

**Steven R, Hyman**

**December 4, 2001**

This is an NIMH interview with Dr. Steve Hyman, director of the NIMH. The interviewers are Dr. Stanley Schneider, retired from NIMH, and Dr. Wade Pickrin of the American Psychological Association. The interview is conducted on December 4, 2001.

Hyman: I am Dr. Steven E. Hyman, director of the National Institute of Mental Health. I was born in New York City, July 25, 1952.

Pickrin: Thank you. Stan, would you like to start asking some kind of organizational initial questions?

Schneider: I'll read these few sentences here. Steven Hyman, director since 1996, restructured the extramural program in September '97 to address new developments in science and changes in the healthcare system. Emphases included basic research in molecular biology, neuroscience, genetics, and behavior, the translation of basic knowledge into clinical applications, and research on interventions and outcomes that have impact on clinical practice, and policy relevant to research and research dissemination. As I recall in listening to you during those early days, those were things that sort of came out. And one of the things that I was curious about was in light of that. How do you think that's gone during your tenure? What do you think still needs to be done versus what was accomplished?

Hyman: Well, I think, actually, I'm actually well satisfied with that reorganization. In some sense

-- and I say this with all respect to my predecessors -- NIMH really didn't have stated priorities, and I thought it was very important that we factor in the scientific opportunities that we had and the public health needs. And the scientific opportunities, just to review a few, involve the incredible growth in genomics and genetics and molecular biology on the one hand, manipulation of the mouse genome, but also the development of noninvasive neuroimaging, which created an enormous new arena for cognitive and affective neuroscience, for example.

You should recall that the first fMRI paper, which looked at the visual system, was published in 1991, so we're sitting at just 10 years after the initiation of this technology. And previously, for example, because PET scanning required radioactivity, had much slower collection times, you couldn't scan children or adolescents ethically because of the radioactivity. So there were these incredible opportunities.

At the same time, neuroscience and behavioral neuroscience were just making enormous progress, and the Institute really didn't, wasn't recognizing this. The second thing is that the investigation of the disorders I think was rather stymied. People were trapped in a series of pharmacologic models, basically, and, with certain variations, experiments focused on monoamine neurotransmission had been going on for about 40 years without really substantial progress, while the application of cognitive neuroscience, affective neuroscience, really was just knocking at the door, you know, wasn't really being applied. And actually, as I leave, another important area that's just being born is what we might call social neuroscience, the notion that our ability to interact with others has something to do, obviously, must have something to do with the circuitry of our brains

and the ability to explore. So there were these incredible scientific opportunities, and I wanted to make sure that our fundamental science that really goes from molecules to behavior, and which was burgeoning, really got the resources and the encouragement and the focus that it deserved. That was the first thing. The second thing is I was very discouraged by the research on the disorders and wanted really to almost start over, and start over by bringing together careful, thoughtful clinical investigators -- you know, psychologists, psychiatrists, but the discipline didn't really matter -- but careful investigators with basic behavioral scientists and basic neuroscientists to really rethink how we were going to investigate these disorders. And, of course, I also believe that one of the important keys -- and still believe this and made big investments in this to understanding the disorders -- would come from genetics. But, again, it's very important that genetics, as a purely reductionist operation, cannot succeed, that everything that goes into it depends on phenotypes, and in our field, in the mental illness field, the phenotypes are fundamentally behavioral and ultimately will be brain and behavior. That is, they may involve neuroimaging or other investigation of circuitry, but we're going to see abnormalities based on our sophistication at behavioral neuroscience; that is, at probing through cognitive and affective tasks the circuits that might be malfunctioning in these disorders. And without this better understanding of what the disease entities are, without the phenotypes, the genetics, despite these incredible technologies, really couldn't hope to succeed. So, as a result of this reorganization, we have a much greater investment in our fundamental science, again, going from molecules to behavior, and we have programs of what we call translational science focused on the fundamental disease

processes, which have really focused on recruiting good basic scientists to collaborate with good clinical investigators. And we've expanded these translational programs also to therapeutics, just as . . . I think some people might consider this unfair, but I would say that psychotherapy development has been moribund, that all of the psychotherapies in our armamentarium really date from even two or three decades ago, and that we haven't, we don't even have appropriately developmentally sensitive forms of cognitive behavioral therapies. And so a perfect example of an enormous need for translational science, even in therapeutics, is to bring the cognitivists, the social psychologists, and so forth, people who understand motivation, together with the interventionists to think about how else we can alter human behavior.

Schneider: This is another element that's obviously embedded in just a few statements you made at the time, and that was the desire really to bring the neurosciences and behavioral sciences into some sort of a synergistic relationship.

Hyman: Yeah. I actually don't see the boundary between them. I think, while obviously not every psychologist needs to be a neuroscientist -- and there are many neuroscientists are going to be working at more reductionist levels -- I mean, the fact is that I think it's an accident of our history that the disciplines are too separate and that, in fact, we should be busily erasing the boundaries.

Schneider: Yeah. And as a matter of fact, I think a lot of what happened, when I got to the Institute, and I think in my understanding of what went on before that, psychology was the basic science in mental health, and that has changed over the years so that the neuroscience, the development of neuroscience . . . This was very threatening to a lot of psychologists.

Hyman: Yes. You're absolutely onto something critical. Psychologists, who should be very wise about human behavior, actually became part of turf struggles and boundary struggles. And in fact, of course, all of these disciplines need each other. This is something I said going in and we're not all the way there yet. It's almost six years. But take the fact that we now have incredible control of the genome of the mouse, so we can insert genes, delete genes, turn them on in the hippocampus when we want. All the most interesting phenotypes are behavioral. We still lack, we still lack an adequate number of well-trained scientists who can map animal behavior onto the brain, I mean this group of behavioral scientists, without whom, frankly, all of this fabulous genetic manipulation is not going to teach us very much. If right now somebody gets a mouse mutant and says, "Well, okay, I did the mouse water maze and memory is okay, and I did the mouse intruder test, and they're not aggressive," you know this kind of cookbook approach to behavior. We're not going to learn anything profound or anything new. So we just need each other. It's quite straightforward.

Schneider: That was a major, major thing. Your remark about the diseases is also, I think fascinating to me. A very subtle change took place. The influence of outside organizations on the Institute shifted. The American Psychiatric at one time was very much involved and was replaced really by NAMI, which had its own agenda, for purposes that are easily understandable.

Hyman: Just before I arrived.

Schneider: Yeah. And this made an enormous amount of difference. There was always a plea almost. We've got to get back to the serious mental illnesses. This is really our business.

This is what we should be involved in. That's not the only thing we should be involved in, but certainly to forget that population would have been a very difficult thing, I think for the Institute.

Hyman: Right, right.

Schneider: And I think that you're absolutely correct that what's happened is it's fallen behind the times, like the psychotherapy has fallen behind the times.

Human: Right, right. Well, I think I would say that one of my goals was to make sure that nobody thought that any one professional society or patient group owned NIMH, and I think we've interacted successfully with a very diverse group of stakeholders. I mean, this is a democracy. We get monies from all the taxpayers. So, yes, it would be a tragic disaster. It would be absolutely wrong not to worry about people with severe and persistent mental illness, but at the same time, we have to educate those families that, for example, behavioral science has a critical role in addressing these problems. I mean, without good cognitive science, we can't understand the terribly disabling impairments that occur in schizophrenia. And with all sympathy for the fact that these families often equated psychology with, oddly, but did with psychoanalysis, with a system that had marginalized them, stigmatized them, and had really done everyone an enormous disservice, and, of course, had no relationship to the truth. But there was an enormous rebuilding mission. I think, again, it's a mistake . . . Sometimes, when I arrived, I almost thought that NAMI almost was intimidating the Institute. NAMI members actually needed to learn that, by studying behavior, by studying development, we're not blaming or stigmatizing the parents. We're really trying to understand fundamentally these disease processes so that

we can do a better job treating and preventing. I won't say that we've been entirely successful. I think that we live in a world where these illnesses, despite our advances, remain stigmatized, and where we also, tragically, have subtle sniping between different professional groups who have different toolkits, and their toolkits may affect what they see as the facts. But I think we've made progress in changing the perception of what mental illness is.

Schneider: I must say, Steve, I . . . It's been a disadvantage not to have been at the Institute during your tenure here. But I think I've mentioned this to Wade and a couple of other people. I think it's interesting because, in writing about this, I have to go back and simply say that my feeling was it was almost impossible to do a history of this Institute that the Institute made some very wise decisions early on, and that is that it brought people in who knew their fields pretty well. It gave them pretty much carte blanche. The bureaucracy layers had not yet begun to gather on top to sit on everything that was going on. And, as a matter of fact, it was possible for a guy like Felix to have really good vision. And the questions about what made it possible for that to be the case in those days is an interesting question that I think deserves some examination. But I have been, I think, very taken with what little I've read and heard about your tenure to sort of say, hey, listen, this guy has got vision, and it's, to me, the most important thing that a director can have in this Institute.

Hyman: Well, you need vision, but to translate vision into some sort of reality, you have to be, unfortunately, pretty tough. And also, you have to take care of explaining. You have to make sure, you know, you have to explain.

Schneider: You've got to involve people.

Hyman: You know, I think -- and I'm . . . You know, it sounds trivial, but my one sort of cluster of overarching goals -- I mean, we've talked about particular areas of science and the importance of genetics and cognitive neuroscience and so forth -- but my overarching impression was that our field, even with the differences among professions and advocates, was really on an island somewhere and had grown increasingly divorced from mainstream biomedical and behavioral science. Remember, a lot of the best psychology is being done in faculties of arts and sciences, where people have only a vague sense that NIH even exists. And so I have this overarching sense that what we had to do is become right in, not on an island, but have full membership in the thick of it, in mainstream biology and behavioral science, and I think we've done that. And then I think we also really have to raise the bar on quality. I think one of the things that living on an island lets you do is to develop your own standards. I abolished our NIMH study section in genetics and said we were going to have our genetics grants reviewed at Genome, and everybody said NIMH, no genetics grants will ever be funded again because these behavioral phenotypes won't be acceptable. We've done spectacularly. Now, certain of our investigators have indeed fallen by the wayside, but, in fact, a schizophrenia grant was a very highest-rated grant in one year in that study section. I mean, the point is that, once you've gotten past a little bronchoconstriction, the phenotype of asthma isn't so easy either, and the problems with the genetics are not so different. And so I think it's been incredibly healthy for us. One of the things I made sure is that our applications were reviewed against everybody else's in the right venue, and one of the things that I did

when I started. You know, I was the first permanent director after rejoining NIH. There was a pressure for our study-section system to join NIH. But instead of cutting backroom deals and creating chimeras -- and here I was very fortunate to have really, I think, thoughtful and wonderful colleagues directing other institutes -- is we started first with neuroscience and then with behavioral science, as you know, basically started with a blank sheet of paper. So the existing NIH institutes didn't have to do this. They had perfectly legal study sections. But they -- I was fortunate that they agreed to start with a blank sheet of paper. So we brought in, after doing a lot of homework, the extramural community and basically redid neuroscience review from the ground up, and then we did the same thing, as you know, for behavioral science review. Neuroscience review involved, first, eight institutes, and then two of the institutes that initially were skittish about it saw it was working and bought in, and that was the Deafness and the Eye Institute. Alcohol, unfortunately, is still the only one off on an island. And then the behavioral science review, having seen this process, there were 10 institutes that were really very, very much involved. And, of course, no review system is perfect. Any aggregation of grants creates some advantages and some disadvantages. But the point is that NIMH applications are reviewed in the thick of it with everybody else, and we are just part of a much broader community of science, and that just absolutely cannot help but be good.

Schneider: It's interesting because one of the things, of course, that distinguished the Institute in its earliest days was how different it was from the NIH, and that its differences had some advantages that still are some of the toughest problems to address, like, for example, the

whole issue of the community mental health centers, and how you relate science to people is still something that scientists basically . . .

Hyman: Right. I agree with you. There's some unfinished business. But you may have seen that we have . . . You know, one of the things that really struck me . . . You know, it's interesting. I came in; I had all of these agendas really focused on more basic behavioral science, neuroscience, and genetics. I was sure that we knew how to do clinical trials. But what I found in many ways is that the weakest part of our portfolio was clinical trials. We were doing these very small trials, asking relatively trivial questions in rarified populations studied at academic health centers. And then we wondered why these treatments were not adopted or didn't work in community mental health centers or primary care settings. So we've moved to not terminate our program of earlier-stage randomized, placebo-controlled, double-blind clinical trials, but we've supplemented it with this large program looking at effectiveness, which is the buzzword for looking at real-world populations in much more relevant treatment settings and so forth. And we have these four very large studies out doing that. None of them have results yet, but we look forward to that.

Schneider: What programs are these being done in?

Hyman: They're being . . . I mean, The Division of Services and Interventions Research is really focused on these larger-scale trials, and they all include both psychotherapy and medications. But we've also started a new program -- the announcement is on the street - - where we're actually trying to network primary care settings, neighborhood health centers, mental health centers with investigators. Now, part of the issue that you

recognize in terms of making these connections is that it's not just in our field, it's really in every field, is that at some level you need to understand what the practice is out there, what are the constraints, so that ultimately you make sure that the treatments that you study are, in some ways, implementable. And you can imagine coming up with a wonderful psychotherapy that's not implementable in any real-world setting or for any person who has a day job. So this is not to say we need to dilute our treatments, but, rather, we'd better, in thinking about treatment design, listen to the people on the front lines. So we've tried very hard to do that. We've made some progress. But it is unfinished business. It is very, very difficult.

Pickrin: Let me ask a little bit about the public health aspect of what you've done. I'm thinking I'd like to note here something about the AIDS program. I understand it's very familiar with that as well.

Hyman: Well, you know, it's interesting. We have the AIDS program because the infectious . . . It should really be the behavioral medicine of the Infectious Disease Institute, but, historically, they didn't have behavioral expertise, and I think they also believed that there would be a vaccine and this would all be sort of window dressing. Of course, it's turned out to be of extraordinary public health importance. It's also an area of our research that I'm particularly proud of, and I'm proud of it because it has really made a difference. And here, while I have supported it, the NIMH staff, especially Ellen Stover and her colleagues, just deserves enormous credit, because if you think about it, they were building a program about a stigmatized disease that was doubly stigmatized by being in an institute that didn't deal with any similar problems, was highly isolated. They

also had certain advantages, which is that we knew what the target behaviors were that led to this illness. There was an incredible galvanizing sense of emergency which attracted some really good scientists. And also, interestingly -- although this connection was not consciously made by anyone -- the underpinnings of those risk behaviors were the very same things we had been studying because they were risk behaviors for lots of other bad outcomes, including youth violence, drug use. So we actually had a kind of a foundation, only, unlike our other prevention programs, we don't know how to prevent depression; we don't know how to prevent schizophrenia. We literally had target behaviors, which were certain sexual behaviors. And, of course, for NIDA, they were injection-drug use. But basically, the NIMH program took it for granted that this had to be a public health approach, meaning that we were going to do population-based interventions, not boutique interventions, number one. Number two, that this was not going to . . . You know, we were going to basically . . . People had very low expectations of us, but in fact we were going to start with the best theory about behavior change.

Schneider: Including our acting directors at the time.

Hyman: Well, we don't have to trash-talk anybody. But we were going to start with the best theory about behavior change. We were going to take the knowledge that we had about risk behavior in general; we were going to focus it on intervention development; and then we were going to do real clinical trials. And what was amazing, one of the things that really shocked me about NIMH is that our trials were so small and so rarified in mental illness that there wasn't even a DSMB structure, you know, Monitoring Board Structure,

at NIMH. The only place that had a trial of adequate complexity and size to require a DSMB was the AIDS program, and this was for the trial that was really looking at some of the most disadvantaged people in our society. And really convincing results came out, and we've now taken some of those convincing results. Not only have they been incorporated into public health ideas throughout the United States, but we now have international programs looking at low-cost preventive interventions in several countries in Africa, in Russia, in South America. Now, I don't kid myself. These interventions are powerful but far from perfect. We still need many other kinds of interventions besides behavioral interventions. But, in sum, I'm incredibly proud of this program. And in my reorg . . . We just started talking about the reorganization. In my reorganization, I brought the AIDS program in from the cold. That is, I brought it in and basically allied it with our other sort of applied behavioral science programs because I felt that they would benefit from being connected to a successful program. You know, I think a lot of people in some of our prevention programs were . . . I don't mean . . . It sounds unkind, and I don't want it to be unkind because these were people with great hearts who really cared, but I think they were either kidding themselves or in some sense didn't really expect to succeed. They were just going through the motions. So when I say kidding themselves, you could put together programs for high-risk youth to prevent conduct problems, but when you actually looked at what they cost and what it took to implement them, they didn't have public health significance. And the AIDS program was an example of interventions with real implementable, in the most disadvantaged parts of our society and with real public health significance, and I think mental health programs really benefitted

from the AIDS program. There's one other issue . . . I'll give you a perfect example.

Think of treatment adherence. Right?

Pickrin: Oh, yeah, absolutely.

Hyman: I mean you can see the direction of benefit really flowed from the AIDS program to the mental health program.

Schneider: I can remember one time, Ellen and I worked very closely when this thing started. I think she was given something like a couple hundred thousand bucks by William Kowsky [sp.] in 1980 to go ahead and solve the AIDS problem. And when the reorganization took place in '85 and training division was done away with I went into basic sciences and basically became sort of a consultant to her. I was absolutely stunned by the entire thing when I first learned about it. I remember once saying the whole institute has to be involved in this. I'm not kidding. I mean, there were clinical implications. There were service implications. Every part of this institute. The neurosciences were the only ones who bit. I mean, I must give Steve Koslow some credit. He may have been looking for the money that was available. That's okay. The fact of the matter is, you know, I kept saying, "Listen, as far as I can see, this thing is a total model for the interaction between hormonal and immune, neural systems, and behavior in a very strong cultural context. It's got it all, and it's there. And it's the sort of thing that we need to be involved with." The reaction--he was an acting director at the time -- was basically one of disgust. And, as a matter of fact, this was fairly typical. This was always looked upon as arrival of funds. We're spending money on this? Why?

Hyman: This is money we would not have had, and if you don't create this isolation cell block

under research, the whole institute benefits.

Schneider: So, anyway, I'm very glad to hear you say this.

Pickrin: Let me pursue this just a little because of a couple of things you've said. First, this idea that an intervention needs to be implementable, and then I want to trace it back to something else you said. When you first came, you wanted to improve basic science while addressing public health needs at the same time. And so I'd like your reflection on the role that an agency, NIMH in this case, can play in terms of directing science, in this case mental health science or mental illness science, the role that it can play when you have, on the one hand, to use an old, outdated phrase, Ivory Tower scientists, and on the other hand, folks who just think that none of that's any good because you've got to be out there in the street doing checks. So I'd like to get your thoughts about that.

Hyman: Yeah, the balance. So, of course, we need all of these things, so we're very fortunate. We have a budget now of about \$1.2 billion and a very substantial and diverse group of scientists out there, and I think it's important to think about our goals. Ultimately, the goals are to stop, to conquer, to end these terrible illnesses. And how are we going to do that? Well, we don't know enough. We're working on the brain, the most complex object in the history of human scientific inquiry. And if that weren't bad enough, what we're working on are those aspects of brain function -- cognition, emotion, and behavioral control -- which represent the highest integrated aspects of that brain function. And when we work on, whether it's on diseases or important behavioral phenotypes, we're also working on phenotypes that are underpinned by extraordinarily complex gene-gene, gene-environment interactions. We have hard problems. And we are only going to

ultimately get where we want to go by doing a lot of outstanding, fundamental science, literally going from molecules to behavior.

Pickrin: So the role that NIMH can play, then, and that you've led NIMH into playing is, because of the resources, the financial resources in this case, and the institute itself, I mean, actually what you have here, is to begin to push the field in that direction.

Hyman: Absolutely. And the question has been asked of me often, why do we have to do it? Why doesn't NSF do it? Well, of course, every branch of NIH shoulders some of the responsibility for fundamental science. You know, it turns out that some of the genes that are going to be important in the brain were first discovered in cancer research. That's great. But that doesn't mean we can shirk that responsibility. And, you know, we have, because we have a sense of the goals, we don't . . . And, of course, the thing about science is you don't know where it's going, but because we have a sense of the goals, we also know what should be encouraged and what interactions . . . I mean, nobody else is going to encourage the development of affective neuroscience. Right? Nobody else is going to support research on the underpinnings of fear, for example. Right? And that's good basic science; it's great basic science. But then you have to do something else, which is that we do have to make sure that when it is ready, some of this is translatable into research on diseases. So since I've chosen fear as an example, when I arrived I was chagrined to see that so much of research on anxiety disorders involved basically infusing anxiogenic drugs into people with anxiety disorders. You know, God, you give someone with a panic disorder high-dose caffeine, can you imagine, they have a panic attack. And then you would measure everything you could think of or you'd give them lactic acid. So

what I basically did is I did away with the existing large-scale centers program that was funding research on the illnesses, mostly in the psychiatry departments, and basically said, you know, if you want to have a large-scale program . . . You know, why do we have large funding mechanisms? Well, it's to do research that you can't otherwise do, and one of those things might be the brain together, areas of science that wouldn't normally work together. So we basically created this program, this translational program, so what we have now are people working on clinical anxiety disorders collaborating with basic scientists, people like Joe LeDoux and Michael Davis, and instead of infusing caffeine into people with panic disorder, what we have is people coming up with cognitive and emotional tasks and thinking about whether they can image the brain circuitry that's involved in these disorders. It's basically taken a science that was trapped in a hall of mirrors and going nowhere and connected it with this fundamental science.

Pickrin: This is administratively, if you will.

Hyman: Yes.

Pickrin: What NIMH could do and in fact has begun to do.

Hyman: Has begun to do, exactly. Support basic science, but then make these marriages between clinical investigators and fundamental sciences, where . . . I mean, some of this happened, now is starting to happen spontaneously, but some of these actually initially were even shotgun weddings. I mean, it was quite interesting.

Schneider: It's very interesting. It's not generally considered what role we had in the creation of other institutes. When I came here, one of the first things I had to do was transfer grants in certain areas of child mental health and aging to the new institute, which at that time

had both aspects to it. There was no institute for that. Drugs and alcohol basically sprung from the NIMH, and basically was built upon a rejection of these fields, I think, by standard psychiatry, which didn't want to deal with those.

Hyman: Tragedy.

Schneider: Yeah. The Neurology Institute shared with us, particularly in the intramural program, some stuff, and so you had this . . . and then AIDS, I think, and its emphasis on behavior. I was astounded to see, I mean, after so many years of talking about this with my late wife, that the National Cancer Institute, with Barbara Rimer, is now really doing some very interesting behavioral stuff in the area of cancer. They wouldn't look at this stuff a few years back.

Hyman: Well, but their tobacco cessation program is exactly analogous to . . . I mean, in some ways, if the Infectious Disease Institute were doing the AIDS behavioral prevention, that would be the analogy to the Cancer Institute now taking on tobacco prevention. And there, I mean, there are different ways to go about it.

Pickrin: As you leave now, what remains as a weak point at NIMH?

Hyman: Well, there are lots of things that really . . . There are many things that remain to be done. One thing which is, I've just gotten going, is really to think about, to see whether our basic science is ready to give rise to completely novel treatments, both pharmacologic and psychotherapies, because while we have lots of treatment, whether they're psychotherapies or psychopharmacologic, the roots of them are all back many decades, and the question is, do we know enough now to do better, to come up with really novel approaches. So that really remains to be done. A second thing that really remains to be

done is to . . . We've begun with these translational efforts, so it's really to make a much better, more thoughtful approach to understanding the disorders that really have to be interdisciplinary. We still, it's really quite remarkable what we don't know even about the basic phenotypes, the basic characterization of some of these illnesses in the light of modern behavioral science. Just think about attention deficit hyperactivity disorder. If this were really a disorder in which attention were the basic deficit, we know a lot about attention. We might by now have a gold standard, maybe computer-based behavioral test for it, but we don't. I mean, it's probably -- people are very appropriately now thinking about maybe the major defects are in executive function, in inhibition. But what's stopping us from doing that? Why aren't the best behavioral scientists recruited to thinking about these conditions and about just the fundamental behavioral cognitive descriptions and then, of course, mapping that onto the brain. I'm very disappointed that we are not farther along in that. And then another area that I think is really a problem for us, and for all of healthcare, is the issue of how we -- I've said this before, but I think it's very important -- how we develop treatments that not only are implementable, but also implemented. How do we disseminate this? How do we understand why it is? And not just categorize it because we've done that. How do we understand how to change the fact that 70 percent, for example, of older men who commit suicide have seen their primary care doctor within a month of their suicide and many of them on the very same day? I mean, obviously there's a conversation that's not happening. Is there something we can do both by changing people's conception of mental illness, but by changing something in these medical settings so that we make a dent in this? I mean, I could list many, many

things.

Pickrin: The prevention program.

Hyman: The prevention program I think is now off on the right foot but has a long way to go, because what we had to do was recognize that while there's a romance of primary prevention, we do not know how to prevent depression and so forth. But there was an enormous public health opportunity that was being missed with this romance with primary prevention, and that is kids with early symptoms of disorder. And this was contributed to also by the reification of DSM-IV entities, which have artificial thresholds. You know depression -- there's a lot of data; we don't have to talk about it -- but depression or ADHD are, you know, these are really continuous variables where we have arbitrary cut points. And there are lots of kids and the fact that they're arbitrary cut points has a lot of implications for who gets reimbursed. But, in fact, there's an enormous opportunity to find kids with a few symptoms of depression or anxiety and intervene early on. We're just really beginning to do that, and part of that is within the prevention portfolio. But one of the things I've been subtly doing is trying to deconstruct the peculiar, almost hypnotic hold that the DSM has had over the Institute and to remind people that the DSM is a document that's fundamental . . . It's a strong document based on reliability; that is, based on the likelihood that two different interviewers will come up with the same diagnosis for the same person. Its strength is not in validity, and insofar as we reify the DSM, we create problems in early intervention, in treatment development -- it's another stumbling block, treatment development -- but also in our science. I mean, if you're imaging people with DSM-IV depression or DSM-IV generalized anxiety

disorder, just assuming that that picks out a uniform pathophysiology, I think you're in pretty deep weeds, and I think you need to remind people that the DSM was not developed to pick out natural kinds. Right? It is a document about communication.

Pickrin:

Stan, I know our time is up.

*End of Interview*