Rationally Speaking #164: James Evans on “Using meta-knowledge to learn how science works”

Julia: Welcome to Rationally Speaking, the podcast where we explore the borderlands between reason and nonsense. I'm your host, Julia Galef, and with me is today's guest, Professor James Evans.

James is a Professor of Sociology at the University of Chicago. He is also the director of the Knowledge Lab at the Computation Institute, where he is a senior fellow. They use big data and high-performance computing to solve a wide range of scientific and social-scientific problems. That's the area we're going to be focusing on today.

One of the main things that James studies is what's called meta-knowledge. That's knowledge about knowledge. He's asking questions like, "Has the pace of scientific discovery changed, and if so, why? How do scientists decide what to research? How well do different scientific fields communicate with each other?" He's approaching these questions in a very empirical way, by cashing out these concepts, these questions, in concrete terms, things that we can measure and get data on.

James, welcome to Rationally Speaking.

James: Thank you, Julia.

Julia: First off, I'm curious about how unusual your approach is within sociology, of mining the data that gets produced in the process of doing science. Because my impression was that sociologists have asked questions about how science works, how the scientific process works, for decades. And I've mostly heard them asking these questions in qualitative ways, for example, looking at case studies. How new or strange is your approach?

James: I would say there is a field called science studies, and it's composed not just of sociologists, but also anthropologists and historians and philosophers of science, that has been interested in these questions for quite some time. It has tended to be mostly qualitative inquiries. And that qualitative turn really took place in the 1970s and early '80s in response to a more quantitative sociology of science and sociology of scientists, which really wasn't focused on the ideas of science, but the institutions of science. This new science studies was focused on the ideas and the epistemology, and it tended to be qualitative in response to that.

That being said, I would say just in the last few years, there's been a much wider reception and interest in this work from within that community. Partly because in the world of science, one of the things that's changing is this emergence of ubiquitous digital data, big data, and massive computation.

In some sense, there have been more and more who have studied, who have come to study privacy and big data and all these things in ways that really can only be, or often only approached with computation and big data themselves. I would say there's been a growing reception within that community to exploring different kinds of representations and using a really wide range of data to ask questions, deep questions about where knowledge comes from, how questions and answers emerge, how certainty comes about, with this data.

Julia: Let's talk about what kind of data you're using. I briefly defined meta-knowledge in my intro as being knowledge about knowledge.

James: Mm-hmm (affirmative).

Julia: Maybe you could just elaborate on whether that's a good way to describe what you mean by meta-knowledge, and what kinds of data you're collecting that help us learn about knowledge.

James: Yeah. It is. It's knowledge about knowledge. How can we understand -- from the way in which knowledge has been approached, socially, technologically -- to think about the future, and to reconfigure the future in ways that solve our problems, that answer our questions.

We're using really all the data that's available to us. Some of that data is archival, so that involves hundreds of years in some cases, in some mature fields like physics and chemistry, of publication data. It involves hundreds of years of patents. It involves pre-publication data, like conference proceedings and transactions. The first scientific "journal," quote unquote, wasn't really a journal at all, but a compilation of meeting testifications. The philosophical transactions of the world's society, which began in 1660, was really kind of a conference proceedings plus transactions or letters.

We use all those things, but more now as well. With the emergence of really powerful learning technologies that allow things like your iPhone to be able to recognize your speech, they're also able to recognize and extract things like images and equations and other concepts from papers, more and more systematically. All that becomes available data, and much of it's shared informally in blog sites, on the web, and in other ways that we also extract.

There's this enormous explosion of pre-publication archives where people share early manuscripts and they actually update over time, things like the arXiv for the physical sciences. It's where much of mathematics and physics takes place and is updated over time.

All these data. But we're also interested in opinions, and intuitions by individuals. We've been working to create intelligent and adaptive surveys and ranking mechanisms so people can share their maybe-even-not-completely-articulated intuitions. About which things about which they're more certain, or about which they might be more interested, or they might attend more to.

We're interested in putting all these data together through a massive integration into models that allow us to understand how it is that science as a system thinks.

Julia: James, one of the questions I know you've investigated, that I've wondered about myself over the years, is whether the pace of scientific progress has changed over the years. And how so, and why?

I've just had sort of off-the-cuff debates with people about this. And what I think is so interesting is that there are these different, contradictory a priori models one might have about this question. Where on the one hand, someone might say, "We should expect that the pace of scientific progress would slow down over time," because -- this is basically a low-hanging fruit model, where we solve the easy questions early on. And as we use up the easy questions, we're left with harder and harder questions that take longer and longer to solve. So the pace would slow down.

On the other hand, there's this other a priori model someone could have that says, "Well, we should expect the pace to speed up, because each new scientific discovery is the product of the pre-existing knowledge that we have. The more knowledge we accumulate, the more we should be able to discover." Maybe it's not fully exponential, but there's some kind of exponential or synergistic component there, that determines the pace of progress.

So far, for me, these debates have all been a priori, maybe with a little bit of anecdotal squinting at examples. But I'd be curious to hear what you've discovered about this from the data -- and how you've defined scientific progress, because that's not really a straightforward thing to define.

James: Uh-huh. Yes. Let me maybe rephrase that question back to you in terms of the way in which people have talked about it in my field.

The "burden of knowledge" is the way in which most people talk about that first question, the low-hanging fruit question, or the low-hanging fruit theory. That basically, we've picked the low-hanging fruits and we have to basically climb higher and higher in the boughs of knowledge before we want to gather.

It turns out that there have been some very interesting papers in the last few years by Benjamin Jones up at Northwestern University, Bruce Weinberg at Ohio State University, and colleagues have explored this possibility. They do show, over the course of the last hundred years, a progression in the age of scientists until they write their first big paper, by medical scientists until they achieve their first big grant, Nobel prize winners until the age at which they do the work that gets recognized for the prize. It turns out that they don't find that for invention, however.

The other effect that you're talking about, I would say people often talk about it as what Stuart Kauffman at the Santa Fe Institute labeled the "adjacent possible." The idea that every time you end up discovering new bits of knowledge, they become components that can become combined with the things that were available before.

Julia: Yes.

James: Basically, what's going to happen at the next period is only one step from what takes place in the current period, but that everything available to you at that current period can be combined in this new way.

I would say, there are more questions than answers, in this domain. Maybe that's depressing.

Julia: Hey, a lot of my episodes are about philosophy, so that's nothing new for me.

James: Yeah, and the more we learn, it's kind of like a growing sphere. The surface area and the outside is the questions that we don't have answered, and that sphere is continuously growing with the size of what we know.

Something that it appears that we do know, is that yes, it's certainly taking people longer to produce professional science in some ways. What we don't know is whether that's because we've exhausted all of the low-hanging fruit, or if we've exhausted all the low-hanging fruit on the trees that we've chosen to pick from. Which is to say, disciplines or fields are like nexuses of questions, and methods, or perches, to solving those questions. By designing a field, like physics or sociology or anthropology or ecology, we're basically taking a class of questions and we're mixing them with a class of approaches, and that's a discipline.

The question as to whether that burden of knowledge is the result of just an exhaustion of all simple things, or just an exhaustion of those simple things that we've chosen to focus on recently, is a big question. Because it seems that sometimes you see interdisciplinary work end up yielding outsized benefits. It tends to yield those outsize benefits when there's been a focus on the disciplines prior. It's like, as more focus goes into these nexuses of questions and answers, or questions and approaches, then it provides more opportunities for there to be outsized benefit, from linkages and arbitrage from questions and answers that take place outside.

I think one other aspect of that that we've actually studied recently is: How it is that as fields mature, the questions and answers of science change. For example, as a field early in the life cycle of a field, it turns out it ends up being really efficient for people to exploit the structure of the knowledge that they already know. Things, molecules, diseases that look central in this kind of space of scientific questions and answers, end up getting disproportionately focused on. That ends up being a really efficient strategy.

What we find professionally- So this is a paper that I published in the Proceedings of the National Academy of Science last November. We find that as fields become more mature, the most efficient question for the reconfiguration of scientific knowledge, ends up being a very risky question. Scientists only have so large a budget of experiments or investigations that they can engage in. And so what we find is the most efficient way of discovering the chemical relationships that have been kind of discovered over the last fifty years, is very different from the pathway that science as a system has in fact investigated them.

Julia: Can you explain a little more what you mean by efficient?

James: If you were to just create a strategy for asking questions, and you were to unleash that strategy on a system of relationships, and it would use its logic, its strategic logic to ask the next question, we basically find that the strategy the scientific community has pursued, both in patents and publications over the last fifty years, ends up being conservative. And increasingly so. Which is to say, they pick really central things, like really central biochemicals and molecules, and they investigate things that are very close to them already in that scientific space.

Julia: Okay. I'm trying to make the analogy to fields I'm a little more familiar with, which tend to be in the social sciences instead of the hard sciences. Would it be something like in, say, cognitive science or behavioral economics, it would be taking a phenomenon that's already well-established like -- well, I was going to say priming, but that's a bad example maybe because it's been undermined recently. But let's pretend that priming hadn't been undermined.

James: Prospect theory, or something like it.

Julia: The conservative, non-efficient thing would be to just do more experiments looking for more examples of priming in slightly different contexts than had already been studied. Whereas the more efficient but riskier thing would be to try to discover a whole new phenomenon that's not priming.

James: Right. And to connect things that are less dominant than priming with other less-dominant things at distant parts in this scientific system.

Julia: Yeah.

James: It turns out that if you want to just discover what was discovered, you can discover this much more efficiently through a strategy that goes precisely in the opposite direction as that with which science has gone. Which is to say, you start off by asking conservative questions that exploit the knowledge you have, but then you recognize this diminishing margin of returns to studying priming in the next context, for example.

Then you start looking at other, maybe less pronounced heuristics or biases, if we were taking the psychological example, and looking at those in other contexts. And because this network of scientific claims ends up being fractal, then that ends up being the most efficient strategy.

It's basically a strategy that begins conservative and becomes really quite risky later on. Versus the strategy that we see, which is actually the most efficient for discovering about fifteen percent of what we know. Unbelievably inefficient at discovering one hundred percent of it. Because scientists need to stay in the game. They can't amortize really risky experiments, because each scientist is like a little entrepreneur, and if they have enough failures, then they go out of business.

Basically, the strategy of science seems to optimize getting tenure and job security while also exploring the scientific space, but it certainly isn't optimal at the level of the system for exploring the space of scientific possibilities.

Julia: I understand the logic of incentives there, but why has that increased over the years? Has something changed about the way we award tenure to scientists, or about the job market, or something like that? Or is it something about the content itself, that the questions themselves have gotten, I don't know, easier to investigate conservative things than efficient ones?

James: I think that's a great question. I think it certainly appears as though there's an increasing preference for focusing on research within-field, and not across-field. And that could be the result of the enlargement of science, which naturally places competition in a new way.

Many have argued -- and I would say that the evidence isn't really completely in -- that it also has something to do with the decreasing, in the US, federal largess for science. There's more bodies that are asking for relatively less dollars, and so that puts a pressure on the system of advancement.

I think it's definitely some mixture of those two forces. I think that the jury's out. I don't know exactly what that mixture is, but we're very interested in exploring it, and have been by looking at things like competition in funding more directly. The jury's still out.

Julia: Yeah. I also wonder whether some element of the increased conservativism might be a good corrective, might be a thing that we should have been doing all along, but weren't.

You could say there are diminishing marginal returns to discovering priming in one additional new context. In a sense, that's true. But given the many problems with science and especially social science that we've been uncovering lately, or that have been getting more public attention lately, maybe all of those minute, more conservative experiments are playing the role of replications. Where the more we look at a given phenomenon in different contexts, the more we're going to notice if that phenomenon was on shaky ground to begin with.

Maybe the stuff that looked like efficient, risk-seeking or at least risk-neutral exploration early on was just sloppy. Because we weren't actually trying to be meticulous and make sure that what we were discovering was, in fact, real.

James: I think that's a very generous interpretation of the system.

Julia: Yeah.

James: I think it's actually quite doubtful. The reason I say that is, we've been engaged in a large number of quasi-replication studies, both in the social sciences and in the natural sciences, in exploring drug-gene interactions and chain-gene interactions. We've been scraping many millions of these from texts and then looking at how they stack up against intensively replicated high throughput experiments.

There are very few cases in which people are precisely replicating work that was done in the past. They're engaging in work that is really exploring this adjacent possible, but in a very local way. There are very few of these streams of claims that end up being really independently replicated multiple times by different kinds of research groups.

Because the scientific returns to those replications are very low. It's very difficult to publish a replication of a finding that's not controversial, or that's not seen as really substantively critical for understanding a particular system.

I agree that this move that we see, I think in psychology and in the social sciences -- I think it really began in the biomedical sciences, and medical genetics -- I think it's a good thing. I think that really trying to decrease our false positives is good for certain kinds of problems.

But not for all kinds of problems. There are a lot of problems where we might want to do the opposite, which is to say, we want to decrease false negatives. This is not only true in tracking the sources of terrorism, but there are other classes of problems where we don't just want wrong positives, but we don't want to miss hidden positives. I think both of those forces, I would say this replication movement which I think is an exciting one, and I think big data and exploiting high throughput experiments, and this enormous archive of history can allow us to get a handle on that.

I think we also need to be able to model where it is that scientific attention has gone. To figure out what are the classes of questions that systematically haven't been asked, that could have been asked, that could yield valuable and interesting and promising results.

Julia: Cool. Shifting tracks a little bit, another question that you've investigated that I think is really interesting is the cultural landscape of different scientific fields.

We're used to thinking of culture as being literal culture, a shared language, shared customs and thereby, ease of interaction, etc. But as you've noted, there are also cultures within science. There's jargon that some fields use that is incomprehensible to other fields. There's ways of analyzing data that are common in some subfields and not in others, etc. How did you approach the question of where the cultural boundaries lie across the different scientific fields?

James: I think there are a number of ways in which we've approached it. I'm guessing the article that you're referring to in which we talk about finding these cultural holes between scientific fields is one where we trace out the jargon, the frequency of phrases in different fields of science. And then use those to trace how it is that fields that cite one another, that may be relatively close in their apparent attention to the other field, what they would have to know, what they would have to learn from the codebook of science, to be able to read that work in their neighboring disciplines.

We find that some disciplines, like molecular biology, have an enormously large shared vocabulary. So it's shared among those scientists, but it's very different from the rest of science. They can speak with one another relatively easily. Whereas other areas like the ecological sciences, have very little shared vocabulary, and so this enormous balkanization between people who study bears and people who study rats.

In the social sciences, they're somewhere in between. We share a fair bit of vocabulary, but there are boundaries between certain domains of social scientific inquiry, especially those that are intensively invested in statistical and causal inference, and those that take a more discursive, interpretive, qualitative take.

Julia: When you mined the language used by these different fields, were you using something kind of like Amazon's statistically improbable phrases, where you looked at phrases that were disproportionately common in one field than in others? Or how did you do it?

James: Right. You can imagine across the space of all possible phrases, that that would form what we'll call a probability distribution, and there are ways of assessing the distance between two probability distributions. This one culture will very frequently talk about some things and very infrequently talk about others, and never at all talk about others. There are divergences, like the Kullback-Leibler divergence and the Jensen-Shannon metric distance that calculate the distance between these probability distributions, or these probabilities of attention to different phrases.

Julia: What consequences follow from there being these varying degrees of shared culture versus non-shared culture? What does it matter?

James: What does it matter? I'm interested in this study both instrumentally but also humanistically. For me, it doesn't have to matter.

Julia: Sure.

James: But I think it does matter. I think that you have a situation where there are ideas and phenomena that end up being highly relevant across a linguistic boundary that make it very difficult for ideas to actually pass across that boundary.

And this is just one of the dimensions, it's not the only dimension that shapes that flow of ideas. In earlier work, I've studied plant biology. And plant biology and molecular plant biology represent only about a tenth as much research attention and produce as mammalian biology, which is of course, relevant to medicine and health and all these other kinds of things. It's also showered with more resources and more awards.

And so the consequence of things like language boundaries, but also in this case that I'm describing, status boundaries, means that ideas flow from one area to the other. They flow down the status hierarchy, but they don't flow up the status hierarchy. And they tend not to flow across these language boundaries, and especially when the language boundaries involve very special esoteric language.

I think it highlights the fact that language and jargon do lots of things. On the one hand, they do very useful things. They make it so that we can talk about things that are of common interest in a very short-hand, efficient way, but they also exclude other people and they make it very difficult for somebody outside the field to understand that short-form code. We're trying to exploit that to suggest hypotheses, to propose questions, to do large-scale experiments that would likely not have been asked by a scientist perusing the literature as a function of these kinds of cultural holes.

Julia: Now that you talk about this, I know I've observed this anecdotally in the area of rationality research and theory, which I've been most focused on. It's theoretically situated at the intersection of a bunch of different fields that include cognitive science and economics and philosophy, that are all in different ways asking questions about how do people form their preferences? And in what sense are people being irrational if their actions don't seem to match their preferences? How should we interpret it if people's preferences seem inconsistent? That kind of thing.

There is a lot of non-shared language and a lot of questions that I would have expected one researcher to have heard about, but they haven't, in part because they're being discussed in these little enclaves in fields that use different language. I think the same thing happens in the study of, say, consciousness, where you have computer scientists and philosophers and cognitive scientists and neuroscientists, all approaching the same questions but from different angles with different language. That does makes collaboration, interdisciplinary work, much harder.

James: Right --

Julia: Although- Yeah, sorry. Go on.

James: Go ahead. No, go ahead.

Julia: I was just, at the same time, as I was complaining about that, I was thinking about a potential upside, which is: Maybe if different cultures develop different approaches to the same problem, you get this diversity of approach that's actually a valuable richness. Meaning maybe we're more likely to hit something great because we're covering more of the search space than we would if everyone was working together.

James: Yeah. I think that's a very interesting, and validated intuition in a number of ways.

I have a colleague named Karim Lakhani at Harvard Business School who's been studying group problem solving behavior. He runs a co-competition, crowd sourcing environment for NASA and for a number of other environments.

He recently performed a study in which he compared competition approaches to solving a certain programming problem, to what I'll call a wiki approach. The competition approach is individual teams end up trying to solve a certain problem.

And then also a wiki approach, where everybody posts every interim solution along the way, and are aware of everybody else's solution. What happens in that setting is that basically, the entire community hill climbs. Whoever has the best partial solution, everybody flocks to, and then pursues that. Which, if the problem is simple -- In his case, the problem was simple -- that ends up being very efficient.

If the problem is complex, then you have what we often call in this kind of high-dimensional universe of possible problems and solutions, a local maxima. You reach the hill top, but you can't go higher there, you have to travel to another part of the landscape before you can find the real peak.

I've found this in modern science, that with the internet, increasingly people are becoming less independent. They're becoming more aware of other people's research. They're flocking together, and it does appear to decrease the amount of local knowledge and independence of various investigations.

You can imagine that five different cultures of research, if they're pursuing different research programs, could be thought of as five independent experiments. If you had them all talking constantly with one another, and they converge to a winning measure and a winning approach at every stage of the game, then you have one independent experiment.

Julia: Right, exactly.

James: I think there's certainly services that independence provides. Language is one of the ways to insulate people from that.

Julia: Right. There's one other thing that I wanted to make sure to ask you about. I forget which paper I read this in, maybe it was the Science paper, but you were talking about one benefit of looking at meta-knowledge as being the potential discovery of what you called ghost theories. Which are these unstated assumptions or premises, paradigms that exist in the background of our thinking that maybe we're not always explicitly pointing at, that just shape the way we approach problems.

I was hoping you could just give some examples of ghost theories and talk about how, if your investigation of meta-knowledge has uncovered any of these. Or if not yet, then how it might.

James: There is a famous paper in the history of science by a historian named Paul Foreman called "Weimar Culture Causality and Quantum Theory: adaptation by German scientists and mathematicians to a hostile environment," in which he makes this claim that certain achievements in quantum physics in Weimar, Germany in the 1920s ended up being partially the result of this atmosphere in which those scientists found themselves. You have certain approaches, in this case, rejections of things like causality, determinism, and materialism that were very common in the air that end up shaping their intuitions, he argues, that led to a particular scientific approach.

If you look back at Newtonian, Isaac Newton's time, he is producing deterministic systems in a state that is largely deterministic. Absolutist, monarchy, and like everything has an absolutist flavor. And today, even though sometimes deterministic models may perform just as well as stochastic, probabilistic models, probabilistic and stochastic models capture more of the mood that everything is contingent.

I think there are a number of ways in which certain ideas and theories can feel more or less natural because a certain intellectual milieu. Because other things, other institutions behave that way, or appear to behave that way. It ends up acting like a soft or a subtle prior, a ghost theory that you end up confirming. In the sense that if you come towards a theory or a model that looks the same way, then it feels right for reasons completely independent of its experimental validation.

Julia: Yeah. That's a very compelling model to me, and it rings true. But one reason I was interested to ask about this is it feels like the kind of thing that would be really hard to pin down in a way that you could test for it. I was curious, are there quantitative approaches to uncovering ghost theories?

James: It's hard, precisely because people don't articulate it. And it requires a phenomenon where you have multiple cultures that are focusing on the same class of phenomena, who are themselves unaware in some sense of the diversity that surrounds them.

This is where, I think, publications and patents and other articulated inscription-based data fail us. And where using ranking activities and sorting activities, and other intelligent, adaptive survey and information tasks of people, that allow them to reveal how they would think about things.

For example, I published a paper with some colleagues… a few years ago, in which we explored how scientists who studied metastasis, the cancer process by which a mature cancer ends up spreading to other tissues and inflaming them with the cancer.

We asked people who exist in very different parts of the intellectual and scientific space: Cancer physicians and surgeons who are holding a 3-D cancer in their hands as they extract it. And epidemiologists, who are looking at scads and reams of data. And geneticists, who are thinking about drivers that might turn on or radicalize abnormal growth in the cell.

And what we did was we gave all these scientists a textbook, canonical pathway of the various stages of metastasis, and we asked them if they would add anything that we'd missed, or if they would rearrange the steps of the process.

We found that every one of the roughly thirty physician scientists ordered that sequence in a different way. There was this enormous diversity. And they in some ways assumed that everybody else -- there was actually much more diversity than there was in the published literature.

It's kind of like the opposite of a ghost theory. People actually assume that there's agreement on these things, whereas there's only agreement in their very local group of people who study this particular problem from their particular angle. That certainty, or that feeling that other people share that opinion, ends up again suggesting that their assumption about agreement is itself a ghost theory, which is in this case, wrong.

Julia: Maybe the ghost theory here is that there *is* a correct ordering.

James: Right.

Julia: Well, I don't know. Maybe one of those small groups is actually correct and the others are wrong. But that wasn't the sense that I got.

James: Some of them were a small difference, but some of them were large. Like is it possible for a metastasized cancer to then subsequently metastasize? There are really two big groups, some who believe that it can't, and others that believe that it can.

Exactly. The ghost theory here is that there's agreement, and correctness. It may be that one of these thirty persons is absolutely correct, and it may be that there are as of yet undiscovered elements in that process that will reconfigure our view of it for everybody.

That's one example of how you can can tease out intuitions about how probable people think certain kinds of explanations are, even if that doesn't come out in their writing. Because maybe they're not able to test that thesis to their epistemological standards of their field, or maybe they just assume that everybody agrees with it, when they may or may not be correct.

Julia: Right. That's interesting. I had assumed that the study of ghost theories would have to involve a lot of qualitative, creative hypothesis generation work at the outset, and that the quantitative, more rigorous hypothesis testing would come in only later after you'd uncovered some promising potential ghost theories. But it seems like you're pointing at a general approach that may actually be pretty fruitful in yielding ghost theories, which is asking people not just object-level questions about their field, but their impressions of other people's answers to those questions as well, to uncover false consensus.

James: Right. There are also traces, and these are softer traces, in approaches like the one I described earlier on. For example, where you're able to identify a systematic field-level strategy for asking certain classes of questions and following questions with other questions. That suggests a kind of hidden theory that those next questions are the fruitful questions to ask, and they trace out what you imagine would be the kind of class of scientific theories that are worth asking, developing, testing, etc. You can see from the trajectory of scientific questions what assumptions would need to have been held for people to have made the inference to move from this thing to that thing. Does that make sense?

Julia: I think so. Do you have any examples from social sciences, either of ghost theories that you've uncovered or that others have uncovered, or of your own hypotheses about what kinds of ghost theories might be operating?

James: That's a good question. I am a social scientist. Most of what I've studied is in the natural sciences, and the biological and the physical sciences. Let me think for a moment.

Julia: Is that because the data is just more concrete or quantifiable in the hard sciences?

James: That's very interesting. The place in which I started studying this was chemistry, molecular biology, elsewhere. It turns out that yes, it's because the nouns and the verbs are so well-behaved.

Julia: Yeah.

James: A chemical and a reaction, they're described in very similar ways, systematically, over a large corpora of texts.

You can actually quantify even this. I have a recent paper where we explore how ambiguity works across fields. We measure ambiguity as a function of how frequently other meanings or synonyms with slightly different patterns of meaning, end up basically being replaced with each other. Conditional on contexting, what we find is that indeed, chemistry, biology and medicine use natural language in the most templated, precise ways, but the humanities and the social sciences use them in much more ambiguous games -- ways.

Julia: I think "ambiguous games" was a little Freudian slip there, I've got to say.

James: Okay, actually, the paper is about an ambiguous game, and it shows actually that regardless of which field you're in -- conditioning on the amount of ambiguity that's there -- that the more ambiguous the claims in your abstract, in your title, in your paper, the more likely it is for people who build on your work to build and engage with others who are also building on your work.

Basically, really important ambiguous works like Darwin's *Origin of Species* and like Kuhn's *Structure of Scientific Revolutions*, and the kind of humanistic and social sciences, end up setting up powerful debates, and those debates are the things that fuel scientific fields. Not canonical answers that solve -- those are the ways to kill a field.

Yes, I think ambiguous games are precisely taking place in the social sciences, but they're also taking place in the sciences. Really important work often ends up being important because it has many interpretations and fuels debates for generations to come.

Julia: It's probably important for me to hear that, because the prejudice that I had in mind when asking this question was that I was thinking about some fields that have taken a post-modernist turn in the last few decades, like comparative literature, or probably some aspects of sociology as well. Continental philosophy for sure.

I've always had this emperor-has-no-clothes, sneaking suspicion in the back of my mind about a lot of these fields that it's not just that they're dense and hard for outsiders to understand. But that even the insiders within the fields aren't actually communicating with each other. They're giving the semblance of communicating with each other while just sort of… performing obscurantism at each other, or something.

That's a very hard thing to prove in any kind of conclusive way, because the response can always be, "Well, you just don't understand. And we say we understand each other," and so how can you really argue with that?

In fact, one of my earliest blog posts was this kind of half-baked idea about how you might demonstrate this using information theory. If you could show that words in a field, like in comparative literature, or some other sort of more post-modernist field, were being used in extremely different ways within the field, perhaps that would be a way to show that people weren't effectively communicating, because they were using words so differently.

James: This is actually precisely how we measure it. We basically take the synonym substitution entropy over context.

Julia: Entropy, exactly. That's what I was-

James: Yeah. This is our measure of ambiguity, because that's precisely what it means. It means that if conditional on the context, you have no idea what this word means because it's substituted with every other possible meaning equally.

And so it does turn out that that's more intense in the humanities. We haven't really looked at the time trend. I certainly believe that it's possible for one to have an excess of obscurantivism. It certainly appears that there is an integrating benefit of some level of ambiguity, that things that end up being more ambiguous than their peers, wherever their peers are, in the sense of that being true not just as a whole, but within every particular field that we studied. Those things that are more ambiguous end up drawing together the things that end up building on them, into a field, if you will.

Julia: Okay, so I got some validation there, and also some correction, that the thing that I was assuming was wholly bad has an upside as well. So that's probably good.

James: At some level, when politicians speak as ambiguously as they do -- and I have a cynical take on this -- basically they're just trying to curry to the many audiences and garner as many votes as they possibly can. So that they can gain the privileges of office. But that function of drawing people together through that ambiguity around these shared symbols may actually perform some function that's integrative.

Julia: We can leave it as an exercise to the listeners, how much they want to think that post-modernism is doing the good kind of ambiguous or the bad kind of ambiguous. That's an exercise left to you.

James: That's right, or the next study. Yes.

[interlude]

Julia: Welcome back. Every episode, we invite our guest on Rationally Speaking to introduce a pick for the episode. That's a book or article or website or something that has influenced his or her thinking in an interesting way. James, what's your pick for today's episode?

James: Okay, I have two picks. One is a Science magazine article from 2009 by Michael Schmidt and Hod Lipson called "Distilling Free-Form Natural Laws from Experimental Data." The second is an article in the Proceedings of the National Academy of Sciences by Charles Kemp and Joshua Tenenbaum called "The Discovery of Structural Form" from 2008.

The way in which these articles are similar is that they both basically take just raw data from the world and they throw it at this machine learning, robotic algorithm, Bayesian thinker. They both have different forms, and then they discover enormous amounts about really the structure or the form of those things.

In the case of Schmidt and Lipson's piece, they take these very quasi-random two-hinge pendulum movements. And they basically throw it at this machine that induces and produces things that look like the manifold equations, the ways in which a physicist might draw those out themselves if they were theoretically articulating them.

Kemp and Tenenbaum take association data, email data from the Bush Administration, and they take association data between the characteristics of various emails, and voting data between judges in the Supreme Court and they induce what the form is that characterizes that data. Is it a continuum, as in the case of the Supreme Court? Or is it a tree in the case of the Bush Administration? Or is it another organization?

What's interesting about these pieces is, at least for me, the way they've influenced my thinking is that because of the presence of these assumptions and these ghost theories and these heuristics… with the explosion of increasing data, physical and social data from sensors and also archival data that's extractable, I'm really interested in approaches that weaken the assumptions that we put into our analysis. And in some senses, that allow us to reveal based on last year's data, what the assumptions were that led people to find the structures that they found in the data that they had available to them.

In some sense, these automated approaches to analysis also allow us to reveal our biases to ourselves and to some degree, overcome them.

Julia: Interesting. Wouldn't there still be biases built into the way that we set up the algorithms that are mining data? Or is it just that they're going to be different biases, and so it's useful to have different approaches to investigating the world that are biased in different ways, so that we can notice those differences?

James: When you have more data, you can have weaker models. When you have less data, you require stronger models. I would say that these are weaker models. They really exploit the data and allow a much wider range of possible answers to emerge than many of the models from before.

I think it's not just that they're different, although I think the diversity of questions is important, but I think it's also important for us to systematically try to, where possible, create the explanations that give us the most insight with the fewest assumptions.

Julia: Excellent. Cool. We'll link to both of those papers, and I like that you came up with a thematically-related pair of texts. That doesn't happen often enough on Rationally Speaking. We'll link to both of those as well as to your website. And maybe I'll even throw in a link to my old blog post in which I came up with the idea of measuring entropy in different fields in order to expose post-modernism.

James: Please do. I'll give you some credit. That's excellent.

Julia: Excellent. Thank you. James, thank you so much for joining us. It's been a fascinating conversation.

James: My pleasure, Julia. Thanks.

Julia: This concludes another episode of Rationally Speaking. Join us next time for more explorations on the borderlands between reason and nonsense.