# BRAIN SCIENCE PODCAST

*With Ginger Campbell, MD*

**Episode #83**

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**Interview with William Uttal, PhD, Author of *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience***

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## INTRODUCTION

Welcome to the *Brain Science Podcast*. I'm your host Dr. Ginger Campbell, and this is Episode 83. Before we get started I want to remind you that you can get detailed show notes and free episode transcripts at brainsciencepodcast.com. You can also send me feedback and suggestions at docartemis@gmail.com. I'm also DocArtemis on Twitter.

Today's episode is an interview with Dr. Bill Uttal, whose latest book is *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience*. This book was written for those working in the field, but it brings up issues that I think are important to anyone who is interested in how the brain makes us human. A key question we will address is whether brain imaging can tell us how the brain creates the mind.

Those of you who are regular listeners will appreciate that this conversation builds on the ideas of several recent episodes. I strongly suggest that you listen to last month's episode first. But don't worry if you're a new listener, because I will be back after the interview to review the key ideas, and I will provide links to the related episodes in the show notes.

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**INTERVIEW**

**Dr. Campbell:** My guest today is Dr. William Uttal, and his book is *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience*. Bill, it's great to have you on the *Brain Science Podcast*.

**Dr. Uttal:** Thank you for inviting me.

**Dr. Campbell:** Before we start talking about your latest book, *Mind and Brain*, perhaps you could tell us just a little bit about yourself.

**Dr. Uttal:** Well, I am more than anything else an interdisciplinarian. And nowadays I call myself a cognitive neuroscientist. I started out in physics; went into the Air Force and learned a lot of electronics; got a PhD in experimental psychology and biophysics; worked for my first postdoctoral job at IBM research at the Watson Research Center. Then I was invited to the University of Michigan, where I spent most of my life.

But then I got the wanderlust, and wandered off to the Naval Ocean Systems Laboratory in Hawaii. After that I moved to Arizona State University, first as chairman of the psychology department and professor of psychology, and then later as professor of engineering. I retired twice; once from the University of Michigan in psychology, and once from Arizona State University in engineering.

**Dr. Campbell:** So, over your long career obviously you've seen the field of cognitive neuroscience emerge. Your focus was research originally?

**Dr. Uttal:** Yes, but it was actually individual perception. 'Cognitive neuroscience' is a new name for a field that's been around probably longer than conventional psychology. It used to be called 'physiological psychology.' Probably back to the time of prehistory, people have been interested in the brain and their awareness, and they've developed all kinds of models and explanations to account for how our minds emerge from the organic tissue we call our brain.

**Dr. Campbell:** And one of the things that makes your book, *Mind and Brain*, really interesting to me is the fact that you come to it with a historical perspective. And as you're looking at the current studies, I like the fact that you put them into a historical perspective, and really show that what's going on now is in many ways a continuation of the past—both for good and evil.

**Dr. Uttal:** That's entirely correct. Modern cognitive neuroscience doesn't make much sense unless you look at it in historical perspective. Also, if you don't simultaneously emphasize both aspects—the cognitive and the psychological on the one hand, with all of its problems; and the neurophysiological and neuroanatomical on the other, with all of its problems—it's a tough nut to crack.

The problem has been considered by some philosophers—Schopenhauer, most notably—as the 'world knot;' the allusion being it's the most important problem there is in the history of science. And that's the way I feel about it nowadays; that there's nothing more interesting, more exciting, more challenging than the mind-brain problem.

**Dr. Campbell:** Well, obviously, I agree. That's why I do this podcast.

Can you tell us a little bit about how *Mind and Brain*, the book, came into being?

**Dr. Uttal:** Well, it's kind of an interesting history. I was consulting for an army laboratory. They had come to me because of some of my earlier works, and we had a kind of a meeting of the minds. They were interested in building an argument to preserve their psychological testing and training methods. And they were under a great deal of challenge, I guess you'd say, from their upper management to drop all of the behavioral measures and go to brain imaging as an alternative. My books had been very critical of brain imaging, and that's what led them to me.

I worked with them for almost a year. Then, all of a sudden, we came to the agreement that there was a book here that should be written. They were wonderful; no pressure on me. I could say anything I wanted to. It was an impetus for me to get the book done on schedule. So, that was the immediate reason that I wrote the book. However, I must say that it was a continuation of what I was going to be writing anyhow, and I was delighted to have the support to do this project.

**Dr. Campbell:** What do you see as the intended audience for this book?

**Dr. Uttal:** Oh, that's always a question. I think when I sit before my keyboard that I am speaking to colleagues: senior-level graduate students, philosophers, cognitive neuroscientists; people who are fairly well-versed in the basic scientific issues that are involved.

**Dr. Campbell:** But I have to say, since I am not a worker in this field, that I do think this book is something that can be read by someone outside the field; but if you were a student, you would obviously read it in a different way.

Talk a little about what your goals are with this book.

**Dr. Uttal:** There are some ideas that I want to get across. Some of them we've already alluded to—how difficult and challenging the problem is. From that comes an important idea, and that is that we really don't know as much about how the mind and the brain are related as it seems that we do. There's an awful lot of hyperbole in this field. People are overstating what has been accomplished, promising that we're on the edge or the verge of some kind of great breakthrough like reading the mind, or if I may, controlling machinery from the brain directly.

So, these general background goals are what I'd set for myself. But there are other specific ones, and one of them is to establish a scientific foundation that argues against what I've called the 'classic phrenological idea.' Phrenology was a very popular field a couple hundred years ago. When it was at its heyday, the idea was that you could feel around on the skull of a person and find where there were bumps or crevices, and assign each of these locations to a particular cognitive process; like acquisitiveness had a particular bump just over the ear.

This idea was debunked 150 years ago, but there are still vestiges of it. And the major vestige is the idea of localization; that there are particular places on the brain (we don't do the skull thing anymore) that are associated specifically with particular thoughts, mental activities, cognitive processes, whatever you want to call them. And so, my crusade for the last 12 years or so has been to argue for and present evidence that there is no localization; that the brain responses, even to very, very precisely defined stimuli, are distributed over major portions of the brain.

So, that's the great theme. Within that there are so many other details. If you recall, in the first chapter of the book I tried to spell out in detail what were the postulates, the assumptions, the places that I started and my thinking started. It has been, for me, a fascinating experience over this last decade or so that my ideas have developed, and I've seen consistency, I've seen a logical sequence in them. At the same time I understand that mine is a very, very minority view, and that many, many people—scientists in the field—disagree with me, and believe in the notion of localization and function-specific areas of the brain.

My feeling is that the data is coming around to my point of view, and that people are going to have to change their points of view. But I'm not really convinced that anybody can change the huge commitment that we have to fMRI—functional magnetic resonance imaging—techniques.

**Dr. Campbell:** There are a couple of things that I think you talk about early on that we might want to touch on now, and then come back to in more detail. One is that the basis of your book is really doing a very thorough review of the current literature—mostly the imaging literature—and specifically looking at the big meta-analyses. And we'll talk about why that's important.

But I also remember that early on you talked about a couple of the problems with imaging studies that crop up over and over again; in particular, underdetermination and lack of replication. Perhaps we could go ahead and just talk about those two problems and what they mean.

**Dr. Uttal:** These are two of my favorite topics. And I'm going to deal with the easy one first. The literature in this field is terribly inconsistent. There is a remarkable lack of consistency and lack of replicability in the work that is published all the time. I have another book that's actually in press now, that's entitled *Replicability of Brain Imaging*. In that I looked at how well MRI images replicated within one subject (intra-subject), between subjects, between experiments, and between meta-analyses. And it is astonishing how little agreement there is.

I don't want to go into the details unless you wish me to; but let me say that meta-analyses—which are supposed to be the average of the average of the average—simply don't agree in those relatively few instances when you can find meta-analyses that deal with the same subject, that can be compared with each other. So, as far as that goes, I have to say that replicability is not a characteristic of this field; and it is a major problem.

Now, the other issue—the underdeterminativeness—is primarily a problem with the psychology. The problem with any behavioral measure or any psychological measure is that there are literally an innumerable number of alternative explanations that could explain what's going on in something. For example, if you carry out a learning experiment, and you have data on how the behavior of a person changed over time as they went through trials of learning, if you stopped right there you would have an incomplete knowledge of what mechanisms in the head might have caused that.

All behaviors (as are all mathematical models) are underdetermined, or indeterminate. They can describe, but they cannot pick out the unique internal mechanism that accounted for them. This is a mathematical proof. This is something that has come to us from Automata theory; the highly abstract mathematical theory that deals with machines that make decisions for themselves.

The hope is that you would be able to find some way to look into the brain and find out what the real determinations, the real mechanisms are. But it turns out that you can't do that. It turns out that at the level of analysis at which the brain is being studied you cannot find an exclusive answer—a determinative answer—to the question of what kind of mechanism. So, psychologically we'd love to have a definitive statement of what kind of machinery would produce what kind of behavior. The only way we can do that would be to open the brain; but when we open the brain it remains functionally closed, because of its enormous complexity.

I can generalize this point to say that there are a very large number of scientists in the field who do not appreciate that there are fundamental limits in cognitive neuroscience, just as there are fundamental limits in physics. The speed of light, the inability to construct a perpetual motion machine, these are limits that are accepted by physicists. Psychology and cognitive neuroscience have their own limits, and these prevent us from resolving the underdetermination, or the indeterminativeness of the mechanisms that account for observable behaviors.

**Dr. Campbell:** As you were talking I found myself sort of thinking of the problems with String theory.

**Dr. Uttal:** Exactly! String theory has its problems. People are disagreeing whether or not it's the Holy Grail, or whether it's simply a mathematical truism. The problem with string theory is the same as with the brain; it has too many loose ends. Too many factors are involved in it to resolve which version is the true one, and the experimental data exists to support neither of these theories in modern science.

**Dr. Campbell:** And in the book you said (and I'm sorry, I don't remember what page this was on) this means that all cognitive reductionist models of the mind are underdetermined.

**Dr. Uttal:** Yes. There is not enough data in the cognitive theories to resolve the underdetermination. And where we would look for the explanations in the brain, it's hopeless, because the brain is so complicated, and is not amenable to analyses of the kind that are necessary.

By the way, there's another very, very important point that we're talking around, and that's the levels of brain that can be studied. You can study the brain in terms of the biochemistry of synaptic junctions. You can also study the brain in terms of the electroencephalograph. Between them there are other levels of analysis.

Our best guess now—without a lot of supportive evidence—is that the brain is best studied at the level of the neuronal nets (the network of cells that make up the brain tissue);that that is where the mind resides, in terms of the information processes at this very fine microscopic network. Unfortunately—because that problem is so complicated at that level because there are so many neurons—our technologies now are the MRI machines, which, though wonderful for studying anatomy and physiology, blur and obscure all of the information processing that is going on at the microscopic level.

When you do an MRI, you get a picture of large numbers of cells. Their activities are all averaged or pooled together. MRI does not attack the problem at the level it should be attacked, if we could attack it there. The microscopic neural networks are the source of the information that ends up being our minds.

The tools that we have popular today for studying the brain are tools that operate at a much higher level. And in spite of all of the activity that's going on in this field nowadays, I think it can be at least argued—not accepted by most people, but at least argued—that we are barking up the wrong tree. We are looking for information about mind-brain relationships that have already been lost—have been already averaged out of existence—when we use an MRI machine.

**Dr. Campbell:** That's a really important concept, so I'm going to repeat it for the listeners. The point is that brain imaging is at the wrong level of analysis if we are trying to explain how the brain makes the mind.

**Dr. Uttal:** Yes.

**Dr. Campbell:** And so, this underdetermination problem also means that scientists who agree on the basic assumption that the brain makes the mind can come up with models that are totally opposite; like for example, you mention the opposing models of Christof Koch and Susan Greenfield.

**Dr. Uttal:** Yes. The two of them have slightly different points of view; but their explanatory models—it's sometimes hard to call these vague verbal statements 'theories'—they really are simply not operating at the proper level of analysis.

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**Dr. Campbell:** So, we get to a situation where it looks like the mind is being created at the microscopic level, but this information disappears with imaging because it's pooling a large number of neurons. What about trying to use microelectrodes?

**Dr. Uttal:** Microelectrodes are a wonderful problem. They allow us to get at single neurons and observe their behavior. People now are doing a lot of work on making multiple microelectrodes and recording simultaneously from very large numbers. When I say 'very large numbers,' I mean by that something like 100 or 1,000. However, that's not enough. What happens in almost all of these cases is that the technology becomes the issue.

Can I record from 100 or 1,000 neurons? Yes; that's a technological tour de force, but it can be done. But what happens when you then look at 1,000 neurons, all interacting in idiosyncratic manners, each neuron with perhaps 1,000 different inputs onto it? You have a data processing problem there that takes you right back to that problem we talked about before—in terms of the computability and the complexity; the ability to look at that. Anyhow, even 1,000 neurons is probably not enough to encode a thought.

**Dr. Campbell:** It's still several orders of magnitude away from what the brain is doing, isn't it?

**Dr. Uttal:** Exactly!

**Dr. Campbell:** So, if we can't use imaging to solve the mind-brain problem, what can we use it for?

**Dr. Uttal:** I'm glad you asked that; because I'm a real fan of MRI. I think it's probably the most important technological development in medicine since people learned to wash their hands, or the invention of anesthesia. What it can do—and does extremely well, and to the everlasting thankfulness of everybody who's had an MRI instead of exploratory surgery—is to measure the anatomy and the physiology of the brain.

It's a magnificent machine. But the important thing for my field, and our field, is that it's not very good at establishing what the mechanisms are of the brain that account for the mind. It's not very good for cognitive neuroscience studies, whereas its magnificence for neuroanatomical and neurophysiological studies is unmatched.

**Dr. Campbell:** But it seems like researchers are focused on answering the question that they assume imaging can answer; which is where processes happen in the brain. But your book argues that they're not really answering that question.

**Dr. Uttal:** Well, the problem is again that the question *where is something happening in the brain* may not be a good question. That's a question that has its roots back in phrenology. That's what phrenology was all about. And for 200 years, all of our brain sciences have been dedicated to 'where in the brain.' Classic physiological psychology, for example, would stimulate a particular place in the brain, or they'd cut out a particular place in the brain, but everything they did was place-oriented.

And it's just not the only question when we know these days that the responses that are elicited in a brain when you ask a person to think about something, or when you provide them with some kind of a task, the responses are distributed over vast regions of the brain. And it is probably not too much of an exaggeration to say that almost all of the brain is involved in almost any thought.

And when you think back to the psychological level, it's pretty much the way psychology works, too. We can be emotional, but not without perception, not without attention, not without having learned something. So, the idea of looking for narrowly-defined little functions or modules of behavior, and then looking in the brain for little narrowly-defined regions of brain activity that account for them is, I believe, illogical.

**Dr. Campbell:** Yes. Before I read your book I thought that the problem with getting valid imaging studies was that we needed to have psychologists design good experiments. But now I appreciate that it's even harder than I imagined; and maybe impossible, if we're asking the wrong questions.

**Dr. Uttal:** That is entirely possible.

**Dr. Campbell:** I'm just going to briefly mention another thing that you had in the book which is relevant, which is that these complex functions that are often trying to be studied with imaging aren't necessarily even being consistently defined.

**Dr. Uttal:** You mean what we're asking a person to do in a psychological test?

**Dr. Campbell:** What is it that we're measuring, once we get past something like 'Did you see that black dot?' For example, you have a chapter on thinking and problem solving, and I think you mentioned that it's really pretty hard to even necessarily define or separate out those two things.

**Dr. Uttal:** Right. That's the same problem we were talking about before when we talked about the psychological parameters. Psychologists do not do very well in precisely defining their terminology. Thinking, decision-making are very, very complex cognitive processes. And what we do in the psychological laboratories is to constantly refine, and simplify, and try to get an experimental paradigm set up that will allow us to control almost everything. But it doesn't work, because of this intrinsic interconnectivity of the psychological processes.

The chapters in my book that are separated out—the different topics—from each other, that's an artificial thing that I just had to do to keep the book from being an unintelligible mishmash. But in fact, it's very hard to study any psychological process without involving all other psychological processes. This means then that the brain is being challenged to do something that it really probably can't do. Where we have learning, the brain may have processes that are concomitant or correlated with it, but they are of a different language, a different level of discourse, a different level of analysis.

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**Dr. Campbell:** We've talked a lot about the underdetermination and the problem of the fact that the imaging is maybe asking the wrong question, but there are some other problems with trying to correlate the results of the brain imaging. And your book does go into these in quite a bit of detail. Do you think there's any more that we should talk about before we start talking about the meta-analysis and how that fits in? I had in mind things like the one-to-many problem, and the many-to-one problem; those sorts of things.

**Dr. Uttal:** Right. The one-to-many problem is, of course, underdetermination in another set of words. One psychological process can be explained by a very large number of physiological mechanisms if you don't have any other constraints. So, that means pretty much the same thing.

**Dr. Campbell:** And the many-to-one means…

**Dr. Uttal:** That means in many different parts of the brain many different brain arrangements, machinery, interactions may produce the same kind of behavior. This is also part of the underdetermination problem. You can't say which one of these many mechanisms produced it. Also, we're learning now that there are not areas in the brain like face perception areas, or house perception areas; what there are are different configurations, which are not amenable to a study with an inadequate number of microelectrodes or an MRI.

**Dr. Campbell:** You talk a lot in the book about the various data processing issues. And I don't think I want to get into that in any detail; I'd refer people to the book. But I would like you to talk a little bit about one aspect of this, and that is what is euphemistically known as 'double dipping;' because I think that's kind of an important fundamental.

**Dr. Uttal:** Double dipping is one example of a statistical artifact that can mislead you. Someplace in the book—and I've forgotten, myself, where it was—I quoted a statistical analysis that was done to go from the raw data to some kind of an explanation of what was going on in the brain. And it turned out that there were innumerable numbers of statistical tests, all of which are susceptible to bias, misunderstanding, and error. And when you have a chain of mathematical steps, each of which is almost independent of the other, you're very, very liable to have some artifact, some incorrect calculation built in.

The gentleman who did the double dipping work, Mr. Vul[1], was quite correct in observing that they were using the same data for not only identifying what parts of the brain they wanted to study, but then studying those same areas that were defined by the first dip. That was just one example of many different statistical errors.

In my new book, in which I deal more with meta-analyses, there was a fascinating thing that happened. I started listing and tabulating the number of potential biases and errors that could get into analysis of that kind. And before I was done

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[1] Vul, E. , & Kanwisher, N. (2010). Begging the question:The nonindependence error in fMRI data analysis. In S. J. Hand & Bunzl (eds.) *Foundational Issues for human brain mapping* (pp. 71-79). MIT Press.

I had four pages of potential errors, potential confounds, potential mistakes. Some of them were interpretive, some of them were statistical, some of them were mechanical, some of them were design errors in the experiments.

This is a very, very complicated field. One thing that's happened nowadays is that people have moved away from trying to solve the mind-brain problem. A lot of people in this field will say, 'That's not what we're doing.' They've fallen back; they've retreated to statistics. They've retreated back to studying the methods that you use to measure brain activity, rather than the problem of how the brain and the mind are associated.

Now, this may be a necessary part of the problem; you may have to do this before you can do the rest of it. I guess the point is that there is far more to cognitive neuroscience now than the mind-brain problem, not the least of which is the efforts on the part of very sophisticated mathematically-trained people to study the organization from a totally non-cognitive point of view.

**Dr. Campbell:** Just to finish up on the double dipping, I'm going to try out this sort of plain English explanation of it, and you can tell me whether or not it is accurate. My impression is that this basically means that you look at your data—for example, you're doing an imaging study—and you say, 'Oh, the signals were really high in this area,' and then you define everything else based on that. So, for example, you're going to define your cut-offs so that that area will show up. And before you know it, you've basically magnified that.

**Dr. Uttal:** That's exactly correct. The words that they use now are 'regions of interest.' These words mean that before you do your regular experiment, you use some procedure to narrow the regions you're going to study, and then once you've narrowed the regions, you come in and you study those regions. Well, that's double dipping; that's using the information that you got in the preliminary experiment in the secondary experiment. And you bias your results there by limiting or constraining yourself to the regions that were originally defined in the first dip.

**Dr. Campbell:** And it's not like this problem is isolated to this particular area. I mean we see this problem in clinical medicine when somebody is looking at data from a study and they see a correlation, and they go back and re-analyze the data in light of that. But since that was not what the experiment was really designed for, that's again the same issue. They keep forgetting that, for example, an observational study is supposed to give you ideas to test, and that you shouldn't test those ideas off the same piece of data. That's why we have no replication, because everybody's just running around and around in the same data.

**Dr. Uttal:** Are you acquainted with Professor Ioannidis?[2] He takes out after drug testing in general, and points out that the whole idea of testing for small differences in large groups of people may lead us to over-generalize from the data. The generic problem here is that a lot of people who use statistics are not really knowledgeable about the potential biases that can be introduced into their work. And sometimes this happens without people actually realizing what they're doing.

One of my favorite papers is one by a group headed by a gentleman by the name of Ihnen[3]. He took 10 women and 10 men. He was looking for a gender difference between men and women. He had 10 men and 10 women, and he did the MRI experiment and found, sure enough, there were statistical differences between the men and the women. And he was about to publish this, when he

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[2] Ioannidis, J. P. A. (2005). "Why Most Published Research Findings Are False". *PLoS Medicine* **2** (8): e124.

[3] hnen, S. K. Z., Church, J. A.. Petersen, S. E., & Sclaggar, B.L. (2009) Lack of generalizability of sex difference in the fMRI Bold Activity associated with language processes in adults. *NeuroImage*, **45**, 1020-1032.

went back into the laboratory and said, 'Maybe I'd better do a permutation study here. Maybe I'd better mix up my subjects in a different way.'

What he did was to take 5 women and 5 men and put them in one group, and the other 5 men and the other 5 women and put them in the other group, and then do the same experiment over again. And lo and behold, what he found was that there was an equal difference to the first experiment, between the two mixed groups.

The point being that sometimes, even when you find a statistical difference between two groups of people that would suggest that there really is a difference between these two groups of people, it may be artifactual; that it may be a curiosity of measurement that has nothing at all to do with what you thought it was. And I am sure that an awful lot of the literature that's published these days is victim to this and other kinds of experimental biases.

**Dr. Campbell:** Which brings me to the question of why is it that false positives are more likely than false negatives.

**Dr. Uttal:** I don't know the answer to that question, because to my understanding it depends upon where you set your criterion levels; that the false positives and the misses, exactly what the relationship between them is depends upon where you put your criterion level. So, it's not always that case. I believe I'm correct about that.

**Dr. Campbell:** But there is publication bias. Since they don't publish false studies for the most part, that will at least bias the literature anyway.

**Dr. Uttal:** Well, related to that is the fact that (I hate to be critical about everything, but I guess that's my role in life) the very act of setting up a tightly-controlled experiment is more likely to produce false positives than it is to produce misses. And the whole Zeitgeist in psychology, of course, is to tightly control everything you possibly can, and then test for differences. And in doing so, we may be forcing ourselves toward seeing things that are not there—false positives.

**Dr. Campbell:** Well, I guess we'd better start talking about meta-analysis. This is a focus of your book. So, first of all, for the sake of my non-scientist listeners, will you talk a minute about what exactly a meta-analysis is, and why we might want to use them in this situation?

**Dr. Uttal:** The idea of a meta-analysis is based upon the notion that if you have more and more data, you're going to get closer and closer to the truth. One of the problems with MRI experiments, in particular, is they're expensive; and relatively few subjects are run compared to others. So, you might have an experiment with two subjects. And the idea in a meta-analysis is you want to pool all of those data from the 10 experiments and come up with what would be statistically considered to be a more powerful test of the differences.

It's like averaging, averaging, averaging in cascade. And the more you cascade, the more you get closer to the exact average value. For example, if you took a whole bunch of young men and you measured their height, you might find out that they're 5'10" on the average. But if you had 10 or 12 people, instead of 100, you might have an average that is a little bit different, and not quite so accurate.

OK, so what does the meta-analysis do? The meta-analysis tries to pool all of the available data into one grand average. At that point you believe and hope that you have a better estimate of the average; which, in the brain situation, would be which brain areas are responding to your stimulus. The problem is—and this is a problem that is really very difficult to handle right now, because there aren't a lot of identical meta-analyses that you can compare with each other—but let's say that we did a meta-analysis and we pooled a lot of data in single reading—words. What brain areas are active when you read a single word? Then we'd like to have a completely different meta-analysis, perhaps with different experiments that we're pooling, and we'd like to look at the output of that and compare the two meta-analyses.

And in general, they do not agree. What this means is that the work is heavily dependent upon the method. You should come, with the two meta-analysis techniques, to the same answer; because they are the grand averages looked at two different ways. And if you don't get them, then your data is perverted, and somehow it's methodologically dependent. It's a strong argument that you're not measuring something that's significant, you're measuring something that's dependent upon your method.

**Dr. Campbell:** So, we look at all these brain imaging studies using meta-analysis, and what's the general trend?

**Dr. Uttal:** Disagreement, inconsistency, unreliability. That's the trend.

**Dr. Campbell:** Right. So, you could just about pick a brain area and a cognitive function, there's at least one study that that's lighted up for. Right?

**Dr. Uttal:** It's usually not one area of the brain; it's usually the configuration. The outputs of meta-analysis that are typical these days are four or five regions of the brain that are activated. What happens is, if you have two meta-analyses, they might have different patterns of these areas. The numbers that I found were about 45% agreement; which I don't think is very good, considering how much data you have supposedly averaged.

**Dr. Campbell:** And then, for those of us outside the field, we never hear about these meta-analyses; all we ever hear is the *New York Times* touting the latest single study that claims that whatever areas they were looking at are the ones that are active.

**Dr. Uttal:** Right. If you see a study that says a single area lights up when I think about something, it's almost certainly wrong. And I've recommended that no publication should be allowed which defines a particular area of the brain as being associated with a particular cognitive process, until it's been replicated a couple of times. There's this issue we haven't spoken much about: phrenological localization and distribution. As I said before, I believe, most brain experiments now are showing up with more than one area.

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**Dr. Campbell:** Bill, since we're almost out of time, I guess we should shift away from this for a few minutes and talk about the practical implications of this in terms of the real-world applications.

Of course, you said already that when we're doing this imaging we're at the wrong level to answer many of the questions that we want to ask, and we lack the tools to deal with the true complexity of cognitive processes. Yet we're faced with all this exaggeration and hype. Is there a particular example of how this hype plays out that is the one that comes up for you when somebody asks you for an example?

**Dr. Uttal:** There are so many of them. I guess one of the things that I find the most troublesome is people who say that they are reading the mind; they've taken an MRI, and somehow they have interpreted what a person was thinking about.

This work is getting a lot of publicity recently; and from my point of view—and I acknowledge it might be a minority point of view—such a thing only works at all in the sensory regions; the primary projection regions of the brain. And there, there's enough topological or retinatopic preservation of information that you can find a distorted little map, if somebody is looking at a cross or something like that. But to attempt to reconstruct what a person is seeing from that kind of approach seems to me to be on the fringes of plausibility.

**Dr. Campbell:** And the experiments that really have been done, it has basically been they've taken scans of people when they were looking at particular objects, recorded what their scans looked like for those objects, and then scanned them again and looked for those patterns to see if they could tell which of some few predefined objects they were looking at. Right?

**Dr. Uttal:** Yes.

**Dr. Campbell:** That's a long way from real mind reading.

**Dr. Uttal:** Right. But that's where the mind-reading business is right now.

**Dr. Campbell:** And then, of course, there's the issue of lie detection. And the fact that lie detector tests haven't gone away, even though they're notoriously inaccurate, makes it worrisome what they might try to do with the MRI in this area, doesn't it?

**Dr. Uttal:** Well, that's my favorite. I actually wrote a book on neuroscience in the courtroom[4], in which I pointed out that of the 16 experiments I knew of at the time I wrote this book a couple of years ago, none of them agreed with each other. The whole business of lie detection is one of those ways in which people have gone so far overboard that it's becoming embarrassing. There are companies now that will offer you brain image lie detecting for thousands and thousands of dollars, based on what the scientific community would believe to be completely incorrect data.

**Dr. Campbell:** So, that's really as pseudoscience as those dousing machines that governments have bought, that were supposed to be able to detect bombs, that were totally bogus. It's about the same level.

**Dr. Uttal:** Polygraphs didn't work; the new methods don't work.

Getting back for a moment to the reconstruction of experiences from brain images, there's another issue there that's quite important. People use the word 'reconstruction.' Reconstruction, to me, means that you're taking the bits and pieces and putting them together to recreate an image.

What I think they're really doing is recognizing. They have a library of responses, and they choose to manipulate the MRI data so that they can select one, or a few, from that library. And that recognition or selection is vastly different than reconstructing. So, even in the limited context of the primary visual projection area this doesn't hold water.

**Dr. Campbell:** Bill, another issue that you brought up in your book was the fact that cognitive neuroscience and psychology both lack a specific universal theory. And this sort of comes back to that whole underdetermination thing; we don't even have a single theory to try to test. But why is this important?

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[4] *Neuroscience in the Courtroom* by William R. Uttal

**Dr. Uttal:** When we started I said that I think this is the most important problem in human history. This, and cosmology, and genetics; which are much simpler problems, in a mathematical sense, than is the mind-brain issue. So, I believe its essential importance is that it lies at the roots of human nature; who we are and what we are, and probably where we're going.

**Dr. Campbell:** You actually close your book with what you call 'A New Brain Metaphor.'

**Dr. Uttal:** Right.

**Dr. Campbell:** First of all, talk about why you propose a metaphor rather than a theory.

**Dr. Uttal:** Because I don't think the group of properties that I've included in my new metaphor deserves to be called a theory. It's not as complete, nor as elegant. And it doesn't really explain anything; all it really does is describe some properties about the way that scientists are looking at the mind-body problem nowadays.

15 years ago it was very different. The phrenological tradition was still very strong; and people were publishing papers regularly, showing that this spot on the brain was uniquely associated with a particular cognitive process. The data, as it's come in, has revoked this idea completely. Now we know that much of the brain is involved in responding to almost any stimuli. We know that there is heavy interconnectedness between all the parts of the brain.

Another thing that's different about the metaphor is that it really emphasizes the multiple functions that are carried out by any area of the brain. No area of the brain is specific, just as no area of the brain represents any particular cognitive function, and many areas of the brain can do the same thing.

Now, these are just properties of a new way of looking at the brain. That's why I call it a metaphor. I wouldn't have had the chutzpah to suggest that this is a theory. It's a metaphor that's momentarily useful. It summarizes some of the properties of the brain, but it doesn't rise to the level of theory.

I hope I've answered your question.

**Dr. Campbell:** Yes. And I think it's important to notice that all we've been working off of, for thousands of years, have been metaphors; the metaphors have just been constantly changing. And you're just saying we need to change from our localization metaphor. That's one that we are comfortable with. And it's a little bit, in some ways, hard to let go of something that we're so comfortable with.

**Dr. Uttal:** And we all have to go down one level. We have to go to the level of the neuronal networks—the networks of individual cells—and how they're interconnected, and how their synaptic efficiencies change with experience, and how, as a collective entity—not as a chunk of the brain, but rather as a state of millions and billions of neurons—somehow this magic occurs, and out comes consciousness, the mind, behavior, whatever you wish to call it.

**Dr. Campbell:** I think your book makes a very important contribution to this conversation. And, while it might still be somewhat of a minority viewpoint, in the last couple of years it seems to me like there's an increasing number of people that are saying this same thing in different ways, and also even beginning to try to attack the problem with things like network theory.

But there is something that I want to clarify before we close. We're not saying that localization never exists. Right? Because it clearly does exist at the level of the spinal cord and at the level of sensory organs like the retina.

**Dr. Uttal:** Absolutely! And I don't know how many times I've said this, in every one of my books, and it never seems to be caught on to. Localization is a perfectly good metaphor if you're talking about the primary sensory areas of the brain or the final motor output areas of the brain.

There's no question that the literature has made it absolutely unquestionable that the brain has specialized regions for vision, where the signals from the eyes come up into the primary visual cortex in the back of the brain, and similar localized function comes down from the motor cortices.

My comments deal entirely with what I am assuming is what people mean by the word 'cognitive.' It's beyond the sensory, beyond the motor. It is the processes like thinking, emotion, attention, learning: the chapters of my book. And that's where the localization metaphor doesn't work anymore.

**Dr. Campbell:** And the other important point—and you made this well already—is that the MRI is a very useful tool for providing anatomic information, and it's useful in so many diagnostic situations. So, it's not that we're attacking MRIs.

Is it possible that we could redirect the use of the functional MRI in more productive directions? I mean would it help, for example, if we changed from this pooling of data from several people—pooling those together; which obviously is bad because of the variability—would it be better if we started doing serial studies of individuals?

**Dr. Uttal:** Even there, the problem of reliability raises its ugly head. In my book—not in the one that you've read, but in a new one that's coming out this summer—I deal with the problem of variability within an individual. It turns out that if you take a single subject, in which things should be as stable as it's possible, and you put them in an MRI on 30 successive days, and you ask what are the brain responses to asking them to think about a cow, the answer to that is that on almost every day there will be a different brain response.

So, serial use of a single subject doesn't overcome the problem. There's variability intra-subject, inter-subject, inter-experiment, inter-meta-analysis. Variability is so severe here, that we are floundering in our search to find a stable piece of data. And no theory, no explanation is going to work unless we have reliable data. Without reliable data it's all hand waving.

I have a feeling—and I can't document this really—that what we're really looking at is a quasi random system that is constantly changing, and adapting, and readjusting; and that there's really no reason to expect that there should be anything stable, except for some sensory and motor regions.

**Dr. Campbell:** Well Bill, is there anything else that you would like to add before we close, that I've left out.

**Dr. Uttal:** I think that there's another general point about resource allocation. In all of my critical comments there's one positive theme, and that is that I really believe strongly that behavioral science—psychology—can carry its own weight. And I'm afraid that what's happened over the last 12-20 years, perhaps, has been that psychology has been diminished in its role because of the superficial attractiveness of the MRI machine.

These pictures of brain activity are absolutely fabulous; they are flabbergasting in their beauty and their seductiveness. And people have turned away from asking questions about the nature of human thought that we really can attack now. We may not be able to understand the mechanisms, but we can understand how we behave, and answer questions that may be more useful in the immediate term than this wonderful but frustrating search for the answer to the mind-brain problem.

**Dr. Campbell:** Yes, it must be frustrating to be an experimental psychologist now, where it's almost like you have to have imaging or you're not going to get funded to do a perfectly good study. I think we have similar frustrations in clinical medicine.

Bill, I always like to ask my guests—and you have much experience, so I definitely want to ask you—do you have any advice for students that are interested in cognitive neuroscience or cognitive psychology?

**Dr. Uttal:** Yes. My feeling, as I've advised students my whole life, is to find out what really excites you, and follow that. If cognitive neuroscience and cognitive psychology excite you, give it your all, regardless of anything that old cranks like me have to say about the limits of the field. Because there is an enormous amount of wonderful neuroscience that's being done nowadays that may not be directly related to cognitive processes. And as I said before, psychology is probably, in the short run, more likely to produce important answers to questions of human welfare than the neuroscience aspects are.

But most of all be true to yourself. If you find something that's exciting, give it a try. I have a grandson who is a wonderful guitar player; and he's decided not to go to college right now, but to pursue the possibility of a career as a member of a band. Blessings on him! He loves it; he's so happy. And I'm so happy for him. Anybody who pursues a career for some secondary reason is probably going to be disappointed. Passion for the ideas in a field—it doesn't matter what it is—that's the way to go.

**Dr. Campbell:** Somehow that seems to be the answer that most of my guests give. It must be true!

**Dr. Uttal:** Right! I'm not surprised. Because we've been lucky; neuroscientists and cognitive scientists have all been blessed by operating in the most exciting field of modern science.

**Dr. Campbell:** Yes, when I decided to do the *Brain Science Podcast* five years ago, I chose it because that was what I was reading about. And I don't have any control over how fascinating and exciting the field continues to become; I started because that was what I was interested in.

**Dr. Uttal:** And there's always a possibility that some Einsteinian genius might come along and say 'Whoa! There it is! The theory that's been eluding us for all of these years has suddenly become tenable.'

**Dr. Campbell:** Well, I think if we have a new generation that starts thinking of the brain less locally, maybe new ideas about how to picture that will emerge.

**Dr. Uttal:** Right.

**Dr. Campbell:** Because guys of your generation and my generation, like I said, it's hard to let go. I've been thinking about that a lot, ever since I interviewed Miguel Nicolelis; trying to get my head around the implications of it. And there are an awful lot of things, I think about them that way, and I can't figure out how to rework it.

**Dr. Uttal:** Got it!

**Dr. Campbell:** Well, Bill, it's been really interesting talking to you. I hope you've enjoyed it as much as I have.

**Dr. Uttal:** Indeed I have. Thank you so much for inviting me, Dr. Campbell.

**Dr. Campbell:** You're welcome.

[music]

I want to thank Dr. Bill Uttal for taking the time to talk with me about his new book, *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience*. This book takes a very detailed look at the increasing use of functional magnetic resonance imaging in the field of cognitive neuroscience. It's not a book for the average listener, but I highly recommend it for those of you who work in the field.

Before I review the key ideas from today's interview, I want to remind you to visit my website at brainsciencepodcast.com, where you can find detailed show notes and free episode transcripts.

*Mind and Brain* is a critical look at the use of fMRI in cognitive neuroscience. Uttal seems to have two main goals in writing this book. One is to explore the limits of fMRI, and the second is to challenge the assumption of localization. He concludes that brain imaging is at the wrong level of analysis if we want to explain how the brain makes the mind. This is because he thinks that the mind probably emerges at the level of the interactions of individual neurons; and every voxel—which is like a 3D pixel in an MRI image—represents thousands of neurons, so the level of detail has been lost.

Then there's the problem of poor reproducibility, which results in divergent results between studies. However, there is a much more fundamental problem, and that is the possibility that imaging studies are focusing on the wrong question. The fact that the various studies seem to show activity in so many different places suggests that asking *where* is the wrong question. Instead of asking where a particular function resides in the brain, Dr. Uttal proposes a new metaphor in which much of the brain responds to almost any stimulus, and every area of the brain participates in multiple functions.

He says several times that challenging localization is a minority viewpoint, but I actually think this is changing. I refer you back to Episode 79 with Dr. Miguel Nicolelis, and also to last month's episode. While I agree with Dr. Uttal that we may be over-committing limited resources to brain imaging, I don't think our prospects for understanding how the brain generates the mind are as bleak as he supposes. Researchers like Olaf Sporns[5], who I talked to back in Episode 74, are beginning to tackle the problem of the brain's complexity with tools like network theory.

We will be following up on this topic very soon with an interview with Sebastian Seung, whose new book, *Connectome*, argues for the importance of developing the techniques for unraveling the brain's wiring diagram at the individual neuron level. So, stay tuned for that.

For those of you who don't listen to the announcements, I want to mention that I always include the episode announcements in the show notes at brainsciencepodcast.com. If you want to get the show notes automatically, be sure to sign up for the *Brain Science Podcast* newsletter at brainsciencepodcast.com. That's the best way to make sure you don't miss new episodes when they are released.

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5 *Networks of the Brain* by Olaf Sporns

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Thanks again for listening. I look forward to talking with you again very soon.

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