



Perspectives on Research

jur interviews Thomas Smith, Ph.D.

Thomas Smith is Professor of Sociology at the University of Rochester.

jur. What kind of research are you involved with?

Smith: I've always been interested in strong personal ties. They were taken up in the study of the so-called primary groups in my field. I had sort of drifted away from that for a while, but came back to it in the mid-1980s, and I had an insight that many of the patterns that you see in the attachment and interaction of people who have strong emotional relationships to one another resemble patterns that you find among substance abusers. So the intuition was that a model of strong interaction and strong attachment might be discovered in models of addiction.

I began exploring that intuition and began to educate myself. I studied biology as an undergraduate at Johns Hopkins, so it wasn't entirely alien to come back to the field; I ended up writing a book around 1991 called *Strong Interaction* in which the argument was expanded, and it proved to be a very powerful argument that covered a great deal of territory. It went all the way from studying the interaction of infants and their caregivers all the way through studying markets and complex organizations.

I thought the theory was a very powerful approach and although I had explored, to some extent, the neurological side of addictive disorders – in particular, substance abuse and building models of substance abuse – I was uncomfortable with my handling of those materials, and I began to think that I ought to really consider the neuroscience literature.

In 1991, just when this book was about to come out, I was invited by an old friend of mine, Tom Scalea, who was the head of Trauma Medicine and Emergency Medicine at Kings County Hospital in Brooklyn, to consult with him about chronic violence among adolescents. He invited me down and he said: "We're being overwhelmed with violence and adolescent injuries every weekend – Friday and Saturday nights – and the same thing is true all around the country. What I would like you to do is to meet with some of these kids and to think through the problem and, if you can, come up with some recommendations for how we might deal with it." I read everything there was to read about violence, and thought about it for a while, and we wrote some grant proposals. There

were some practical implementations to deal with people who are chronically violent.

But in the course of doing the work on chronic violence, I began to read the literature on biological psychiatry, because it was my belief that chronic violence exhibited a pattern comparable to that exhibited by chronic criminality. Chronic criminality is a term used to describe people who are chronic offenders. It turns out that maybe 85% of all of the crimes committed that are listed in the FBI's Uniform Crime Reports are committed by a very small number of people – 10-15% of all the people who are ever arrested. So, in other words, there's a kind of a power law that describes these things.

My guess was that the same thing was true of chronic violence: that there were a few chronically violent offenders. And as it turns out, I was right. So we wrote up a number of things about that. But I was thinking about this in the context of post-traumatic stress disorder. If ten percent or so of the population were doing all of this violent crime, there had to be some unifying feature that caused it in this group. And although there were a number of correlates, the most significant one, to my mind, was that 90 percent of the people on death row for committing homicides admit to having been molested sexually or abused physically as children, and that probably underestimates the true number.

What I learned in looking at the literature on post-traumatic stress disorder was that a new biological understanding of this had come about in the period between about 1980 and 1985, mostly at Harvard in the trauma lab run by Bessel van der Kolk, and it had to do with endogenous opioids and norepinephrine. But as soon as I read the first phrase in which endogenous opioids appeared, something clicked (*snaps his fingers*). I recalled that I had just written this book about strong interactions, and here there appeared to be a naturally occurring substance in the brain, which was heroin-like and, hence, might explain some of these addictive features to interaction and attachment that I had been writing about.

So I immediately began looking into the literature on endogenous opioid peptides and I discovered that they had only been discovered in 1972 at Johns Hopkins in the labs of Solomon Snyder, who turned out to be somebody I had met when I was an undergraduate there. I had met him also because Dean Green lived in Baltimore for a number of years,

saw Snyder, and went to his synagogue; so I went down there a couple of times and, at one point, ended up meeting Snyder and we exchanged stuff.

I got fascinated by opioids and began to read all the literature on them. Beginning in about 1979-1980, the implications of the discovery of opioids for understanding patterns and behavior began to be spelled out, particularly in research on other species, monkeys and puppies. But some of it pertained to attachment and I looked into this stuff and as soon as I read it, I knew that it was something I needed to get my brain around, which wasn't too hard, since it's fairly straightforward, but the hard part was figuring out what to do with this information.

In biological psychiatry and neuroscience literature, and what is now called social neuroscience, experiments had been done that reported correlations between this and that, opioids, something else, etc. But these guys were just recording correlations and effects. But I figured that to make use of it, to study what I wanted to study, you needed to model it. So I began to think about how to represent what was going on formally.

My first pass at this took place in 1992 or so in one of the things I did on chronic violence, and then I wrote a piece in 1993 or 1994 for *Social Psychology Quarterly*, which is the big social psychology journal in my field about catastrophes and interaction. Catastrophes are nonlinear; they're disjunctive events in nature that can't be modeled in the usual linear kinds of thinking that scientists employ about continuous forms of variation.

I began to think about the toddler – the toddler sort of moving away from his mother's knee and suddenly turning around and coming back and holding on. To me, this seemed to be a clear example of the toddler taking over responsibility for tuning these brain systems, because what happens is that attachment by a mother to her child, if the child is in distress, causes the release of these calming opioids in its brain and Mom also gets a fix. She picks the baby up, she gets opioids as well. But the toddler is moving away from Mom and toward Mom, and moving away is comparable to Mom moving away from the baby, which causes the opioid system to shut down and the arousal system to kick in.

So on one hand, you've got opioids at high levels when Mom is attached and then when Mom separates, you get high levels of activity in the arousal system of noradrenalin and norepinephrine or other hormones and things that are part of the arousal system called the HPA axis. The idea was to model a kind of reciprocal dependence between activity in the arousal system and activity in the opioid system, in terms of the behavioral controls over those two systems.

This mystified me for a long time. How I was going to do this? But I had figured out what the model should look like, and it turned out to have a formal logical structure akin to something in chemistry that is called a hypercycle. Hypercycles are linked autocatalytic sets or cross-catalytic and autocatalytic sets, sets in which a reaction occurs and there is a reaction set in which a feedback mechanism is present that will stimulate the mechanism to continue. So you have two of these, these cycles as they were, and they sometimes get linked together because the byproducts of one or both of these two cycles will play a part in the other reaction set. So then you have two linked cycles

– those are called hypercycles – and they stay linked together because the dynamics in these linked causal arrangements are stronger than the dynamics of the two separated reaction sets.

So the argument is that you get strong molecular interaction that produces a more complex structure, and the strong forces keep that structure in place. This is actually an argument for how life emerged – an argument about how simpler chemical reactions combine to produce more complex chemical reactions, and you get more complex and highly organized systems working on the basis of simpler ones.

The argument I began to make was that something comparable to a hypercycle was at work in interaction, and that it entailed a cyclic reaction set involving brain opioids, attachment behavior, and withdrawal symptoms that arise in relation to fluctuations in opioids, and another cyclic system that arises in relation to the arousal system.

Separation is the typical form of behavior in the infant-caregiver system. You get separation stimulating the arousal system, and then you also have a form of withdrawal in that system that has to do with downward fluctuations in the level of norepinephrine. So you get two forms of anxiety – two forms of distress. The opioid system distress surfaces in babies because babies communicate their distress – they cry. When they cry, Mom picks them up, and this causes opioid to be released, which leads to withdrawal symptoms being read. Then Mom can relax; she goes away, opioid system shuts down and the adrenaline level kicks up. So you have a link-causal arrangement.

I knew this was the formal structure of the system, but I didn't quite know how to model it until one day when I was sitting in my office with a couple of students and we had been brainstorming on this. I said: "Let's put up some reasons on the board that seem to describe what we are talking about here." I wasn't particularly happy, and then I saw something that one of the kids (Greg Stevens) was putting up, and I said: "Whoa, stop right there... that's a population question."

As soon as I said that, I realized that the logic of population biology could be used to describe the things that we were talking about. All we had to do was consider the brain chemicals as populations, so that you had populations of opioids and populations of norepinephrine, and those populations would fluctuate. They would fluctuate under conditions that are governed by population biology models. Essentially, we are talking about the opioids competing for attachment behavior in this sort of behavioral niche, and the arousal system competing with the attention behavior for separation behavior.

That was the deep insight, a moment of genius, (*laughs*) – I thought so, anyway. As soon as I saw that, we had a mathematical apparatus that had been well developed which we could use to model these reactions. So, we began to do that. We decided that we would use computational methods to study the mechanism. We would get computational data, and the data would then be interpreted.

The problem would then be to make sense of the data. We can do simulations and computations until the end of time, but you have to do them intelligently. What we decided to do was design a number of experiments that would allow us to see how distress fluctuated in a system of interaction under different conditions that we could specify. One of the main parameters in the population biology model that we were using



is a parameter that we likened to intellectual and cognitive development. Since we can manipulate this parameter in these models, it's as if we were growing a child. And as we grew the child by changing the value of this parameter then running these computations, we would see vastly different dynamic patterns emerge.

It became clear that there were only some conditions under which interaction would stabilize; they basically had to do with reciprocity, but also with a number of other variables. So we did that piece, which I think was a fundamental rethinking of the category of altruism and reciprocity from a biological perspective.

The second thing we did was to move beyond looking at two people and to try to model what happens in social networks. I wrote a piece with Greg Stevens called "The Architecture of Small Networks," which has turned out to be a seminal piece in the new literature on networks; everybody likes it. They began to see for the first time that you can compute the properties of social networks bottom-up on the basis of generative models that are rooted in the neuroscience of the species – what I call the neurosociology of the species. The people who study social networks in my discipline have taken heed of this kind of work, but there have been many developments in network theory in the last ten years, and they have a life quite apart from my little project, brain and interaction.

That's where my work is now; I have done what I think I need to do with the old version of hyperstructures, which we now call "Ye olde hyperstructures" (*laughs*), and have moved on to new models of how this all works by deepening the thinking to more fundamental levels, and bringing in a lot of other hormones and neurotransmitters that we had – well, we hadn't ignored them, we'd worked on testosterone and dominance in the early stuff; that was very, very interesting, where we simulated dominance arrangements in the family system in small networks. But now we are talking about new models of networks that are likened to synapses, synaptic models. What we have concluded is that the hyperstructure in the old version, as modified to take into account some other chemical players, is actually a mechanism that forms synapses socially, which is an interesting idea.

So we have moved beyond the old stuff and what we really want to do is to study large-scale networks with these synaptic kinds of models. They're much more complicated, and they're going to entail some formidable new mathematics and computation, but that's where we think we need to go. And the work has caught on; there now are people who like this stuff on the brain and behavior everywhere. I got invited last year to give a plenary address at some national meetings, and this year I've been invited as a guest of the International Sociological Association to go to Rome in July to deliver an address to all the people who will be there, which will be shocking to them. Europeans don't know about this sort of thing.

jur: Your research spans all these different fields of chemistry, biology, neuroscience, sociology. Have you encountered any difficulties in trying to combine these different fields into your research?

Smith: Well, the danger is of simplifying work that has been done in other areas, and, of course, what we do with the

neuroscience side of it is to vastly oversimplify the underlying complexity of the brain. But we try to do so without distorting the underlying processes. I think the original versions of hyperstructures were gross oversimplifications. But I still think that they are right; it's just that all the things that needed to be drawn into those models weren't there. And, in part, because we began thinking about those things in 1990 or so, and in the period between 1991 and 2004 – opioids, although they are still extraordinarily important, and endorphins and whatnot – although they are still a major field of study, they have taken a side seat next to some other main players, and the understanding of pair bonding and attachment behavior in other species, and probably in the human species as well, mainly oxytocin and vasopressin, which are two related hormones – men have a lot of vasopressin and women have a lot of oxytocin. So, we began rethinking everything with the idea that opioids and the oxytocinergic system and vasopressin all work together as redundant mechanisms, and there's some sense in which that's true.

The real danger is when somebody at the macroscopic level, which is what I'm working at, begins to try to understand physiological considerations and how social behavior is rooted. The real danger is simplifying the physiology so much as to make it unrecognizable. We've tried not to do that. And the constraint on me, of course, is that I have to write stuff that people who have no training in neuroscience can read; so there's obviously some simplification that goes on, even though we try at the same time to develop the model to the level of complexity that would be satisfying to the neuroscience people who read them or the physiology people.

I remember one of the first papers that I submitted to a sociology journal – they kept that piece for two years, before they finally decided to reject it. And they had all these neuroscientists reading it. Some of them were very, very supportive, and one thought it was so promising, as a matter of fact, that he said he was the editor of the physiology journal and he said "I think you should submit this to the physiology journal." It's interesting! It's a theoretical model, but if you get it into a biology journal somewhere, you will perhaps attract interest on the part of the neuroscience people to do some experiments with this kind of thing.

Anyway, that journal that rejected this piece was the *American Journal of Sociology*, which is published at the University of Chicago, my alma mater, and I sent it to the *American Sociological Review* instead, which is actually the leading journal in the field, and they published it. So yeah, the danger is simplification, and also so overstressing yourself that, you know, you can become an amateur in everything.

But I'm fascinated by the theory, and the stuff that we do has taken on a life of its own. Every empirical subject to which I have turned these models has been opened up by this kind of fresh thinking, so that it has led to new understandings of these things. So it's an inducement to continue to work with these models; it's exciting, new horizons appear all the time. You're drawn on by the excitement of exploring the model.

jur: Where do you see the field moving to?

Smith: Well, there is going to be resistance on the part of social scientists to any forms of reductionism. It persists in sociology,

especially, and I'm certain in economics and political science as well; less so perhaps in psychology. But it persists in sociology because one of the turf-wars from which sociology emerged as an academic discipline was fought by Emile Durkheim in the French University system at the turn of the century. And Durkheim, in order to establish the legitimacy of sociology as an independent academic discipline, began inquiries into a number of subjects, like suicide, which he felt and claimed had a kind of ontological status, that he could describe in terms of what he called social fact, by which he meant that there were forms of variation in these things that could not be reduced to subsidiary levels of analysis, and that could only be accounted for by other social facts, other things that occur at the level of social organization.

So, sociology has proceeded since Durkheim with the doctrine that much of the subject matter is irreducible. And I have attacked Durkheim's writing, making me a heretic by claiming that putting his imprimatur on things does not expand our understanding of things but limits it; and, so, that's heresy among sociologists (*laughs*).

But I began to win people over to my point of view, and as I started to say a minute ago, I love my field, I love sociology, I just love the history of it, and I love what sociologists study; I just think it's absolutely fascinating, I love the people in it, I love the intellectual puzzles. It's not simple, it's very complicated stuff, and the field has been dominated by empirical work by people trying to measure things very carefully and to establish reliable measurements of this, that, and the other thing, and establishing covariation and causal relationships and all that sort of things, and that's great science, and I love that.

But I think the field is going to die; it's going to be eclipsed by work undertaken in adjacent fields unless it takes a biological turn. So I feel that part of the motive to my work is that there's this sort of moral urgency to reform my field, to get people in my field to think more biologically... and I'm getting an audience. I also get hate-mail (*laughs*).

jur: So there's been some resistance, then?

Smith: There's been resistance, yeah. A lot of what I publish in professional journals though is too complex for the typical non-quantitative sociologists to understand. So, I have tried to write a number of simple arguments, versions of the arguments which I put out in various places. Those are widely read now; the technical stuff has been appropriated in a number of areas. The work on networks is taught in network courses at the leading places, you know, Stanford, Chicago, and Harvard.

Resistance is everywhere, resistance to reductionism. See, I'm a strong believer in the view that any scientist has to be both a reductionist and an emergentist [sic]. You have got to seek to find explanations for whatever it is you are studying at more fundamental levels of analysis, but insofar as those are unavailable, insofar for example as more complex forms emerged that are irreducible, then you need to be an emergentist and acknowledge the fact that there are reasonable and separate subjects that constitute a different level of analysis. But it makes a lot of people uncomfortable to hear that work that they have undertaken without any attention to what's going on in neuroscience might partly be understood in terms of implications of activity in various brain systems for

understanding interaction and behavior.

I say if anything social is mediated by interaction, and thinking – interaction and communication – if anything social is mediated by that, that means every subject in the social world has got to heed the forces that are at work in social interaction. From my point of view, the core dynamics of social interaction are driven by physiological forces and they are the things we can see in the most elementary forms of interaction, what I call the minimal social system, which is the mother and the infant, the newborn.

jur: Is there any way for undergraduates to get involved in sociology research?

Smith: Of course. I would say there are many opportunities to undertake research of the sociological kind. There are sociologists here and in the medical school; not too many, but there are some. And there are sociologists on campuses around here; some of them work with me. So all kinds of subjects are open.

I have undergrads who are working with me on all kinds of things right now. Several of them own computational work, those who have learned to program. I've trained and sent off to graduate school a handful of students over the last 5 or 6 years. My former students are at Cornell, Michigan, UCLA, Chicago, and Harvard these days. And they carry the message (*laughs*). They've gone off to the very good schools. I have a former student who is an associate professor at Geneseo, who is doing a post-doc with me starting next year; she plans to try to measure some of these things by brain scanning.

But for students who want to work with me, I encourage independent research; but if they want to get involved in a research project, they have to know how to program. If they know how to program, we'll set them up.

jur: What advice could you give to undergraduates that are thinking of pursuing a career in research?

Smith: Learn as much math as you can and as much programming and statistics as you can as an undergraduate, because you need to lay down basic disciplines before you start in graduate school. My philosophy of education is that when you get out of this you need to have learned how to read, write, and count better than you could when you came in. The counting part turns out to be very, very important if you're going to go on to do scientific research.

The thing is, research has changed a lot; nature now is being tackled in its full complexity – well, maybe not its full complexity, but with an eye on the real complex organization of natural systems, instead of as in the past, when scientists had limited computational resources available to them and they would simplify things. You would try to work in little packages that were parts of wholes, and you tried to figure out what was going on in those parts with linear equations. Of course, nothing is really linear, so what you need to learn is nonlinear differential equations (*laughs*) and some statistics, and if you can program, computational skills always come in handy.