

Oral History Interview with Eugene Rosa

Following is an interview with Eugene Rosa (ER), Professor of Sociology at Washington State University. The interview took place in his home in Moscow, Idaho on June 21, 2012. Scott Frickel (SF) conducted the interview. The interview was tape recorded and transcribed in full by a professional transcription service. SF and ER did light editing to smooth out pauses and enhance readability. No substantive changes were made to the text.

SF So Gene, thanks for agreeing to do this. This will be an important contribution to the oral history archive that we're building for the ASA Environment and Technology Section.

ER The pleasure's mine.

SF Great, I guess I'd like to start out by covering some of your sort of personal background and then the broader institutional history of environmental sociology. So to begin, could you tell me a little bit about your educational background, your training and eventually I'd like you to hear you talk a little bit about your decision to become an environmental sociologist and whether or not that was a conscious decision on your part of something you fell into or how that came about.

ER Okay, my undergraduate institution is Rochester Institute of Technology and the principal reason I went there is they had a co-op program meaning it was a quarter system and where beginning your junior year it alternated between working a quarter and going to school a quarter. This was an important opportunity because I was putting myself through college with very little help from my parents who were poor and I also could live at home. That's where I went and got my undergraduate degree and I majored in something practical because I assumed I needed to have steady work on a regular basis. Basically I got it in accounting and actually in my off-classes quarters worked in a certified public accounting firm as a public accountant. I also minored in economics. Despite my chosen major I had immediately, within the first or second year, an affinity or a particular proclivity to get most of my enjoyment out of liberal arts courses rather than the business courses that I was required to take. So my thinking began to ferment along the lines of how do I now pursue something that I really feel I love which is the liberal arts despite wanting a practical skill as well. It was then that the idea of graduate school came up, as I remember it. But at the time I was in the Navy Reserve because I had been drafted to Vietnam and both the National Guard and the Army Reserve quotas were filled for months and months. So my chances were very good of getting drafted and getting sent to Vietnam, which I didn't want to do. So one option was to join the Navy Reserve who, as long as one maintained an acceptable grade point average, would defer active duty. So when I graduated from RIT, I had to go and do two years of active duty in the Navy. I was on a ship called the USS Rushmore (an LSD) which stands for landing ship dock, not (inaudible). When I completed my duty and returned, I had a number of school loans facing me because as I said earlier, I put myself through college; so I went and worked for a public accounting firm and got all my

loans paid off. For graduate school I only applied to places that had interdisciplinary programs, such as Michigan State. I applied to a program at Suny Binghamton and I did hear back from them with a solicitation to enroll in a new program. They had just started a new program from a gaggle of young scholars, mostly from Berkley, around political economy and they looked on my records which they found impressive and fitting with their program. They said you'd be better in our political economy program, so I actually applied there in political economy. Then there was Syracuse which had a very strong interdisciplinary program in the Maxwell School which had a very good reputation. On top of that, I got restricted inadvertently to universities in the state of New York because I won a state wide competition called the Herbert Lehman Fellowship and the only stipulation was one had to do graduate work in one of the New York universities. So I chose Syracuse as my place to go, which it turns out for many reasons was a very good choice for me.

SF Can I just ask you quickly then where did your feelings about interdisciplinary scholarship come in? And when you say interdisciplinary, what exactly are you talking about?

ER Well there's a... I guess it's a combination of rationality and irrationality which is maybe the way we do a lot of things. The irrational part was I wasn't exactly sure what to major in, so this was sort of a way to sort of wend my way around that problem. The other was that it was my experience from the variety of social science courses I took as an undergraduate, an experience deepened even further in graduate school is, if you went into a new area, a new subject area, for the first several weeks you're totally baffled because you're in a different linguistic paradigm and so the terms are not familiar, etc., but once you get beyond that barrier, it's pretty easy to recognize that there's a lot of similarity in what people are saying. It's just that we put up these silos and barriers and other things that in many ways are artificial, useful institutionally, but artificial and I thought there's no reason for that. Another rational reason I was attracted to interdisciplinarity, was that my first major in going to graduate school was international development and I was very clearly convinced the economists did not have all the answers about international development. So I said there needs to be a larger context in which we try and understand how to get people who are poor or are otherwise disadvantaged, how to get them help themselves, how to help them up by their bootstraps and I definitely thought we needed some sociology and anthropology. So my triple studies were primarily economic sociology and anthropology and for the interdisciplinary program at Syracuse, it required a lot of course work. One had to take 75 hours of course work and received 15 hours for dissertation credit. So you got a pretty solid background in those three areas or whatever three areas you chose. Then what happened at Syracuse the internationally recognized programs in international development that I applied for—both in Latin America and in West Africa—were being disbanded. So one of the real assets I'd gone to Syracuse for now was disappearing. At the same time, the smartest person I ran into in terms of professors was Alan Mazur who had just arrived from Stanford and just was head and shoulders above everybody else in terms of cleverness and creativity and so

forth. He happened to be in sociology, so I revised my distribution so that I did whatever my 30 or 40 hours in sociology and the rest in anthropology and in economics. Indeed, I actually never “TAed” in sociology, but in anthropology teaching cultural anthropology. I sort of slid into, by accident, an interest in the environment at that point in two ways. One is as part of our methods course which was taught by Alan Mazur to all the social science folks doing interdisciplinary work. I don’t think it possible to make the assignment he did nowadays. One assignment was to conduct a survey that Mazur actually used in a study that he then published. It was about technology attitudes, especially nuclear energy, and so I began to get some acquaintance with that technology and in energy in general. He introduced me about to other models of how to go through graduate school and one was to really identify with a mentor and go through it under a mentorship rather than just being one of a number of students. So basically I apprenticed myself to him. I had my own money, so he didn’t have to support me because I had the Lehman Fellowship. Then when that funding ran out, I had such good standing, I was awarded “TA-ships” that and assigned to teach introductory anthropology. During my mentorship with Mazur I did a lot of independent work and we published something like five papers while I was still a graduate student.

SF And what was the nature of that work?

ER Well it took place around the first oil crisis in 1973 and ’74 and there was a scholar at the University of Pennsylvania, (Sam Klausner) who headed an institute there called The Center for the Research on Man, something really grandiose like that and he organized a session of social sciences at the February 1974 AAAS meetings, American Association for Advancement of Science around the topic of energy and invited Mazur to present. In turn, he invited me to be the co-author since I had done all the data analyses on the ideas we would present. Because the oil embargo didn’t end until March and the AAAS meetings were in February, the topic of energy was full blown and particularly timely. There are obviously people of a certain cohort who remember the embargo. The gas lines were so horrendous at the gas stations, a couple of things happened. One is California switched to an odd/even day system; if your license plate ended in an odd number, you went one day while even day numbers went the next day.. There were some shootings at some gas stations and then some clever entrepreneurs, like wealthy executives, would hire temporary workers like from Man Power and Kelly Girl to sit in the car and wait in the gas lines. Anyway, the embargo was very much in everyone’s mind. The topic of our paper was Alan’s idea and the question was this: all right, so the uniformly conventional wisdom, virtually unchallenged, was that the U.S. (and other nations) must continue to increase supply. Mazur suggested we test the exact opposite position, namely that we should shift our attention to the demand side and decrease demand. After all, it recognized two sides of the same coin, more or less. So he got me to be interested in that. We got accepted on that program and so I spent Christmas break in the library in those days, handwriting in (code sheets) the UN data from Series J I think it is, that had international energy statistics, as well as a whole variety of quality of life statistics: life expectancy, levels of education, a whole

variety of things. We just asked the straightforward question, what would happen if you reduced energy demand? What kind of social consequences would happen. The startling answer was not really much at all; nations with much lower per capita consumption of energy enjoyed quality of life comparable to the U.S. Only later did people like Amory Lovins and Lee Schipper and a number of other folks from the technical side begin to see demand reduction as a realistic possibility. This alternative strategy then became part of the conversation in terms of a national energy policy.

SF Where did you publish those early papers?

ER That one was published in Science, 1974. It pretty much stands up today. First of all, if you go back to it, it kind of is amusing at how primitive the methods we used. Basically, you know, we only looked at the data with correlation coefficients which today would not fly at all. But even with more sophisticated, controlled analyses, you find out the basic message is the same. Now if you add in things like more recent work with Kyle Knight, who's just finishing his degree, on subjective wellbeing and happiness, you find the results even stronger in the sense that not only doesn't the strong connection between energy consumption and well-being show up with a so-called objective indicators, it doesn't show it up in peoples' subjective experience either. People find other things to be more important: social capital, equitable distribution of wealth, and a variety of other social things. In view of these results the consumption of energy and some of other resource uses are in a clear sense just wasteful.

SF And so was this early work on nuclear energy, was this your dissertation or was this a side project?

ER No, I did my dissertation on bio-sociology, which is I was looking at... let me put the point in a context here. My mentor, Alan Mazur, always had a macro project and a micro project going and that comes from one of his mentors which was Art Stinchcombe who advised him as a graduate student that it's a good idea to have a macro project and micro project going on because if you get bored with one you can always go over to the other one. So that was Alan's standard approach. Hence, one aspect of his micro work was on integrating the primate evidence with human behavior. The other was looking at physiological measures of mood and so forth, and so my dissertation was about whether some of the subtle signaling systems of primates, especially chimpanzees, applied to humans and the evidence seemed to show that it does.

SF So was this experimental work?

ER Yeah. Basically I wanted to see if eye behavior would predict a person's status in small groups and was able to do so with some moderate degree. This tied into how the higher order primates, especially chimpanzees, established and maintained status. I did not experience much interest with my dissertation work on the job market. And the job market was especially horrendous right then, even worse than it's been in the last couple of years. So out of desperation I

applied for an NSF postdoctoral fellowship in energy studies and was successful. It is likely that I had an advantage because almost nobody had published in that area. But I had this paper in science and the experience of the survey that had been done nuclear power. I probably was a good outlier for them, so I got a fellowship with the National Science Foundation and I was able to go to Stanford because my mentor had worked there for a couple of years earlier. He didn't stay ensconced in the sociology department. but interacted with the engineering department. In fact, I think he jointly taught a course with one of the engineers. So we activated that contact as a sponsor which was Tom Connolly, who said, fine, bring him out. So I went out there and I did both those things again. Micro, I was looking at human brainwaves and seeing if they would predict peoples' behavior in small groups. At the same time, I was a member of a team that assessed and created a national model based on Amory Lovins work that came out about 1978. He developed a scenario that said we could get along with about half the energy we were using and see no real change in our social patterns, our quality of life and so forth. And so we had at Stanford a unique take on this that no one else that I know of pursued. A standard tool for projecting the future in those days was scenarios. We created our scenarios beginning with social scenarios based upon what we thought would be prevailing values. Now in many ways what we did looks crude and primitive in retrospect, but it was absolutely breakthrough thinking and if the folks at Stanford were clever, they would have marketed it in a much better way, because shortly thereafter Harvard produced a book called *Energy Future* that was a runaway bestseller. It did a review of what things were and where the leaky parts were and which parts would be plugged and so forth. Anyway, so at that point I was an energy guy basically, not knowing that there was such a thing as environmental sociology. I just was doing this kind of research because it seemed like interesting problems to me.

- SF Well going back to Syracuse and then moving to Stanford and at either of those places, I mean other than Alan Mazur, were there any other people in those academic contexts who were doing what you would today call environmental sociology?
- ER Nobody that I knew, the first one I met I think like that was Riley Dunlap at a meeting of ASA in about '76 or '77, something like that. Jim Short had introduced me to Riley, but up to that point I had no idea there was such a specialty in sociology.
- SF So you met those two when you were at Stanford?
- ER I met Jim Short while I was at Stanford because he was an advisor to a big project run out of SRI International which used to be Stanford Research Institute. But because of the protests in the '60s, all these research wings of the universities had to separate because a lot of their funding was coming from the military or sources that were politically incorrect. Anyway there was a big project SRI that had Nancy Tuma and Mike (Hannah) very much involved and their student was Peggy (Thouts) who was my partner at the time and so Jim Short

was an advisor to that. So he'd come to Stanford several times and so I met him there and then, when he learned of my interests, he told me about Riley.

SF Do you recall... I mean that was a long time ago, do you recall the nature of those discussions, those very early discussions with Jim or Riley?

ER Well Jim really had little to say about environmental soc or environment itself. His main connection was to say, oh, I should talk to Riley Dunlap and so at the next ASA meetings, whenever that was, '76, '77, I don't think in '78 we met. I was aware of an opening at WSU and had actually applied, along with my partner Peggy Thoughts in '78, but openly admitted to Riley at that meeting that I hadn't even heard of Washington State University before.

SF Where, I'm sorry?

ER Yeah, right, so..., you know, I was asking him questions about where's WSU located, what's it like around there, what's there to do, what are the amenities and so forth—those kinds of questions. Again, it was a very, very tight market and we were both lucky and delighted that both my partner and I got jobs together at WSU. It is important to keep in mind that this was before spousal accommodations.

SF And that was in 1978?

ER '78.

SF And so prior to that you had talked to Riley and '78, if I recall, is the same year that the their first paper was published.

ER It was right around there, yes, right.

SF So do you have... I mean, again, do you recall any kind of discussions with Riley or anybody else at WSU that... I guess I'm fishing for and am interested in hearing you talk about sort of the nucleus of this field and your perceptions of how that came about.

ER First of all, Riley, as he still is, was very enthusiastic about the field and extremely encouraging to me to do my work there rather than the other things I was doing. The other was within a matter of a year or so we had hired Bill Freudenburg. We also hired Marv Olsen who was a fairly well respected political sociologist who shifted his interests to the environment. And there was already a demographer in the department who sort of was interested in the environment, although often a counterpoint to the idea of limits. So all of a sudden, out of nowhere, we had five or six people around an area that almost nobody else was pursuing. This was key to my staying at WSU. I thought, well here's an opportunity to be at the foundation of the building of an institution and we've got a critical mass to do it with. Thus even though experiencing some pretty thin times in a professional way, I was motivated to stay and contribute to the emergence of

this field and the perpetuation of the institution. I'm glad I did so then and we've been fairly successful in maintaining a certain core of people to continue the tradition.. A little later on, Lee Freese who became chair, read Bill Catton's book as a matter of being the chair's duty and had a total epiphany and started working entirely on the environment—something that he had never done before. Lee tried to do theoretical work and formalize some of the things that were being said in sort of a casual ordinary language way. We, therefore, had this sort of continuous revival of interest in the area and we were also beginning to have some success in terms of publications and places to publish and so forth. As the field spread a bit in the larger institutional kind of way.

SF So let me take this line of question in two slightly different directions. You've repeatedly referred to the field, circa 1976, '77. To what extent was there a field of environmental sociology at that time rather than a handful of people who were interested in this project?

ER I would say, your latter description is more accurate of what was really there. There were people like somebody you've probably never heard of, (Charlie Wolff), who was doing social impact assessment which was a big interest at the time. At the same time Charlie was an energetic promoter of environmental work.

SF Was he at WSU?

ER No, he was located in New York City, maybe even as an independent consultant. Then when Freudenberg got his Ph.D. that's what he studied at towns in Colorado for his dissertation. So there were these, for the most part, isolated individuals doing this work. So in that sense there was no field in a conscious way as later it would emerge.

SF And so going back to the sort of departmental context at WSU in the mid to late '70s, were the four or five of you who found yourself employed there, consciously... I mean to what extent did you all consciously and collectively go about laying down in a strategic way the institutional foundations for this field or... yeah, I'll just ask you that.

ER Well we did a number of activities that would point in that direction. So, for example, Bill Freudenberg and I organized a regular informal colloquium where we went around to different either faculty or student houses and basically did what we're doing now in 2012 in our conference room. [this refers to the new EARTHs colloquium initiated by Gene in 2011-SF]. We did it much less formally and supportive, typically having refreshments, etc. In that informal environment we were able to encourage students to make presentations that they might not otherwise make because of the colloquium culture.. As a cadre we took other actions toward institutionalization. There was concerted action to get sessions at key meetings. Again, we didn't sit down and say, we have to do this to get an institution going, it just seemed like a number of things seemed like the right thing to do. If we were actually laying out a strategy we'd do them anyway, but I don't

think we ever sat down and said, we're about to build an institution and here are the steps we will follow.

SF And so another piece of repertoire tactics that you're describing is a directory that is cited in those early papers by Canton and Dunlap that somebody at WSU put together, that directory of people around the country, sociologists I think primarily, who WSU folks saw as doing relevant, environmental sociological work. Do you know anything about that directory and who put it together?

ER My best guess is it was Kent Van Liere who was Riley's first and very successful student in the private sector. He started out... where did he start out, Tennessee I think, at University of Tennessee where he sort of laid the ground work for them to have a fairly robust program. Ken then he got hooked up with Tom Heberlein at Wisconsin. Tom had a research shop doing survey work and they built that shop up to where it was very profitable and quite a bit of money was made. I think, but I don't know where he went after that, but I think it was probably somewhere else in the private sector. As for the directory, I can't say who all the final authors were (my guess is Riley's name was on it somewhere) but my belief is that probably a lot of the leg work was done by Kent.

SF The other local institutional question I have about WSU in those early years was the reactions that you all got from other departments and from administration? Was there general support when you went outside of the department or did you go outside of the department and what kind of reactions did you get?

ER Well let's start in the department. There was remarkable support given... we were taking a risk, with one or two exceptions. For the most part there was considerable amount of support for us to do whatever it was we were doing, just do it well and attract recognition to the department and university was the bottom line. So we took that as something of a license to just go forward and try to do the best we could in terms of doing good work and making it visible. At that point in time, the resources here were remarkably greater and there was virtually zero emphasis on us bringing in external funding, so we could do just about whatever we wanted to in terms of intellectual pursuits, in terms of how we went about our business and so forth. In many ways, I'm a little bit nostalgic about those times. So, as you know, for almost every job applicant that I know of now, one of the elements in the hiring process it's whether they're gonna (rain makers or not.) Even small colleges are asking about that, but we didn't have that constraint. Administration, I would say either benign neglect or certainly not any push back and then it turns out we got a dean from political science who... John (Pearce) who had done some work on the environment through political science and was extremely supportive and he was a dean for a very long time. I think twice as long as any other deans in the conference, what was then the pack 10, now the pack 12. I think he was dean for 15 or so years or something like that and he was very supportive and he's the one that got the Boeing Professorship for the department, as well as the Myer Professorships that are distributed. One important aspect of the Myer professorship is that a focus on the environment was one of the eligible fields, another important institutional push from the dean's

level. At the upper administration, sort of the same, I heard the same message basically; we're not particularly concerned about what you do, just do it well and bring good visibility to the university.

SF Other departments, was there an effort to reach out beyond sociology in those early years?

ER Not that I recall and certainly not to the extent I would like to have seen it. I had some involvement with the then Office of Applied Energy Studies who had a colloquium series that I took part in and eventually collaborated with on some projects. I may be missing something, but... because this is going back quite a ways, but I don't think there was in general much outreach in the sense that I was engaged in. For one thing, the department had a certain amount of, for lack of a better term, arrogance at the time. We were definitely one of the best departments in the university and some cases probably *the* best department. And then as time went on we lost a lot of resources so it was harder to maintain that and secondly, the university was clever in promoting other departments that really lifted themselves up in quality by long strides. So as a result, I don't think we looked far beyond our own borders for those kinds of things in those days that we sort of take for granted now.

SF Right, that's interesting. Why don't we shift gears now away from WSU, per se, and think about the larger field a little bit and the broader scope of environmental sociology's history and I'd just like you to take me through some of the key... what you see as some of the key turning points in the development of the field and, you know, maybe along the way comment on how those debates or new insights or innovations shaped your own thinking and your own sort of research trajectory?

ER Okay, do you have a...?

SF It's a prelim question.

ER Do you have a (stem) question I could respond to?

SF Well what do you see as the fundamental innovations or... publications or projects that have really moved the field forward in the last 20 or 30 years?

ER Well that's a tough question.

SF You know what, let's come back to that actually. There's something else that I've got here in my notes that I scribbled down that I think I want touch on because it has to do with you and this question of interdisciplinarity which came up immediately in our conversation and really hasn't gone away, which is that I mean when I look at your work and the work that you've produced, it is strikingly interdisciplinary. I don't know... I haven't done a count, but my guess would be you have as many publications in non-sociology journals as you do in sociology journals, possibly, maybe more?

ER They probably would be close.

SF Yeah, so maybe you could say a little bit about your own experience working in, you know, in the field of energy studies, in risk science, risk analysis and what it's like to work with not just other social scientists, but natural scientists as well and how you came to get involved with those other communities and how that's shaped your thinking?

ER Well in terms of working with or interacting with people outside your field, it forces you to be a student again and so to me that's an exciting proposition because it means you're gonna learn something and learn it beyond what would be the normal boundaries of whatever field you're working in. And also sometimes it means serious counterpoints of your thinking which is sort of a mental hygiene; it's mental hygiene because we easily go down the path that we think we really have, I wouldn't say truth, but we really got a good purchase on whatever it is we're thinking about. Counterpoints are useful ways to have that purchase bump up against other realities that may be just as viable and just as reasonable. So I think part of our job as scholars and researchers is to be learning at all times and there's a lot of ways of doing that. I just happened to find that interdisciplinary context is one, a very rewarding way of doing it and two, one that almost forces you to do it. It's pretty hard to be a team playing in a context like that without doing your homework and knowing some of the basics of the other field and so I would say that's the core element of it.

SF Did you find in those experiences and I don't really have... I don't have a good sense, so you might want to talk a little bit about your work in those interdisciplinary contexts, in your experience though, how have scientists treated your work and your contributions?

ER Well, I would have to say that in a variety of the contexts that I've found myself in, for example, I worked on the safety of nuclear power plants in terms of control rooms with a group of physicists and engineers and was generally accepted because they recognized the need for the kind of knowledge I was gonna bring to the situation. The same is true now in my work with biologists at Stanford who feel the same way. There's sort of a natural affinity to be in context where if you're invited then typically your work will be thought of in a positive way and you're gonna be supported. The other place where it shows up is in who cites your work and so there are people in the other sciences who I have never met and probably never will, who apparently find the work, in itself, useful. I've also had this experience which I thought was quite interesting and definitely gratifying. I wrote an article on nuclear power that included the notion of a second form of transmutation. In terms of normal fission processes in a commercial nuclear reactor you split an atom in two and by doing that you release a tremendous amount of energy. That energy is basically used to heat water and drive turbines. One of the major problems which we still haven't solved is the waste produced in the process. These are transmuted products, entirely different chemical elements. The waste issue is a big problem because it includes a lot of

plutonium which is bomb-grade material. Well there was a proposal that came out of the Reagan Star Wars program where a technology was developed where the waste could be subjected to second (order) of transmutation, I think to blast it with more neutrons or something. That would presumably render the waste fairly innocuous and therefore you'd have a much easier way of sequestering it than you do now which is still challenging us, I wrote about this in the article mentioned above. I had a scientist from Los Alamos National Laboratory call me to ask me some questions about the process and somehow it got around to, what's your (meaning my) position? I said, I'm a professor of sociology at Washington State University. He said, I thought for sure you were a physicist. That was one of the most gratifying days of my life, that I could translate something quite scientific and write it in a way that another scientist thought it was written by a technical scientist. So I guess there's a knack to how you absorb this information and put it out there.

SF That's a great story. So we've talked a bit about your work in energy, but you're also known quite well for your work in human ecology. How did you get hooked into human ecology and that work and maybe you could tell me a little bit about the development of STIRPAT and that whole side of your research program?

ER Yeah, in about... I may have the dates a little bit off, but they'll be roughly correct. In about 1992 or thereabouts, the National Academy of Sciences discovered that the real problem with many of our environmental issues were, just like many technical problems more generally, due to humans. Yet they had no committees, at the academy that really was dealing with this issue. So I think it was originally an NSF initiated committee that was created called the Human Dimensions of Global Change and Paul Stern was the staff director of that committee and continued to be for its entire existence. The key sociologist on the committee was my good friend Tom Dietz. It also comprised a group of political scientists like Oran Young, geographers and economists and they always had at least one physical scientist to sort of keep the social scientists honest. Anyway it produced a report I think in 1994 that's referred to as the Rainbow Book which is called the... I think The Human Dimensions of Global Change or something like that. It tried to piece together the little evidence there was of people and the environment, to piece together the available evidence of which there wasn't a great deal around issues of humans and the environment interaction. One of the ways they organized their thinking was to take from Paul Ehrlich, John Holdren, and Barry Commoner a little accounting formula that states: impacts to the environment are a multiplicative function of population, affluence and technology. Well immediately if you look at the equation itself it is sort of tautological in a lot of ways which doesn't seem to bother natural scientists. We, I think in the social sciences, gets much better training in the philosophy of science and are much more cautious about using tautologies as readily, which I find interesting in some way. So Tom Dietz from his experience on the first committee was thinking about this quite a bit and the main thing he couldn't figure out on his own was how to measure the technology variable. So we were at AAAS meetings in Chicago, I don't remember what year, it might have been, really... it was early '90s and... seldom would we find ourselves in a saloon together, but we were so

happened to have been in a saloon together and we started writing the equation out on a napkin and asking ourselves the question, how can we measure technology that's not tautologic because the way it typically was done. That way was to take a ratio of energy to GDP which, if you pieced all this apart basically, it results in circular reasoning. So he said, wait a minute, if we convert this into a stochastic form, we can put it in the error term and therefore we got not only technology but also culture in the equation. No one else was doing this. The main point is that we can actually test the model in a rigorous way. So it was sort of a combination of his insight and my encouragement and our collective understanding of ecological first principles—not to mention the fact that we probably had had three or four beers by that point that accounts for STIRPAT. On those same napkins we wrote the stuff down and that got us started. But, before testing the model the first thing we needed to do was to articulate what it is we wanted to do, what had been done before, what was wrong with it and how our model, later named STIRPAT, might be a way of moving forward and getting a deeper understanding of human ecology at a structural level. So we wrote this article for Human Ecology Review in 1994, that basically starts with a question, how can we better understand the interactions between humans and the environment and then we laid out some of the approaches that had been tried including Otis Dudley Duncan's POET model and several other things. Then said here's another option and that's where we laid out what the estimation procedure would be and how you would interpret the results. At that point we had a not done a lick of empirical work. Rather it was all a matter of conceptualizing what we would do and then it was a matter thereafter of asking where is the applicability of the model appropriate? From the beginning we have tried to avoid Mark Twain's little boy with a hammer where the world starts looking like tacks. Instead we try to find data and context that seemed to be amenable to using that model without forcing variables into the model that didn't belong and so sequentially go along and say, okay, what if we add this or what if we do that. What if we considered this specification or what if we considered this different dependent variable what happens? What if it's the ecological footprint versus CO2 loads, etc., Inherently it built up a research program that focused on the drivers of impacts. That was the organizing question; what human practices at a structural level are causing most of the impacts on the environment. it's a pretty straightforward question. When we played out the impact part of the question we then switched directions which was to ask a complementary, but different question. We then anticipated a neoliberal response, which is, yeah, but you only gave us half the equation. People are living a lot longer, people are enjoying life. There's all kinds of reason to believe this cost to the environment is worth it in terms of life satisfaction, happiness and so forth. So now we are doing analyses to make the cost/benefit comparison. Is it true that these impacts are creating worthwhile benefits? The answer is, no, we don't find that. So we're midstream on this body of work. After that I'm not sure what we'll do next. In sum, the STIRPAT work has been a rich learning and puzzle solving experience. The most most widely cited version of our output is the article that Richard York and I did, along with Tom, from Richard's dissertation in ASR 2003.

SF I'm struck in this story and other stories you've told today about how it seems to me and this is just... you tell me if this on track or not, that your research programs seem to have developed in response to institutional incentives or opportunities, that is your... in this latest story you were responding to a National Academy of Sciences report and a mission and before, you know, a lot of your risk work and energy work has responded to similar sorts of institutional mandates or opportunities from government agencies or what not. A, do you think that's an accurate general statement about how you've chosen research problems and then, B, the inverse of that is to what extent have debates, ideas, trends within the discipline of sociology been influential in shaping your research trajectories and ideas?

ER Let me answer the second one first. I can't think of where the latter has been very influential because first of all the first rule of the ecology work I've done is every analysis (here I am not referring to true conceptual work) but to empirical analysis; namely, it must be a combination of physical and social variables. For most of the discipline, physical variables just don't exist or they're so taken for granted that they're not problematic in any sense of the word and so there's not a... well there's not a large corpus of debate or influence that really can come from there. On the first issue, at one level I would say your interpretation is correct, but I think I'm working at a different level which means that it raises the question of who will provide the context and the conversation where the key issues are really going to be honed and where you can figure out what's a serious thing to work on. The National Academy happens to be one vehicle where this takes place. I mean that's their job and so they summarize what we know, but the main point is they also lead us in directions to tell us what we don't know and where we should probably direct our efforts. In that sense, if one wants to say that's institutionally driven, that's correct. But it's really not that it's the only place where you go to look for the best challenges and the most interesting ideas that you can deal with and try to translate them into something that is worth it. Let me add, there's an add-on to sort of the faint praise I have to say about sociology and a lot of environmental sociology. , Much of it is to massage traditional concerns in sociology by throwing in environmental variables and mix. I have no deep aversions to this sort of thing, but I think we need to do much more. Also, it sort of butts up against this principal I try to follow of integrating physical parameters with social ones in a disciplined way.. This is not to say that there are interactional issues that are not interesting. But if we could fall into that pattern of focus we're not going to make the kind of discoveries I think that we need to do to really move our fields forward.

SF Let's follow up on that a little bit. So I'll go back to that question I posed earlier and try to get some traction on it. In thinking about the history of environmental sociology as a field, where do you think it's taken wrong turns or, you know, why is it given that we're now 30 years beyond the HEP/NEP framework that so much environmental sociology still looks like sociology?

ER Well I don't want to impose motivations on others, but institutionally making careers means publications and research funds and so forth and there is a risky

path and a less risky path and more or less this risky path is to expand on what's already there institutionally. I want to say this in a way that's not shamelessly self-promoting, but I've taken on a lot big risks in my career. All these things that I did could easily have fallen on its face. I had no way of knowing for sure and, in fact, I must tell you one of my eventually published article in Social Forces, from my dissertation, was first sent to another journal that sent it back and said this is not even sociology. So I think there's an element of wanting to take risks and being a risk taker because there are a lot of opportunities to do good work and do interesting work, but stay within a fairly narrow kind of, I'm reluctant to use the word, but I'll use it anyway, paradigm rather than moving outside of paradigms and maybe even challenging them.

SF Do you have any thoughts about how some of those institutional barriers and cultural barriers can be broken down?

ER Well I tell you if I had the answer to that we'd be doing a lot of different things at a lot of different universities. What's happened though is there's an encouraging part and a discouraging part. The encouraging part is the recognition that we need to do a lot of that and that the universities in particular now recognize the huge door of opportunity there which it wasn't for a long time. About 20, 30 years ago if you'd bring up the idea of interdisciplinarity, you were seen as incompetent in a discipline or mad; that's no longer the case. In fact, now the institutions are asking the question of how to make interdisciplinarity work. One obvious place where institutional force can come from is for agencies like the National Science Foundation to structure their funding around these sorts of things. And they do have some programs now that are fairly big programs where they require the cooperation of truly interdisciplinary teams. In fact, more and more of their programs are requiring that they be truly interdisciplinary and not just superficially. It's no longer interdisciplinary to just put an organic chemist and a biochemist together. Now teams may include a chemist, a physicist, a social scientist, even a legal scholar, whatever. Because the universities are being increasingly commodified, it is funding agencies and foundations who are the real catalysts for the types of institutional changes needed. The other way to break down barriers, I suppose, is to demonstrate that one can make a career by doing interdisciplinary work, so that young people have some model to follow where it doesn't seem as risky as it might otherwise be.

SF You could talk a little bit about policy and how you have striven or strove to connect your work to policy domains, policy problems and the sort of impact you think your work has had on policy?

ER Yeah, that's kind of interesting. I never had any intention of really doing policy per se, but it sort of just happened in the sense that some of the topics I chose to work on really had no deep intellectual or theoretical content to them, but mainly how do you better inform policy. But now theoretical and policy work are viewed quite differently among sustainability scientists, which includes my work. There's been a development where this is now becoming widely accepted as a perspective the idea of "use-inspired science." It is science that begins with a

challenging real problem which, in addressing it, can contribute to theory, policy, or both. In fact, there's a section of the proceedings in the National Academy of Sciences that was led by Bill Clark and Bob Cates and Roger Kasperson and my late friend, Steve Schneider and others on sustainability that is grounded in that idea. They were influenced by the book by Donald Stokes, a political scientist, called Pasteur's Quadrant. Its thesis was that the dichotomy between pure and applied science or you could say curiosity driven versus policy research is a fiction created by the scientist who set the stage for the National Science Foundation whose name escapes me at the moment...wait, it was Vannevar Bush. He pushed the idea through the Congress on the basis that the focus of the agency should be on pure science exclusively; practical and policy benefits would then trickle down from it.. It's a trickledown theory of science basically and Stokes pointed out that really a lot of science is not one or the other, it's in the middle called, as I said earlier, use-inspired science. Its exemplar was the extraordinary French scientist Louis Pasteur. I've been reading about Francis Bacon lately and Bacon was also an example of research that started with a practical problem and then used that practical problem to make much larger discovery. One of the first things that Pasteur studied was how beets turn into alcohol. Well, you know, eventually he took his findings and that of many other studies and figured out germ theory which by any standards is a pretty powerful thing to discover. Use-inspired science connects up with policy relevant work in human ecology because they both now proceed under the notion that you've sort of got this flow that's inspired by use—about additional knowledge that can contribute to theory or can pinpoint the causal arrows of what we're doing to the environment. Knowing that gives us key leverage points on what we can do to change things or gives us key information about how policy might be done better. Again, empirical results can either flow up to contribute to theory or flow down to policy, but in all cases the final resting place begins with with the idea of use-inspired research.

SF And you've been on... have you not been on committees that have looked at and tried to more directly contribute to policy discussions or decisions?

ER Well it turns out, yes, I served two terms on the Human Dimensions Committee that I mentioned earlier where a lot of this STIRPAT business got started and then four other committees on National Academies of Science. The one that most directly influenced policy was a committee on nuclear waste I served on. It was I think in 2002, somewhere around there, maybe 2003. In January 2009, recognizing that there was a serious problem with the waste program in the US; Nevada was not going to take nuclear waste, especially not as long as Harry Reid was the Senate Majority Leader. President Obama appointed a Blue Ribbon Commission to see what we could do about getting to a better solution for nuclear waste. That commission leaned heavily on that report that I was a part of for the National Academies in 2002 or 2003. In fact, they've adopted a number of the features of that report including staging the repository and, much to my satisfaction, much more and greater involvement of the public at every stage of the process. In fact, they are now taking steps to try to implement public involvement. The Blue Ribbon Commission produced a report in March of this

year I think and now they're trying to figure out how to implement it and the implementation is very much along the lines of what we outlined in that 2002 or 2003 National Academies report.

SF It must be gratifying?

ER It is gratifying, you know, even though there's a time interruption and there's dilutions of some things and accentuations of others. It's very gratifying. It is indeed.

SF So I've got one more question and we can talk more if you want, but what would be your... looking forward, what would be your hopes for the field of environmental sociology, what would you most sort of hope it is able eventually to succeed at?

ER Well perhaps a much more active role in interdisciplinary work. I think the door is certainly open more than I've ever seen before. We just need to get more colleagues to walk through. The other is...

NOTE: At this point a vacuum begins running in another room, distracting both parties for the remainder of the interview.

SF That's okay, I think we're good.

ER All right.

SF I'll bring this [microphone] closer to you then.

ER There is a remarkable opportunity to be engaged in much larger projects than we can take on by ourselves if we're willing to be part of an interdisciplinary team. What that means in many cases has less to do with risk and more to do with one's self identity in terms of group processes ... It often means you're going to have to be the soldier and not the leader because the problem is often driven by the science of the issue rather than the social aspects. On the other hand, there's instances where it's defined as a social issue, where really it's more social in a scientific sense. In that context, the social scientist would probably know the context better than anyone in which case then maybe there's a strong basis to elaborate the case and to get important social science information into the process. Importantly, there's more and more opportunities for this. The other one, which I haven't fully resolved in my own mind, but I think is part of what we should be doing, I've been doing research like many others in sort of a "neopostivistic" viewpoint where our job is to do the science and leave the action to others. Some of these areas of research are such that others are not going to see it unless we do something about it, unless we bring it to their attention. So I'm not sure what level of activism we should be to maintain our professional standing, but I do think we need to engage in more active participation and actually try to solve problems rather than just the analyzing and outlining them. So for example, Fred Buttel in that 2000 article in ASR pointed out that the

majority of our field is to point out what the problems are. Well, okay, we're diagnosticians, but I think maybe this is the time to start thinking about also being practitioners in some sense of the word.

SF It's interesting because I think there's some... I mean I guess I should pose this as a question. Do you see a tension between those directions toward greater engagement in public education, public decision making, processes on the one hand, greater engagement with scientists in interdisciplinary work relative to the work that would be required to maintain and expand and extend the field's identity and status within sociology? That is if we all went out and did a lot of interdisciplinary research, maybe we'd stop being environmental sociology?

ER I think that's a danger, yes, but I'm a risk taker. The tension has always been there in the discipline. In fact, Jon Turner and another Turner wrote a book a few years ago where it talks about sociology as characterized by a social welfare inclination on one hand and scientific inclination on the other, a tension that's never been resolved and I think that's probably still true. Concern for the environment revives that tension in a different context. But having role models like Steve Schneider who did high quality scientific work to the extent of being considered one of the top climate scientists in the world and yet go around the world and say, look at the situation we are in. If we continue to do things as we are, bad things are going to happen. It didn't vitiate his status as a scientist and to what extent he's changed our minds I think has been a very important aspect of his legacy. So there certainly is a possibility of this, it doesn't mean everybody can do it or can do it well, but I think we need to do more of it. At least stand up and get the facts out as we best know them to a wider audience at the very least.

SF That's great, that might be a good place to stop and let's see if we have more to...

ER No, I think that's good.

SF Okay, great. Well thank you very much Gene, this has been a pleasure.

ER The pleasure was mine.

Interview ends

Addendum: Below are contents of an email from Rosa to Frickel, dated June 28, 2012, which ER agreed to add to the interview. The comment is presented here in full. -SF

Scott,

Upon reflection I was very displeased with the truncation of my answer to your last question on what the area needs to do in the future to survive and cherish. What I meant by action is that we need, on the one hand, to have our work become part of the leading conversations in the political and policy communities. We are virtually invisible in the discourses, where they still exist, that frame our thinking and policy visions about

the environment. On the other hand we must make our work useful to the scientific community. Far too often are environmental issues embedded with sociological concerns and amenable to sociological input, but neglecting of the science part of the issue.. Part of the solution to this can come with an increase in authentic interdisciplinary research—combining traditional sciences with sociology—we conduct. But it won't come naturally. We need to seize opportunities when they present themselves and be forceful in our claims for the importance of our knowledge.

I don't know whether it is too late to include this, or how. Perhaps we could do a brief add-on tape recording if necessary.