2010 Senior Researcher Award Acceptance Address

Robert A. Duke

What if Research Was Interesting?

When I first learned that I’d won this award, I was deeply touched and honored and humbled. There’s something very special about being recognized by your peers, the ones who actually know what you do and what you’ve done. The colleagues who nominated me for this award are people for whom I have great respect and admiration, and the previous recipients of this award are among the best thinkers in our discipline. I’m truly grateful to be here and to have this chance to tell you about my ideas.

Any time your work is acknowledged in some remarkable way like this, it inevitably prompts a little personal reflection, thinking about how you came to be here and how all of this came about. My goal today is to influence your thinking about the research enterprise in our discipline and what we can do to make it better, more meaningful, and more effective in contributing to human understanding.

But first, just a bit about how I came to be here.

I’m here quite by accident. Actually I’m here as a result of a series of accidents. I had no aspirations to become a college professor and certainly I never aspired to be what anyone might call a scholar, not that I even knew as a child what scholars did. I was an odd little kid with lots of interests, any one of which could have turned into a life’s work. I had the great good fortune to be raised by two parents who skillfully combined undying affection with unbridled curiosity and joy. My father was an intensely inquisitive and self-propelled man, whose formal schooling ended with the 8th grade. He was 50 years old when I was born, and I’m sure he found that raising a kid at his age in the 1950s and 1960s was nothing like what he had imagined. I know that in many ways I was inscrutable to him. But he consistently conveyed to me a sense of wonder about the world. He invented things and built things and fixed things and in the process he broke a lot of things, much to my mother’s silent, and sometimes not so silent, frustration. But in the midst of all his tinkering and breaking and fixing I observed a sense of tenacity and determination that quite often led to satisfying

1The University of Texas at Austin, USA

Corresponding Author:
Robert A. Duke, Butler School of Music, The University of Texas at Austin, 1 University Station, E3100, Austin, TX 78712-0435
Email: bobduke@mail.utexas.edu
conclusions. Satisfying to him at least, even though my mother never seemed to warm to the idea of black electrical tape as a universal healer of broken appliances and nearly everything else.

When I was very small and my father would tuck me in at night, he’d often sit on the edge of my bed and ask me questions about mathematics or something that required me to work out a solution to a problem in my head. I don’t really know what his motivation was for doing this. Perhaps this was a 1950s parents’ version of Baby Mozart CDs.

All of his tinkering and thinking and puzzling and swearing was leavened by my mother’s unfailing good humor, kindness, and patience with my father and me. My mother is a very bright woman, and though she probably knew the answer to what my father and I were puzzling over long before either of us figured it out, she showed in a quiet way how patience and acceptance create an environment that welcomes play, celebrates accomplishment, and revels in the simple joy of doing something that interests you and that you care about.

My parents were my first and best teachers, and my mother continues to be to this day. But throughout my formal education I seem to have been extremely lucky to have bumped into an unusually large number of wonderful teachers in school. I’ve obviously forgotten many of them by now, but the ones I do remember—at least in my undoubtedly modified recollections of the past—were, to a person, characterized by a joyfulness in their work with us children. They seemed to like knowing things and being able to do things and they seemed to take great pleasure in helping my classmates and me learn about the things they cared about. They smiled a lot.

I took away from all of this a sense of wonder about how things worked, a sense of curiosity that energizes me even now. It doesn’t have to do with a particular subject matter, really. I think I could have loved a lot of things, but I ended up loving this.

One of the big reasons I ended up here is that I met Cliff Madsen, who inspired me and became my mentor and friend and who remains so after all these decades. When I went to graduate school at FSU, I had no idea what I was doing, as Clifford and my classmates at the time can well attest. And then I took a class in research, and I recognized something familiar in Clifford: the curiosity and wonder and puzzling and joy that I’d grown up with, but now directed toward a problem that was powerfully interesting: making sense of the behavior of human beings. I was enrolled in a music school, of course, but the classes I was taking in the basement of what is now the Kursteiner Building weren’t really about music or music teaching or music therapy. The classes were really about people and the machinery in our heads that governs how we think and feel and behave.

On my first visit to Clifford’s office I noticed a copy of the journal Science lying on his desk and thought, What’s that doing here? And the more time I spent in that office over the next four years the more I realized that I was in a place that had few of the intellectual barriers that most people think of as disciplinary boundaries. The place was interdisciplinary before interdisciplinarity was cool.

I observed in those precious years in Tallahassee watching and working with Clifford what it means and what it takes to be a clear-headed thinker and a productive scholar,
realizing that the same kinds of practice that led me to be a good musician and teacher would someday lead to my becoming a good psychologist and writer. By practice I mean rehearsal, repetition, doing things again and again with greater and greater levels of discrimination and refinement.

Everything we become good at requires repetition, of course, including doing good research and writing about it. Experiments most often don’t work. Equipment breaks and subjects don’t show up or they take a nap between test sessions when you’d asked them not to or you find out after many hours of thankless labor that you’ve been measuring the wrong variable. It’s frustrating and confusing most of the time. But every once in a while it’s quite lovely because you figure out some little thing that no one has figured out before. In those fleeting moments, you know something that no one else knows, and after you’ve gotten the same result enough times to convince yourself that you’re probably not mistaken, you’ll get to tell other people about what you learned.

* So, what makes research interesting?

Research that’s interesting is research that explains something, even a little piece of something. The point of research, after all, is to make the world more understandable. Questions that ask What? or Whether or not? seldom make the world more understandable, but questions that ask Why? and How? often do.

Many people, after learning about some recent research result, ask So what? But So what? is a flip, and wrong, question to ask. The more penetrating question is What does this explain? I promise you, if research explains something, almost anything, it will be interesting to many people. It’s not necessary that the explanation even be useful. There’s a lot that we find interesting and compelling that has no immediate utilitarian function whatsoever. The extent to which research is interesting in fact has little to do with the immediate applicability of the findings.

Several months ago, John Tierney wrote a piece in The New York Times called “Will you be emailing this column? It’s awesome,” reporting on a study by Jonah Berger and Katherine Milkman from the Wharton School at Penn who had examined the most emailed articles from the NYT online edition over a period of 6 months, from August 2008 through February 2009—over 7700 articles (Berger & Milkman, 2009). Berger and Milkman’s most interesting finding was that the articles most likely to be emailed were those that had the potential to inspire awe. The most emailed stories in that time didn’t concern how to choose a mutual fund or whether to reduce your intake of dietary sodium; they were about cosmology and paleontology and the visual system of the white-tailed deer. In the authors’ words, “Emotion leads to transmission, and awe is quite a strong emotion. If I just read this story that changes the way I understand the world and myself, I want to talk to others about what it means. I want to proselytize and share the feeling of awe. If you read the article and feel the same emotion it will bring us together” (Tierney, 2010).

Often the sense of awe emanates from understanding something new, when we see some remarkable phenomenon explained in a way that we hadn’t imagined. I’d like to propose that What does this explain? is the question every one of us should ask about
the research we read, the research we do, and the research we involve our students in. If the answer is Nothing much, then it’s doubtful that we’re spending time on something interesting. If the research in our field is going to get better, if it’s going to contribute something meaningful to human understanding, then it needs to help explain something.

For the past four years I’ve been working on a National Science Foundation grant that places doctoral students from the natural sciences in public schools as “scientists in residence.” It’s a wonderful program for more reasons than I have time to enumerate here. But one of the outcomes that surprised many of my colleagues after we had embarked on this program was the extent to which the experience transformed the doctoral students’ thinking about their own disciplines and about what it means to do science.

I’ll relate one story that illustrates the point. One of my favorite doctoral fellows in the program, a bright, conscientious, talented woman from geological sciences, invited me to watch a videotape of her working in a 7th grade science class, where she had been asked by the classroom teacher to give a lesson on friction. She followed all of the guidelines that science teachers are given about planning good lessons in school. There’s something called a 5E lesson plan that’s popular in the sciences at UT and many other places as well. The 5 Es stand for Engagement, Exploration, Explanation, Elaboration, and Evaluation (I always have to look those up). This doctoral student dutifully planned a lesson that included each of those elements in order. To engage the students, the lesson started with some questions about moving heavy objects across various surfaces and going down a playground slide with shorts or long pants, questions that were followed by a written definition of friction given by the teacher. The definition was mostly accurate, but not entirely so.

After a brief discussion of the definition it was time for exploration, which in this case meant conducting an experiment of sorts. There was a preconstructed “experiment sheet” that went along with a set of “friction kits” distributed by the school district; each kit included a 50-cm board with foam glued to one side and a heavy metal washer to slide down the board. The students’ task was to build an inclined plane with the board and slide the washer down each side to determine on which side the washer moved faster, the wood or the foam. The prefab experiment sheet had a prefab hypothesis to be completed by the students that went something like this: “Hypothesis: I think the washer will travel [faster or slower] on the [wood or foam] because the [wood or foam] will create [more or less] friction than the [wood or foam].” Below the hypothesis were spaces for data from 10 replications of sliding the washer down each side of the board. Each pair of children in the class was given a friction kit, an experiment sheet, and a digital stopwatch. The video camera panned around the room as the children, none of whose facial expressions conveyed a great deal of enthusiasm (not one of the 5 Es), performed their experiments, timing the washers sliding down the boards. I sensed from none of the children that they were thinking to themselves, “So this is why people love science!”
The problem here is not a lack of caring or diligence or sincerity on the part of the teachers involved. And it’s not a matter of the children’s inherent lack of interest in science. The problem is that the experiment isn’t interesting. The kids knew the answer before they started and once they “discovered” it, they had nothing more to say about it. Which side is slower? Foam! Why? More friction! The end.

I proposed to the classroom teacher that the children do a different experiment with the same stuff, asking the question What effect does the angle of the board have on the relative speeds of the washer on the two sides? If the board is laid flat, the washer won’t move at all, of course, no matter which side of the board is facing up. If the board is standing up on its end, the washer will move equally quickly from the top to the bottom. But at some angle the washer begins to move at different speeds on each side. What angle is that? And why’s that? And how does the ratio of the foam-side speed to the wood-side speed change with changes in the angle of the board? I didn’t know the answers to those questions. The science teacher didn’t know the answers to them either. But we could figure them out with some experiments. Unfortunately, the classroom teacher was convinced that answering those questions would be “over the kids’ heads.” Sadly, it’s all too common in schools for teachers to believe that the interesting stuff is over kids’ heads.

After my discussing the lesson in my graduate class with the doctoral fellow and her peers, and after we all agreed that the kids had had a pretty miserable experience with the friction kits, I worried that I may have been too straightforwardly critical of what she had done. But I was heartened the next day when she sent me an email, asking to stop by my office to talk about a lesson she was planning on plant reproduction. She told me that as she was writing her plan, “I heard your voice asking, ‘Why are you doing this? This is so horribly boring!’” The lesson that she’d begun planning was to start with a worksheet filled with definitions of plant parts. This is a pistil, this is a stamen . . . (and so on). Being open-minded and thoughtful, she’d realized that there was little of interest in those definitions.

How and why things change are the heart of science. How and why things change are the heart of everything interesting. You can think further with knowing How and Why, beyond the immediate result of the data you have in front of you. The kids in the science class had been experiencing the world around them for 12 years. They’d seen lots of plants in that time, probably without asking many questions about them. I suggested that the next lesson begin instead with some interesting questions: Why would a plant expend all the energy it takes to make a flower? And why would a particular flower have the color and shape it has? And if so many plants have both male and female parts, why do they need other plants to reproduce? And if there’s a male oak tree on one side of a meadow, how’s he going to get his pollen to the female tree on the other side? Each of these questions requires an explanation, and the explanations have the potential to make what the kids observe every day more understandable and thereby more interesting. Coming up with explanations for those phenomena is worth the effort, and once you figure out the explanations, you’re eager to share what you know with other people. I couldn’t imagine a kid trying to impress a friend by asking
whether he knows the definition of a pistil and then telling him what it is. I can easily imagine the same kid asking his friend whether he knows how philodendrons have sex and then proceeding to explain it to him.

Understanding complicated ideas is tremendously gratifying. And when you’re able to figure out complicated problems, and you get to explain what you’ve figured out to someone else, it makes research part of a communal enterprise in which individuals and groups feel a collaborative mission to decipher how the world works. My friends in the natural sciences talk about writing research papers that “tell a good story.” What are they talking about? They work on increasing the affinity of molecules to bind with other molecules or with figuring out why clusters of bacteria secrete toxins that kill bacteria from nearby colonies or figuring out what motivates nerve cells to regenerate. Where’s the story in research like that? The stories they’re referring to are explanations of how phenomena work and perhaps also why they came to be the way they are. Historians and philosophers are after the same things, I think, even though their ways of gathering information are quite different from conducting controlled experiments.

In 1994 MENC released a research agenda for music education. I have no idea if anyone looks at it any more, but I read it a couple of weeks ago. In a sidebar early on in the text is a description of four kinds of systematic inquiry that are briefly explained this way:

*Philosophical*—Why
*Historical*—What has been
*Descriptive*—What is
*Experimental*—Establishing cause and effect

Discussions of research in our discipline often begin like this, with focus on the methods of research rather than on the problems the researcher is seeking to solve. This creates mistaken views of what the task is about. When the goal of research is to explain something, then the methods used to develop an explanation are selected to accomplish the goal of understanding. The questions suggest the methodology, but it’s the questions that matter most. All introductory courses in research should begin not with depersonalized descriptions of what we already know, but instead with a historical review of how inquisitive human beings came to identify interesting problems and how they advantageously employed the methods of research in solving them.

I’d like to amend the descriptions from the research agenda. First, as I’m sure you’ve figured out by now, I don’t think that philosophy is the only branch of inquiry that asks Why. In fact, I’m not even convinced that asking Why is what most philosophers think they’re doing a good deal of the time.

In a very important sense, all good research seeks to tell a story of how things work and how they came to be the way they are. Even so-called descriptive research should seek to do that. A couple of recent articles in *JRME* by Daryl Kinney are splendid...
examples of asking interesting questions that begin to tell a story (Kinney, 2008, 2010). All of us in our profession have endured a relentless bombardment of correlation data showing impressive relationships between music study in school and students’ performance in academic disciplines. What Daryl has shown with the kids in an urban school district in Columbus, Ohio, is that you can predict the superior academic performance of kids who enroll in a band program before the kids enroll in band; in other words, the more academically able tend to be the kids who enroll in the first place and they are also the ones more likely to continue. This may explain why the correlations between enrollment in school music programs and academic performance look the way they do. Daryl’s results begin to tell a story that may partly explain the correlations, and they show that there is much more work to be done before we can confidently draw any conclusions about how music participation in school affects students’ performance in other classes.

No matter the means, research that helps explain the world is research that captures the interest of readers and listeners, not because a teacher can use it on Monday or because it will help individuals make decisions about their own medical care, though those may be included in the outcomes from time to time. Research that explains the world is interesting because it allows people to share in a bit of new understanding that’s simply a joy to be a part of.

*So, now being officially declared an old person by dint of this award, I would like to offer my advice to the young who are thinking about or who are just now embarking on a career in research—advice about how you can contribute something interesting and meaningful to our field.

**Read, read, read.** Read the best work you can get your hands on. And today it is possible to get your hands on just about anything online without even getting dressed. (Back in my day, we had to walk to a library. Imagine.) There’s simply no excuse for reading mediocre work. You don’t have to. There’s superb work at your fingertips. Read great science, read great history, read great literature, read great news reportage. Immerse yourself in intellectual excellence every day. This isn’t to say that you can’t or shouldn’t partake of the delightful pablum you recognize as unrigorous. I enjoy a good Keith Olbermann screed and a David Sedaris essay and a Mariah Carey song from time to time. But do that stuff all the time and it’ll make you stupid.

**Write, write, write.** Writing is hard. And if my experience is an indication, it will always be hard. The trick, if you can call it a trick, is to come to enjoy the difficulty of it. One of my favorite pithy quotes about writing I learned from my friend and former colleague at Texas, John Trimble, who wrote what I think is the best book about how to write that’s ever been written (Trimble, 2000). The quote is from the great sports writer Walter W. “Red” Smith, who said, “Writing is easy. All you do is sit in front of a typewriter keyboard until little drops of blood appear on your forehead.” That’s pretty much how it is. I’m not a particularly gifted writer. But I’m a tremendously gifted editor. After many painstakingly composed drafts that I pore over on my own, and after my friends and colleagues and graduate students hack at them with Track
Changing switched on, and after journal and book editors go hacking at them some more, and I write more painstaking drafts and the cycle continues, I’m sometimes left with a pretty good paper. It almost never happens without all of that.

Writing is frozen thought. It’s easy to delude yourself that you really understand something when you’re just thinking and even talking about ideas. Self-delusion is much more difficult when you try to explain yourself in print. When we write we have time to examine our ideas in ways that seem otherwise impossible. We have the ability to read what we’ve written and consider carefully whether what’s on the page or on the screen is actually what we think we mean. The important thing about this process is that it inevitably refines the precision of our thinking.

There is a fascinating phenomenon related to human creativity that was first reported by Simonton in 1977. Now known as the equal-odds rule, it states that in scholarly and artistic endeavors “quality correlates positively with quantity, so that creativity becomes a linear statistical function of productivity” (Simonton, 1996, p. 235; see also Simonton, 1999). Simonton observed this phenomenon after exhaustive analyses of artistic productivity and published research in a range of academic disciplines. In other words, across the span of an individual’s career and among different individuals in a given discipline, the number of important works tends to be proportional to the total number of works produced. It would be understandable for us to imagine that the writers and scholars we’ve heard of, precisely because they’ve produced important works, are very smart and creative and thus produce only important works. In fact, Simonton has found, they also produce more works, and the ratio of the number of important works to total works is relatively constant across their careers. It’s not that productive scholars who are making meaningful contributions are sitting around thinking hard and then cranking out important paper after important paper. That’s almost never the case. Instead, productive scholars are producing lots of work, much of it forgettable, but some of it quite lovely.

The function of producing all this work seems clear: Each time you conduct an experiment or write an essay or conduct a survey or document historical precedents and you show it to people, you get feedback, some of it wonderful and glowing, some of it (if you’re lucky) incisively critical. And that ongoing stream of feedback shapes your thinking and your writing and your planning. It helps you become a better scholar.

Don’t mistake form for substance. I like a well-applied t test as well as the next guy, but that’s not what makes for good research. As we say in my part of the country, doing a Bonferroni correction on data from a dumb experiment is lipstick on a pig. It doesn’t make the study any less dumb, it just dresses it up a bit to look like research. I know that many people taking their first experimental research classes come to believe that the really important stuff about research is the statistical tests. My conjecture is that they think this way because, to them, statistics are algorithmically tangible and because they don’t understand them at all. Statistics are a way of calculating the likelihood of events. That’s it. They’re not research. They’re not data. They’re summarizations of data. What makes for good research is the substance of the questions posed and the
skillfulness in collecting data that are applied in answering the questions. Framing good questions is much harder than calculating $F$ ratios, and if the questions aren’t meaningful, if they don’t lead to explanations of something, then there’s no point in doing the $F$ tests at all.

**Talk to people all the time about your ideas and your work.** You should do this with people who know a lot about what you’re working on and with other people who don’t. The knowledgeable people will ask you good questions, if they care about you, questions that will focus your thinking and challenge you to think better, and they’ll tell you about ideas and techniques and resources that you don’t know about. The less knowledgeable people will ask you different kinds of questions, ones that often will prompt you to think about what you’re doing in ways you’ve not considered before. I find that undergraduates are particularly good at this. My Texas colleague Steve Weinberg, a Nobel laureate in physics, said once that the best idea he ever had came to him while he was talking to a group of undergraduate students.

**Find one or more research buddies.** The act of collaboration among people of like interest is one of the most compelling aspects of the research enterprise. I am energized by the people I interact with day to day, not only the people in music and music education, but other colleagues in biology, psychology, and neuroscience. All of them contribute to my thinking, enhance my perspective, and challenge my understanding. This is especially true about the people who are closest to me, my research peers whom I work with day in and day out. I have the great good fortune to be surrounded by blindingly smart, boundlessly creative, unfailingly kind, deeply caring people who encourage my good ideas when they arise from time to time and who tell me I’m full of shit with equal doses of vigor and affection. It’s a wonderful place to be. Research alone in your office is much harder, not least because you have no one checking your capacity for self-delusion. I know how I want things to come out. I’m a human being with wants and expectations and an ego, all of which conspire to sabotage my science. The people around me help keep those obstacles in check.

**Grow thicker skin.** No one learns anything without feeling at least a little bit uncomfortable in the process. It’s quite peaceful and calm when everything is as you imagine and expect it to be. It’s much less so when you’re faced with powerful evidence that it’s not. One option is to avoid experiences that contravene our cherished ways of thinking. Communing only with like minded people who think exactly as we do certainly has emotional appeal, but, intellectually, it’s a bad option. Another option is to embrace such intellectual dissonances, pursue them, examine them, and let them refine the way you think. We sometimes mistake honest criticism for personal hostility. The criticism is a necessary part of refining ideas. To avoid the criticism for fear of the perceived hostility is a big mistake.

**Hang out with kind, smart, interesting, interested people.** I’m very explicit about that list of adjectives, and the people you hang out with should each be aptly described by all four. I put kindness first, because it’s a *sine qua non*. You certainly can learn something from unkind, smart, interesting, and interested people, but I find that they tend to make you miserable in the process, and I’m unconvinced that the misery is worth
the trade off. The interested part is a big one as well. There are many smart and interesting people who are not at all interested in others, least of all, you. But there are a few individuals in all of our environments who meet all of those criteria. They are truly interested in our well being and they have the courage to tell us what we’d rather not hear. They care that much. Of course, if they’re smart and interesting, then what they tell us is more likely to be actually useful in our developing a better understanding of the world and of ourselves.

Go to professional meetings where you’re likely to learn something about what you’re working on. I wrote in the preface to Intelligent Music Teaching (Duke, 2009) some years ago that I was not so distressed by the fact that presenters at a music conference would choose to lead a group of musically literate adults through a rendition of “Duck Duck Goose” that was designed for third graders as I was distressed by the extent to which the session attendees seemed to like it. There are conferences in and out of music where you can go and learn from people who are working on interesting problems. When it’s cooked right, a conference like that is like a smorgasbord of delicious lectures by very interesting people. It’s like being in your favorite classes in college and it’s an intellectual delight.

Write research that will be read with interest by people outside of music education. E. O. Wilson, the great entomologist and the acknowledged founder of the field now known as sociobiology, published a book in 1998 called Consilience: The Unity of Knowledge, in which he proposed that all branches of knowledge—the sciences, humanities, and the arts—are really all working toward similar ends, based on “a conviction, far deeper than a working proposition, that the world is orderly and can be explained by a small number of natural laws” (Wilson, 1998, p. 4). Wilson may have overreached a bit, and he’s been taken to task by many writers for doing so. Nevertheless, it’s a powerful assertion, one which suggests that the advancement of human knowledge is leading inexorably toward a confluence of ideas among what are now seemingly disparate disciplines. Whether or not Wilson’s arguments are wholly supportable, the increasing connectedness among what were once widely separated disciplines is an objective fact. The more we remain confined in our own bailiwick in music education, the more limited our thinking and our understanding will remain. There are tremendous rewards to be had from reading and writing outside what you may think of as your discipline. You open yourself to a whole new range of ideas and a whole new level of scrutiny, one that will challenge you to think better and learn more.

I’d like to conclude today with expressions of gratitude to a number of people I’ve not yet mentioned. I’d like to thank MENC, and in particular Chris Johnson and the members of the Music Education Research Council, who selected me for this great honor. I’d also like to thank my friends and colleagues who were generous enough to nominate me for this award. I am grateful for the tremendous support for my work that I have received from The University of Texas at Austin, where I’ve spent the last 25 years, and from Director Glenn Chandler and what is now proudly known as the Sarah and Ernest Butler School of Music.
I’d also like to thank the classmates, friends, collaborators, and students who have shaped my thinking and brought me joy over the years. In particular, I’d like to express my deep appreciation to Jim Byo, my dear, longtime friend and most recently my coconspirator in overthrowing the hegemony of published method books, and to Amy Simmons, Carla Cash, and Sarah Allen, my closest collaborators who sharpen my thinking and enrich my life more than they know.

And most especially I’d like to thank the 2004 Senior Researcher Award Recipient, Judith Jellison, who day in and day out puts up with my contemporary versions of my dad’s black electrical tape, and who is undoubtedly the most conscientious, hard-working, and meticulous thinker and scholar I know. She inspires me every day and she possesses the most sophisticated bullshit detector that a person could ever hope to live with.

There was a big bash in Avery Fisher Hall in New York a little over a week ago celebrating Stephen Sondheim’s upcoming 80th birthday. The event included performances by all of the greats of the stage whom you’d expect to see and hear singing all of Sondheim’s best music. From the report I read in the NYT, Sondheim was very touched by the whole affair, and when he said a few words at the end of the evening, he quoted Alice Roosevelt Longworth, Teddy Roosevelt’s oldest daughter and quite a character, who said, “First you’re young, then you’re middle-aged, then you’re wonderful.”

And to all of you who’ve agreed to give me your precious attention this morning, thanks as well. This has been wonderful.

Note
1. Thanks also to my longtime friend and former classmate, Wendy Sims, able editor of the JRME, who allowed this talk to go to print just as I’d submitted it.

References