

John E. Hunter

This page focuses on the accomplishments of Dr. John E. Hunter, featuring some of his writings, biographical information and photos.

*This page is maintained by
Dr. Mark Hamilton
(MHAMIL@UCONNVM.UCONN.EDU)*



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Biography

(The following is from the Research Methods Division Newsletter of The Academy of Management Volume 15, Issue 2, Fall 2000).

John E. Hunter

Michigan State University

Life & Work

I was born during the great depression and I spent the first six months of my life in a tent. My father was killed in World War II and I grew up on the widow and orphan benefits from the Army and Social Security. If it were not for welfare, I would not be alive today.

Abject poverty ruins many lives, but it can greatly strengthen those who survive it. Above all, poverty inoculated me against arrogance, the worst disease that affects scholars. Arrogant people cannot change their minds and thus accumulate errors over a lifetime. However high their initial intelligence, they eventually become very stupid and out of step. I learned early that I am a dust mote surrounded by a universe that gives me an importance rating of 0. You can only control the universe if you play by its rules. Knowledge is the only real power.

I view every act of kindness as a gift and I believe that kindness and knowledge are the only tools we have to combat a hostile environment. The irony of the present is that we have come so far in adapting the environment to us that our main enemy is now us. Most of the evil in this world stems from people justifying hateful actions with moral and religious arguments claiming that God demands persecution of the deviant.

School combined kindness and knowledge-giving, and I loved school. It was the one social institution that I could trust. By contrast, I was convinced that the police were a giant goon squad created by the middle class to persecute the lower class. With age, my socialistic tendencies have died as I became convinced that socialism doesn't work; but I still see massive unfairness even in our advanced age. The fact that I reached college is an unlikely accident that stems directly from the kind actions of many people who extended a helping hand to a very poor boy with no visible way to ever pay the world back for those kindnesses. I got through college on my father's G. I. Bill benefits, again a case where my taxes have repaid welfare costs thousands of times over. I believe that human capital has extremely high economic payoffs.

As Maslow would predict, I entered college still dominated by security needs. Above all, I wanted a real home like other kids had. My wife Ronda gave me a real home and much, much more. She is the force behind my shift from security values to self-actualization. Teen age Jack could never have believed that I would become a scholar and a workaholic; he never predicted Ronda.

My poverty completely colored my approach to learning. I learned early that only objective reasoning could bring any relief to my suffering and I became extremely logical in all my social relations as well as my problem solving. Mathematics was as natural to me as breathing. I loved plane geometry and enjoyed teaching my classmates outside of class. When I hit psychology it was a perfect fit: (a) the logic and objectivity of science, and (b) a

focus on people, which have always been my most enduring interest. I discovered that I also love sociology, anthropology, economics, and all the other social sciences. I really see little difference between them other than the ignorance patterns produced by specialization and the information overload of modern life.

As an undergraduate, I majored in both math and psychology and I loved both equally. In graduate school, I found that I could not do two doctorates and I proceeded with psychology because I thought it much more important. I was thrilled that MSU hired me jointly as a faculty member in both math and psychology though eventually the administrative hassle produced by this resulted in my leaving the math department to teach only in psychology.

However I did develop a calculus course for social science graduate students that attracted top students from all over the social and business sciences. Either my course or their talent was exceptional because an astounding number of these students went on to top research careers. My emphasis was on the development of causal processes over time. When I got my doctorate, I was committed to a life as a theorist where I could best combine my mathematical and psychological skills. These desires were doomed to failure by the nature of the social sciences in 1964 and these problems still plague me. Theory begins with propositions that a mathematician would call "axioms". You assume these propositions and use logical reasoning to derive the implications that form the body of the theory. That body of derived facts is the new knowledge produced by the theory. I am a generalist by nature and I tried to build theories in every area. Everywhere I failed for the same reason. I couldn't find any axioms. Every proposition that I studied had opponents or critics who claimed that that proposition was false. I would find literature reviews that seemed to resolve the issue but I found that these reviews were quite biased. They would cite the evidence on one side of the issue and ignore all the counter-evidence. The objective reviews that cited all the relevant studies were uniformly disappointing. They would cite one list of studies supporting the proposition and then another list of studies disconfirming the proposition. All potential theories were thus emasculated at birth.

I had a near Ph.D. level knowledge of mathematical statistics and I realized early on that sampling error is a major cause of the frustrating situation in literature reviews. In particular, I knew from theory that the significance test doesn't work and I was thrilled by Cohen's (1960) empirical proof that it doesn't work in practice. I was greatly disheartened by the fact that people ignored that paper and I endured 40 years of name-calling from my colleagues when I pointed out that the significance test doesn't work. Why do I fear arrogance? Consider my colleagues who would be enraged if I said that they were incompetent in their own field of specialty. They didn't know statistics. Certainly no one ever had any rational defense to my mathematical proofs, but they could convince themselves that they knew more than I did, and that I was an idiot even if I did have a reputation for brilliance. Arrogance has held progress to a halt for decades in science and can last thousands of years in religions where reason is regarded as a poor substitute for faith.

When Frank Schmidt came along, I found the friend I needed to show me how to make my ideas practical. Frank is immensely practical (as well as brilliant) and meta-analysis finally provided a basis for objective fact finding in the social sciences. As individuals, we cannot do studies large enough to generate trustworthy single results. But when we cumulate data across studies, we can produce reliable results. Not all differences between studies are eliminated by meta-analysis but almost every meta-analysis has eliminated the nonsense conclusion that "some say they find yes, but others say they find no". In most domains, the main cause of differences between study results is sampling error. In almost all cases, meta-analysis shows that the qualitative result is universal (though it varies in size) and that the proposition is thus confirmed. A young person who wants to develop a theory now can start from meta-analytic results to develop real axioms and hence a body of theory.

At present, the scientific view of sampling error is logically inconsistent. Meta-analysis has attained strong acceptance; more and more textbook writers cite a meta-analysis as the final arbiter of fact finding. On the other hand, the significance test is still used to falsely draw conclusions from single study results with an average error rate of 60%. The leading medical journals have banned the significance test. This will eventually happen throughout the social sciences.

There are other problems with current studies. During my years in the wilderness, I studied all study imperfections in seeking to explain the differences in study findings. In particular, no important variable in the social sciences can be perfectly measured. Despite the huge investment in measurement in psychology, we have only scratched the surface. We are still digesting the legacy of knowledge embedded in natural languages as is very evident in the Big Five literature in personality. Our current theories are very poor at showing how individual differences in personality enter into the determination of thought and action. We focus on situational determinants of behavior because they are easily visible and ignore the fact that personality is a much larger determinant than situation. That is, we are like the drunk who looks for his keys under the streetlight because he can see there even though he knows that his keys are not there.

The message that measurement is imperfect is universally confirmed by empirical results but is extremely unwelcome to working scientists. When I have said that results need to be corrected for the distortions due to error of measurement, my colleagues have almost universally condemned me. Their argument is simple: "My mentors don't correct their results. Famous researchers in the current literature don't correct. Therefore correction is a

bad idea." This is arrogance; falsely believing that you have knowledge that you don't have. It is true that this is also the product of social convention ("When in Rome,..."), but scientists specifically outlaw basing judgment on social convention and thus for a scientist, reliance on social convention despite disconfirming empirical evidence is arrogance.

Why is there so much error of measurement? Is it bad scales or items? The fact is that the main source of random error of measurement is randomness in human function. Empirical research has shown that there is a large random element to every thought, every feeling, and every action. Yost's law says that if you want to know what a person will do in a given situation, then just see what that person did the last time in that situation. Yost's law is true, but the extent of repetition is surprisingly limited. The correlation between successive responses is rarely larger than $r = .25$ and the high correlations are for well-practiced responses. This is critical to laboratory work in social psychology and communications. Often the key dependent variable is one isolated response and that response is unique to the experiment. This is a prescription for extremely large random error of measurement.

Measurement is peculiar in that you can often get strong agreement that there are problems. Most researchers will admit that other people have problems measuring their variables. What they cannot accept is that there is any problem with how they measure their variables in their research. I stress: humans have a large random element in all behavior. No matter how well thought-out your measurement scheme, the objects that you are measuring are NOT constant. The only way to get reliable measurement is to make several measurements and average across them to reduce the impact of randomness in the observed behavior. In this regard, I would like to say something about Occam's razor. William of Occam said that in a competition between simple and complicated theories, the simple theory almost always turns out to be true. I strongly believe this to be true of psychology. Randomness in behavior has caused severe perceptual problems for scientists. If you view a short sequence of random acts, you can often come up with some complicated scheme that will exactly predict this short sequence. This leads people to construct very complicated theories. However as Las Vegas gamblers learn to their sorrow, these complicated schemes do not extrapolate to new observations. **CRITICAL FACT:** randomness looks very complicated if you try to explain each variation. However when you average across responses to reduce the random component, the underlying trends are usually very simple. The facts that have been shown by meta-analysis are usually very prosaic facts. However this is true in other sciences. The real complications in science stem from theories in which many facts are woven together to reveal wholly unsuspected patterns. I would like to direct my last remarks to the topic of individual differences. Most researchers admit that people differ from one another, but few researchers realize how much people differ from one another. My first personal research topic in psychology was intelligence and differences there are huge. One triumph for behaviorism was that they figured out how to teach extremely retarded people how to tie their own shoes. I have a friend who read Einstein's theory of relativity in the fifth grade. Differences on major personality dimensions are equally large but less well studied. Most laboratory researchers admit that there are differences out there but they believe that those differences are irrelevant to their own research. I believe this to be false and I believe that laboratory research is hobbled by this false belief. I believe that research in personality has proved my point in regard to research in social psychology and communications.

The issue is this: Laboratory researchers using Analysis of Variance (ANOVA) methodology base conclusions about individuals on findings computed as group means. Is this methodology correct? Among ANOVA experts, this reasoning is known to be false. Group means will only provide correct information about individuals if there is no treatment by person interaction. If there is such an interaction, then the main effect group means can be misleading or even downright false. However, these experts have been ignored by working researchers; in part because the arguments have been presented in abstruse jargon that most researchers do not follow. What I will argue is that personality researchers have proved that the inexpert ANOVA methodology is false for social behavior. People with different personalities show different effects for the same treatment. A situational factor that affects some people one way will have a different effect on other people. I believe this is not just a rhetorical possibility but rather a proven fact in personality research.

I have long been interested in social psychology and when most social research shifted to departments of communications, my research partnerships shifted correspondingly. However I also keep up in personality research. There is a stunning pattern to findings there that relate directly to this issue. A topic such as social loafing becomes popular in social psychology. About the time that interest in this topic dies in social psychology, research on this topic starts in personality research. The difference is this. The social psychologist uses Analysis of Variance methods to interpret the data solely based on group means. The ANOVA dogma teaches that if you use group means, your results cannot be ideographic but must be nomothetic. (It is quite easy to construct mathematical examples that show this dogma to be false.) Thus social psychologists conclude that everyone is affected in the direction indicated by mean change. The personality researcher begins with a very different hypothesis. The personality researcher believes that people with certain personalities will indeed respond in the same direction as the group means. However people with a different personality will respond very differently. Not every personality hypothesis comes out, but in one area after another, I have noticed that the findings show massive disparity for some personality dimension. That is, take any given situational hypothesis from social psychology. There is some personality dimension such that people who are high on that dimension respond to the situation in the manner claimed by the social psychologist (the direction of the group mean difference). However people who are low on that dimension respond in a very different manner; sometimes with

no response and sometimes with responses in the opposite direction.

To summarize, social psychologists using crude and inexpert ANOVA methods come to some conclusion as to how all people think, act, or feel. With a lag of about 40 years, personality research shows that this conclusion is false. While some people react in the manner suggested by the group means, there is a substantial minority (and sometimes a majority) who respond differently. If we focus on current communications research, it is my belief that there is a fundamental flaw to ANOVA methods that look only at group means and ignore individual differences. When it comes to social behavior, it is usually the case that treatment effects differ substantially from one kind of person to another. To ignore this is bad science and we are very much paying the price for bad science right now!

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