researchers simply do not have the time to scrutinize their measurement options, so they rely on indirect sources of evidence to establish their quality.

- I want to measure "affect" and this questionnaire says that it is a measure of "affect."
- This was published in a decent journal, so it must have undergone rigorous peer review; if the experts who looked at it thought it was fine, it must be fine.
- Everybody seems to be using it, so it must be good.
- This questionnaire was developed by famous professor X, who claims it is valid and reliable.

At least in my eyes, it seems the problems Kerlinger identified in the 1970s are even more exacerbated, more dramatic, in research dealing with affective phenomena than most research areas. As Kerlinger said, "ignorance and misunderstanding" are perhaps the main contributors to problematic measurement practices. The theoretical and empirical literatures on affect, mood, and emotion are notorious for being extraordinarily convoluted and confusing, so "ignorance and misunderstanding" tend to be quite prevalent phenomena in this area. Of course, I realize that most researchers would passionately argue that their area of research is the most complex, the most challenging, the one with the most delicate conceptual distinctions. Nevertheless, for reasons that will become apparent in this book, I maintain that the domain of affect, mood, and emotion research, when examined with a sense of perspective, is *even* more perplexing than most other areas of psychological investigation.

In this guidebook, my highly ambitious goal is to help bring some order to the chaos. It is important to warn readers up front that, while selecting a measure of affect, mood, or emotion, they will never be able to use a "cookbook" approach (i.e., a fully standardized, truly algorithmic step-by-step protocol) as has now become the norm, for example, in fields like endocrinology, immunology, or molecular biology. Nevertheless, I am proposing a workable three-step method, which, if properly implemented, should help reduce arbitrary decisions and, in the process, lower the risk that the research enterprise might be led astray by unfortunate measurement choices.

The guidebook is intentionally short because it is meant to be read cover to cover, even when taking into account the enormous pressures of contemporary academic life. I have also tried to strike a balance between avoiding the use of baffling jargon (a key contributor to the

very problem I am trying to address) and not oversimplifying inherently complex and "fuzzy" concepts. Finding the right balance is, of course, a difficult task and, although I hope to have succeeded in some cases, I am afraid that I have probably failed in others.

I am sure that experts in affective psychology will find the descriptions of some issues underdeveloped or unrefined, perhaps too rudimentary or overly abridged. I am also certain that some experts will object that I am presenting some issues that they might consider open or still unsettled in a more definitive tone than is warranted by the empirical evidence. And, naturally, in cases in which I take sides in ongoing debates (as I often do), there will be those who believe I am wrong. To all these colleagues, I offer my sincere apologies, as I openly acknowledge my limitations. My hope in providing this guidebook to the scientific community is to initiate a move in the right direction – or at least a conversation. If I am lucky, things will snowball from there!