Jacob Nachmias

Review in Advance first posted on
July 5, 2019. (Changes may still
occur before final publication.)

ANNUAL REVIEWS

Annual Review of Vision Science

A Conversation with Jacob Nachmias

Jacob Nachmias,[1,*] J. Anthony Movshon,[2] Brian A. Wandell,[3] and David H. Brainard[1]

[1]Department of Psychology, University of Pennsylvania, Philadelphia, Pennsylvania 19104, USA; email: brainard@psych.upenn.edu

[2]Center for Neural Science, New York University, New York, NY 10003, USA; email: movshon@nyu.edu

[3]Department of Psychology, Stanford University, Stanford, California 94305, USA; email: wandell@stanford.edu

Annu. Rev. Vis. Sci. 2019. 5:17.1–17.13

The *Annual Review of Vision Science* is online at vision.annualreviews.org

https://doi.org/10.1146/annurev-vision-011019-111539

*June 9, 1928–March 2, 2019

Keywords

vision, psychophysics, signal detection theory, eye movements, spatial frequency channels

Abstract

We are sad to report that Professor Jacob (Jack) Nachmias passed away on March 2, 2019. Nachmias was born in Athens, Greece on June 9, 1928. To escape the Nazis, he and his family came to the United States in 1939. He received his undergraduate degree from Cornell University and then an MA from Swarthmore College, where he worked with Hans Wallach and Wolfgang Kohler; his PhD in Psychology was from Harvard University. Nachmias spent the majority of his career as a Professor of Psychology at the University of Pennsylvania. He made fundamental contributions to our understanding of vision, most notably through the study of eye movements, the development of signal detection theory and forced-choice psychophysical methods, and the psychophysical characterization of spatial-frequency-selective visual channels. Nachmias' work was recognized by his election to the National Academy of Sciences and receipt of the Optical Society's Tillyer Award.

EDITORS' NOTE

This autobiographical article is in two parts. The first part, an autobiographical introduction, was written by Jacob (Jack) Nachmias; the second part is the transcript of a conversation between Nachmias, Tony Movshon, Brian Wandell, and David Brainard. The transcript has been edited for clarity.

AUTOBIOGRAPHICAL INTRODUCTION

My interest in perception was first aroused in 1948 by a Cornell University undergraduate course taught by Robert Macleod, who had lately come from Swarthmore College. There, because of his years at German universities, he had gotten to know various psychologists who were eager to leave Nazi Germany and offered them positions at Swarthmore. Among them were Wolfgang Kohler and Hans Wallach. Not surprisingly, Macleod's course mostly covered Gestalt psychology. John Krauskopf and Tom Cornsweet were in the same class, and we were all captivated by Gestalt psychology. After graduation from Cornell, I went to Swarthmore to get a Master's Degree, where I took seminars with Wallach and Kohler. Kohler was trying to redeem the implicit promise that the neural substrate of Gestalt processes were DC fields in the visual cortex. In order to follow his lead, I needed to learn neuroanatomy, physiology, and physical chemistry. To study those subjects—and get paid for it—I applied and won a Fulbright scholarship to the University of Cambridge. In addition to classes in anatomy and physical chemistry, I also sat in on neurophysiology lectures by Alan Hodgkin, Andrew Huxley, Edgar Adrian, and William Rushton, who had just switched to studying vision science. I also met young Horace Barlow, on his way to work with Stephen Kuffler.

Those were exciting days for peripheral neurophysiology, particularly nerve impulse transmission along axons and across synapses. Gestalt neurophysiology, in contrast, seemed a pipe dream, so I abandoned it in favor of visual processes along the lines of Rushton's lectures. I wanted to stay in Cambridge and officially enroll as a Part II student, but that required the permission of the professor, Sir Brian Matthews. Somehow, I never managed to get an interview with him, so I had no choice but to apply to US graduate schools. Brown University had one of the best experimental psychology programs, so I applied there but was rejected; I found out later that Walter Hunter concluded I could not do the work because my City College of New York transcript had "BLIND" stamped on it—I had 20/200 corrected acuity. I next applied to Harvard University, and they accepted me because a young assistant professor named Eric Heinemann knew me from Cornell and argued that I had good grades in several lab courses. Since Heinemann was doing research in visual perception, I spent time in his lab, and as a result became a coauthor on a paper (Heinemann et al. 1959).

Floyd Ratliff was also at Harvard then, though just about to leave for Rockefeller University. Since he was dismantling his lab room, he let me do my thesis research there. The faculty at Harvard in "Mem Hall" at some point before my arrival had concluded that the usual system of training blurred the contributions to the thesis of the student and the advisors to such an extent that it was hard to separate them and thus decide whether the student should be awarded a Ph.D. So they hit upon a drastic solution: eliminate the thesis committee altogether! The students were free to talk to any members of the faculty; prepare a thesis on any topic they chose; and, when they thought they had done enough, write it up and turn it in. At that point, a committee was chosen to decide whether the student's conjecture that they had written an acceptable thesis was correct. The result of this system was that theses were thin (including my own). However, there were notable exceptions: A few years later, George Sperling did a much-quoted thesis (Sperling 1960).

Faculty were generally not your friends at Harvard; the best you could hope for was they were not out to get you. However, the system did teach you to be self-reliant, a very valuable attribute. In the same vein was my experience doing my thesis research. I needed the optical bench that Floyd Ratliff was leaving behind. But Floyd warned me that as soon as he left, Smitty Stevens would appropriate it for his studies of brightness scaling. To avoid that, he suggested I dismantle it completely, put all the components on a shelf, and then reassemble it only after Stevens had failed to find it.

I won a National Institutes of Health (NIH) postdoctoral fellowship in 1955 and could have stayed on at Harvard, but chose instead to take it to University of Rochester, with Bob Boynton as my supervisor. The choice of Rochester was due to Floyd Ratliff. He got his Ph.D. at Brown University, and he arranged for me to spend a few summer months there in the lab of Lorrin Riggs. There I worked with Tom Cornsweet on measuring small eye movements. But I could not spend the two postdoctoral years there at Brown because my wife, Vivianne, was a medical student at Harvard, and there was no medical school at Brown at the time. Floyd arranged for me go to Rochester, under the nominal supervision of Bob Boynton, a classmate of his at Brown. So to the amazement of Vivianne's advisor, she opted to transfer to Rochester, which he considered a decidedly second-rate medical school. At Rochester, I set up a lab to measure small eye movements, scrounging equipment mostly discarded by the physics department across the way. I devised a system to simultaneously measure vertical and horizontal components of involuntary small eye movements made by an observer instructed to hold steady fixation. The system generated miles of records on 35 mm photographic paper. I spent the next few years measuring those records, which led to three *JOSA* papers (Nachmias 1959, 1960, 1961).

My first academic appointment was Instructor of Psychology at Swarthmore in 1957. I did not collect any data there, but had the excellent mechanician, Otto Hebel, produce for me components for an optical bench system. I knew I could not stay at Swarthmore because the course I most wanted to teach, perception, was "owned" by Hans Wallach, who showed no inclination to share it. A summer at University of California, Berkeley working with Tom Cornsweet convinced me I had to go to a less confining environment.

I jumped at the chance to move to University of Pennsylvania in 1961. There I set up a proper vision lab of that era and had my first postdoc assistant, Robert (Bob) Steinman. On his own, Bob decided to measure small eye movements by a new method and spent the next few years trying to undermine the current interpretation of small eye movements during attempted steady fixation: I and others interpreted those as outputs of a feedback system designed to compensate for random fluctuations in the output of the oculomotor system. Because microsaccades could readily be measured, they were used to test properties of the system that kept the eye "on target," though the stabilized image experiments showed that this motion was also needed to counter neural adaptation. But surely there must also be "slow" components, which were harder to identify. On the other hand, Bob contended that microsaccades are merely noise that could, with effort, be suppressed.

When I arrived at Penn, the department was a hotbed of "mathematical psychology" as a result of a "coup" by Eugene Galanter and newly arrived Robert Bush and Duncan Luce. Most of the work in those days was writing and testing models of simple animal learning. But there was also interest in the proper characterization of psychophysical data. Elsewhere, Wilson Tanner and John Swets had applied Signal Detection Theory (SDT) to the performance of human observers in different psychophysical tasks. So several of my early publications at Penn were related to SDT: measurements of receiver operating curves (ROCs, tradeoffs between hits and false alarms in detecting a stimulus of fixed intensity under different "payoff" conditions or presentation probabilities) and psychometric functions (probability of detection as a function of stimulus intensity).

Then a totally new research front opened for me at an Optical Society of America (OSA) meeting—I believe in 1966—when I heard an early version of the famous Campbell & Robson (1968) paper on "Application of Fourier analysis to the visibility of gratings." The published paper incorrectly assumes that I was the reviewer. Nevertheless, it gave me an opening for asking if I could spend my forthcoming sabbatical in Cambridge.

My family and I sailed for England the morning after the police riot in Chicago. That year proved to be the most important in my own and my wife's careers. Based on work that year, she published a famous paper with Hugh Huxley (Nachmias et al. 1970), and I several in the *Journal of Physiology* and *JOSA*, with several coauthors (Blakemore et al. 1970, Blakemore & Nachmias 1971, Sachs et al. 1971). I spent much time in Fergus Campbell's lab, which resulted in one joint publication (Campbell et al. 1970). However, to Fergus's displeasure, I also worked with Colin Blakemore, who now had his own psychophysical lab, and in John Robson's lab with Murray B. Sachs, who had come to do electrophysiology with John. But while John did that sort of thing in the lab of Christina Enroth-Cugell, he never got around to doing it in Cambridge. So Murray and I did some psychophysics to find out how a grating consisting of two sinusoidal components was detected (Sachs et al. 1971). While the interpretation of those results and related ones in a paper with Norma Graham (Graham & Nachmias 1971) could be challenged (and were, notably by Don Kelly, from Stanford Research Institute), the data themselves, expressed as frequency-of-seeing functions, stood on their own. However, the earlier results in the papers with Colin Blakemore (Blakemore et al. 1970, Blakemore & Nachmias 1971). I could never replicate with frequency-of-seeing methods. The earlier results were based on the standard psychophysical method used in Cambridge at the time: the method of adjustment, with the investigator often serving as the sole observer. Back at Penn, I did research in that vein for several years, mostly with graduate students (Norma Graham, Andrew Watson, and David Field). I eventually became convinced that the study of pattern-specific circuits in the visual cortex (channels—to use an odd term) by psychophysical methods alone had reached a dead end. So in my last few years in the lab, I studied an entirely different topic, namely, discrimination of metric properties of two-dimensional visual figures (Nachmias 2011).

TRANSCRIBED CONVERSATION, MAY 17, 2018

Tony Movshon: This is David Brainard, Brian Wandell, Tony Movshon, and Jack Nachmias.

Jack Nachmias: Where did you want to begin? I'm remembering that we came through Berkeley a few times because an old colleague of Vivianne [Nachmias'] was there. He became Vice Provost. We stayed at the Faculty Club.

Brian Wandell: It's a nice little downtown spot. I once took Horace [Barlow] on a ride from the mountains down to the Faculty Club at Berkeley to see Gerald Westheimer.

Jack Nachmias: I used to have lunch with Westheimer at the School of Optometry.

Tony Movshon: Actually, I don't know if you have seen his autobiographical piece for the *Annual Review* [*of Vision Science*] as well, which is nice (Westheimer 2016).

Brian Wandell: There's a topic I wanted to hear from you about. I normally thought of Westheimer as a kind of guy who, I would imagine, had a black field somewhere and you put a little light into it and he'd measure how the light would spread. And he would think about photons. And I was thinking there was this transition from that era when people would just imagine vision as "we'll count every photon" to an era that you really opened up. You would look at these guys with their gratings, where it was all about contrast around a mean. I was wondering if it seems like it was a big change to you guys. It wasn't obvious that the shift from counting photons to looking at the relative photons mattered to you in those days. How did that feel? Did you feel a lot of resistance in that era?

Jack Nachmias: Absolutely. There was strong opposition at meetings, from Don Kelly, for example.

David Brainard: But Kelly was himself measuring thresholds for contrast flicker.

Brian Wandell: It's surprising to hear that about Kelly. He was a guy who wanted to fix the eyes, but he wouldn't do a modulation?

Jack Nachmias: Basically he was not willing to believe that anything psychophysically important happens past the retina.

David Brainard: Was it more the idea of spatial-frequency selective channels that he objected to?

Jack Nachmias: Yeah, it was ascribing things that had importance to postretinal activity.

Tony Movshon: Wasn't his framework the retinal framework of photons and backgrounds rather than contrast?

Jack Nachmias: Kelly accepted complex receptive fields in the retina, but that's as far as he was willing to go. He must have thought the information that the retina produced was simply mapped directly somehow.

David Brainard: When you started thinking about channels or contrast sensitivity functions, I guess this is kind of at your first visit to Cambridge. Was that then very explicitly in your minds about psychophysics pushing into the cortex, or was it more abstracted than that?

Jack Nachmias: It was more connected to the cortex for important things to happen at a supraretinal level. And the question was how to study it.

Brian Wandell: So there would be Kelly. What about someone like Matt Alpern? Or again, the kind of thing that [Ed] Pugh and [W.S.] Stiles and those guys were interested in.

Jack Nachmias: They more stood apart because the processes they were studying did not require you to have any special beliefs about the cortex.

Tony Movshon: So Jack, in that spirit, how much of the psychophysics that emerged in that time, let's say the late 1960s, was driven by the discoveries of the physiologists in the cortex? How much of it came from other sources?

Jack Nachmias: It was inspired by cortical physiology. It was also notable to me that, in Cambridge, they treated the entire visual system the same way they would treat a single axon. Namely, they believed that you didn't have to worry much about the psychophysical method. There were a lot of papers in which the data were collected in a single observer, who was usually the coauthor. And the data were collected by the other coauthor, by the method of adjustment or method of limits. I think that one thing that I brought to Cambridge at the time was forced-choice psychophysics.

Brian Wandell: When you went to Cambridge, you had already done your work on discriminability, right?

Jack Nachmias: Yeah.

Brian Wandell: So you were very familiar with the signal-detection method. And that was something that, when you showed up in Cambridge, you started telling them about?

Jack Nachmias: Little by little it crept in, but at the time I was there it was considered a waste of time.

Tony Movshon: I was an undergraduate there doing a project. I did my final-year undergraduate project following on from things you'd done with Colin. It was much easier to measure things with the method of adjustment. It took longer to do it the other way.

Jack Nachmias: Colin Blakemore, yes. That whole episode was exciting.

Tony Movshon: Yeah, that sounded like fun. I was aware of you but I'm not sure we ever overlapped in the lab.

Jack Nachmias: But you know I published two papers with Blakemore, which were based on that psychophysics. They were a little disappointing to me in the end, because when I came back here, I tried to replicate some of those findings. That sharp tuning curve and all that. I couldn't get them.

Tony Movshon: I remember well because I was able to replicate and extend one of them on orientation tuning. But it was also with method of adjustment. I never made the mistake of going back and trying to replicate it with another method.

Let's name a few names. There were a number of people you had to convince about this, including Fergus Campbell, Colin Blakemore, John Robson. Which of them did you find most receptive?

Jack Nachmias: John Robson.

Tony Movshon: I would've thought so.

David Brainard: I also think of John as always interested in improving methods.

Brian Wandell: He is. Would you talk of those days? John had spent time at the Bell Labs in New Jersey. Were applications, in terms of television or displays or printers, ever a part of the discussion back in those days?

Jack Nachmias: Not as I recall, no.

Brian Wandell: I wondered if that seemed to happen over the years when you were at Penn. Did applications come up more and more?

Jack Nachmias: No, I'm thinking now that he [Robson] wasn't that concerned with applications. He was concerned with basic processes.

Tony Movshon: Did you overlap with William Rushton at Cambridge? William left to go to Florida in about 1972 or 1973 or something like that.

Jack Nachmias: He was not there when I was.

Brian Wandell: I have this image of Rushton because, I never met him, but there's this kind of famous *Scientific American* picture in which Rushton and Clive Hood took an alligator and stuck it in a closet and let all the rhodopsin regenerate. Then they opened the door and took a flash picture. You could see the visual purple in the alligator's eye. David is nodding.

David Brainard: I use it in teaching all the time…

Brian Wandell: I still use it in teaching.

David Brainard: … and I make up stories about how it was acquired that involve undergraduates darting out of the way as the alligator came out.

Brian Wandell: My stories are of Clive Hood opening the door while William takes the picture and then he closes the door right after it. I never met the man and know nothing about him. I just think of him as opening the door to the alligator.

Tony Movshon: Clive Hood was terrific. Do you know who Clive Hood was?

David Brainard: So he was Rushton's laboratory guy?

Tony Movshon: Yeah, he was very skilled. Who were the other figures on the ground in Cambridge in your time there? Roger Carpenter?

Jack Nachmias: Roger Carpenter. Yeah, Roger Carpenter was there.

Tony Movshon: He did many things. He's best known for his eye movement work, but he did many things. He was a clever guy.

Brian Wandell: I hadn't realized until I read your piece how you had worked so often and quite seriously for a long time on eye movements.

Jack Nachmias: That was my Riggs-Cornsweet era. I basically started out from the Riggs-Ratliff tradition measuring minute eye movements.

Brian Wandell: How did that fit? Was that Riggs' world at Brown? Was that well embedded in Psychology?

Jack Nachmias: There was definitely a Psychology Department. Brown was fairly individualistic. It was a very small department in those days, headed by a man by the name of Walter Hunter. The Brown tradition was that there were was a lot of interactions. People had lunch together and all that. After lunches we would sit down, and after the first course, Walter Hunter comes up and moves people around because he wanted them to have interactions together.

Brian Wandell: I remember you crediting in your piece a guy named Eric Heinemann.

Jack Nachmias: Yeah, Eric Heinemann was a graduate student at Cornell when I was an undergraduate. He ended up as a visiting Assistant Professor at Harvard, visiting in that they never got tenure, until Dick Hernstein.

Brian Wandell: We used to call those appointments a folding chair.

Jack Nachmias: Exactly, a folding chair.

Tony Movshon: Did you intersect with John Krauskopf in those years?

Jack Nachmias: John Krauskopf and I go way back. There was a course at Cornell in perception by Robbie McLeod. He had studied in Germany and was big into Gestalt psychology. He gave a perception course in which Tom Cornsweet, John Krauskopf, and I were students. It was to Robbie Macleod that we should give the credit for turning us into vision scientists.

David Brainard: That's quite a crew. Was that an undergraduate course?

Jack Nachmias: Um-hmm.

Tony Movshon: Can you imagine being able to look back at that collection of students?

Jack Nachmias: I remember that Eric Heinemann had a vast psychophysical project, which was to extend the Wallach ring-and-disc contrast paradigm parametrically. Beautiful data and a very good psychologist.

Tony Movshon: Did he work with pigeons also?

Jack Nachmias: Later on.

Tony Movshon: I thought so. He did visual discrimination.

Jack Nachmias: He decided it was much easier to do good psychophysics with pigeons.

Brian Wandell: …than with undergraduate subjects.

Jack Nachmias: You had much more control. If you needed ten thousand trials, you can get ten thousand trials out of a pigeon, no problem.

Brian Wandell: In that era back at Harvard, for example, you would have people like Skinner. Skinner was obviously a psychologist, but I was [also] going to say Smitty Stevens. He also seemed to me more like a psychologist than a perceptual person.

Jack Nachmias: Yes, in my day it was a real Psychology Department. There were two poles. There was the Skinner pole, and there was the Stevens pole. They each had their empires. The Skinner pole, probably the most notable graduate was… It'll come to me. Anyway, on the other pole was Smitty [Stevens], who was doing scaling-up the kazoo scaling.

I was evidently a very good scaler in the sense that I somehow used Smitty's algorithm to generate numbers. I'd be in a little booth, and I'd have headphones, and after the sessions, he would take them off and say, "Jack, how do you do it?"

Brian Wandell: Duncan [Luce] told me that. He said that Smitty Stevens was convinced that people who did well on his scale were smarter. That this should replace all intelligence tests.

David Brainard: You mean if a power law fit your data well…

Jack Nachmias: Exactly, I was a power thinker. "How do you do it?"

David Brainard: Where was Ratliff between those poles?

Jack Nachmias: Ratliff was somewhat independent because he was supposedly physiological. He was doing his physiology. I knew him from Brown. I gravitated to him. He was a real mensch. There were few other people like him in that department.

Brian Wandell: That comes across nicely in your story.

Jack Nachmias: In those days, they had the idea at Harvard that it was hard to tell in a thesis ordinarily who had done [what] when there was a big advisory committee. So they solved the problem by saying you don't have an advisory committee. When you think you've done a thesis, you can submit it to the faculty, and they'll let you know whether your conjecture was correct. So all kinds of people failed the first time. Of course, the problem was what to do with those who failed.

Then they instituted a system that was very much like a thesis committee. Namely, there was a committee that you would have to consult, which pointed out some errors you had made. They didn't tell you how to do anything about them, but generally at that point you got hints enough so that you could do it right. I don't know if you guys know about the Ratliff story. In my lab, I'm doing some silly project on visual acuity and perceived brightness. I was using the optical bench that Ratliff was about to abandon.

Ratliff said, "When I leave here, I recommend that you dismantle this thing completely down to the elements and put them on a shelf, because otherwise Smitty Stevens is going to come and he's gonna want to use it to do scaling experiments." So that's what I did. I dismantled these beautifully mounted pieces. It was clean when Smitty came. After he went away, I set it up again and completed my experiments.

Brian Wandell: I could've imagined you bumping into [Georg] von Békésy.

Jack Nachmias: That's a wonderful story. von Békésy in those days was at some kind of research appointment at Harvard, which did not require doing any teaching except giving a prosem lecture, in exchange for which he had a lab. In those days, von Békésy was a very modest man. He would come out of his lab with his gray lab coat, and he would come down the hall to a peanut vending machine, which I also frequented. He'd put [money] in the vending machine and have a handful of peanuts, eat the peanuts, and then say, "It's time to make science." Then he went back to his lab.

David Brainard: Tell us a little about Penn.

Jack Nachmias: First of all, I got the appointment at Penn because of the buddy system. I was at Swarthmore, and a friend of mine was Henry Gleitman. Although Gleitman had not yet moved to Penn, he knew faculty there. In those days, [Duncan] Luce, [Robert] Bush, and [Gene] Galanter used to end the semester quickly so they could go to Stanford for the summer.

Brian Wandell: The summer institute with Pat Suppes.

Jack Nachmias: Exactly. So through them, I'm pretty sure I got the call to go to Penn.

Tony Movshon: The idea was that you would at least stay around in the summer and take care of things?

Jack Nachmias: No, particularly Gene Galanter had a very clear idea of what a Psychology 1 course should be. Of course, Psychology 1 really was an experimental course, and you needed to do experiments. And in order to have undergraduates do experiments, you had to have many sections, and these sections had to have instructors. You had to hire enough people that could man these sections. However, the section instructors were not entrusted with designing the experiments the sections would do. That was decided on high, and then you were to execute those experiments. That's how I came in, as a sort of lackey.

David Brainard: You were the designer of the experiments?

Jack Nachmias: No!

David Brainard: You were the executor.

Jack Nachmias: I was the executor.

Tony Movshon: What year is this that we're talking about?

Jack Nachmias: 1961 or something like that. There were many instructors. All of the instructors resented it, but that was the contract, as it were.

David Brainard: And then you moved from an instructor position to the faculty position, or was that transition made sometime after this?

Jack Nachmias: No, actually, it was at Swarthmore I was first appointed Instructor, and then I was promoted there to Assistant Professor. Then I came to Penn as an Assistant Professor who nevertheless…

David Brainard: You were actually being somebody's assistant!

Jack Nachmias: …exactly, that's what an Assistant Professor was. I think that lasted for about as long as it took for Galanter to move.

Brian Wandell: In your piece, you said something I wanted to ask you more about. You said that there's a certain point you decided that people had gone as far as they could with these channel models, and you decided to shift to visual metrics. Tell me what made you think that.

Jack Nachmias: The models began to seem more and more contrived. I felt their contact with reality became remote.

David Brainard: There can be two kinds of contact with reality. One would be contact with the actual abstracted properties of real physiological mechanisms. And another contact with reality would be as descriptions that made good generalized predictions of psychophysical performance. Is there one of those or the other, particularly, where you thought the contact got lost?

Jack Nachmias: It seemed to me, if you made something that was complex enough, you could fit the concepts to the data. The justification for the assumptions that you made were tenuous, frequently hypothetical, and not independent. Why these particular sets of assumptions rather than those? I might be being unfair, or simply rationalizing my lack of imagination.

Brian Wandell: The reason I'm asking you [is that] I was at a talk that Beau Watson gave.

Jack Nachmias: Beau was a great data collector. He collected data day and night. He designed beautiful experiments.

Brian Wandell: I was going to say that at this point, when he gives talks, he's using single-channel models, not fundamentally different from a point spread function.

Tony Movshon: Don Kelly's revenge.

Brian Wandell: It kind of felt that way. I was trying to see if maybe you are feeling that way about it.

Jack Nachmias: I haven't followed it so I don't know. I mean, the point is there again, the multiple channels and single channels models became sufficiently sophisticated that the predictions were not enormously different. The details might be of interest if you're interested in visual physiology, but if you're interested in predicting visual performance, then they're not.

Brian Wandell: That was the feeling you had back then when you decided to change. That makes sense to me.

Tony Movshon: I'll just mention that one counter case for that is Eero Simoncelli's work on perceptually based image quality metrics, which have become very influential in the world of film and cinema, and so on and so forth. These are model visual systems, which basically estimate perceptual quality as if for a human observer. They do very much better in a multiple-layer pyramid framework, that is, a multichannel framework, than single-channel models do. It's sort of a vindication for that approach from a very different angle. Jack, I want to go back to the work you did on the metric properties of figures. Does that, in your mind, harken back to the Gestalt training, to that approach to visual perception, or is that a sort of superficial similarity?

Jack Nachmias: I didn't use any particular Gestalt approaches. It was more studying a simple phenomenon, which at that time was opaque for neurophysiological modeling.

Tony Movshon: Can you enlarge on that a little? What was opaque about it? Was it chosen for that?

Jack Nachmias: I suppose you could cast size and shape discrimination in neurophysiological terms, but that seemed a little silly. It seemed much better to just deal with the phenomena on the basis of the stimulus.

Tony Movshon: I remember you telling me that many years ago, which I took vaguely as a challenge. But a challenge that I never took up.

David Brainard: As I recall, you were using a threshold approach to shape. You were particularly concerned with effects of judgments of area versus extent and aspect ratios, if I remember correctly.

Jack Nachmias: Right. Why were judgements of aspect ratios so much better than you might expect?

Tony Movshon: A physiologist might argue that that's because you have mechanisms that encode shape, which are part of the nature of visual processing, but mechanisms that encode size are incidental.

Jack Nachmias: True, but again, that's simply describing that phenomenon.

Tony Movshon: But a physiologist could actually try to measure it. They might be foolish to do so, but they might try anyway.

Brian Wandell: There are many reasons to do perception that have to do with behavior, and movies, displays, cameras, and so forth. And there are reasons that have to do with physiology. Both of those threads have been there from the beginning. In the earliest days, the connection to physiology was there from [Hermann von] Helmholtz, and also the connection to applications. The connection to applications was there from the International Commission on Illumination (CIE) and from the earliest days. It seemed to me that, at one point, around the time of your channels work, the importance of neuroscience connections just got bigger and bigger and bigger.

Tony Movshon: My experience, Jack, and I'd be interested to know if it matches yours in any way, was that, in Cambridge, the tradition was always… it was understood that perceptual measurements were a way of accessing physiological facts, right?

Jack Nachmias: Absolutely. In fact, it used to be called dry physiology, in contrast to wet physiology.

Tony Movshon: Right. Exactly, so people like Giles Brindley and others like that.

David Brainard: The other thing we haven't talked about is the early developments in signal detection that you were involved with.

Jack Nachmias: I was involved very early indeed because at Harvard, this was something that [John] Swets and [Wilson] Tanner and other people there were doing. It was considered rather kooky. I was very interested in it. Here's a case in which Horace Barlow, though he didn't credit signal detection theory, actually elevated the false alarm rate to something other than a guess, which is the key issue if you're thinking of signal detection theory. He did an experiment in which he asked observers to use two different criteria and showed that the two psychometric functions are not simply scaled upward at the lower end, but they're rather shifted.

David Brainard: Do you think he was aware of the signal detection theory work, or did he just come upon this de novo?

Jack Nachmias: I don't know whether he was aware of the theory or there was just something about it that made him feel that false alarms weren't just guesses. That was a key notion in the signal detection theory. Actually, in one of the early papers I wrote, I point that out. I definitely consider that an early psychophysical major discovery of Horace's.

Tony Movshon: Certainly Horace became aware of signal detection later and used it effectively both in physiology and in psychophysics. It's interesting to ask if he knew it at that time.

David Brainard: I'm actually always struck when I go back to Hecht, Schlaer, and Pirenne (Hecht et al. 1942). Although it was all Poisson noise and a criterion, basically it's signal detection theory that's producing those curves.

Tony Movshon: The observer has no variability in that. All of the variability belongs to the light.

Brian Wandell: It's all stimulus referred.

David Brainard: Stimulus referred, but none the less, in terms of the idea of signal and noise, where did the noise come from? That's abstract in the theory of signal detection. Some of it is in the stimulus, so I always think of them as deeply linked in that way.

Jack Nachmias: Yeah. Well, gentlemen, it's been an experience.

Brian Wandell: Yeah, it's been great seeing you.

Tony Movshon: It's been fun.

David Brainard: Indeed!

DISCLOSURE STATEMENT

Dr. Nachmias did not get a chance to review this edited article before he passed away in March 2019. The other authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this article.

LITERATURE CITED

Blakemore C, Nachmias J. 1971. The orientation specificity of two visual after-effects. *J. Physiol.* 213(1):157–74

Blakemore C, Nachmias J, Sutton P. 1970. The perceived spatial-frequency shift: evidence for frequency-selective neurones in the human brain. *J. Physiol*, 210(3):727–50

Campbell FW, Robson JG. 1968. Application of Fourier analysis to the visibility of gratings. *J. Physiol.* 197(3):551–66

Campbell FW, Nachmias J, Jukes J. 1970. Spatial-frequency discrimination in human vision. *J. Opt. Soc. Am.* 60(4):555–59

Graham N, Nachmias J. 1971. Detection of grating patterns containing two spatial frequencies: a comparison of single channel and multiple-channels models. *Vis. Res.* 11(3):251–59

Hecht S, Schlaer S, Pirenne MH. 1942. Energy, quanta and vision. *J. Opt. Soc. Am.* 38(6):196–208

Heinemann EG, Tulving E, Nachmias J. 1959. The effect of oculomotor adjustment on apparent size. *Am. J. Psychol.* 72:32–45

Nachmias J. 1959. Two-dimensional motion of the retinal image during monocular fixation. *J. Opt. Soc. Am.* 49(9):901–8

Nachmias J. 1960. Meridional variations in visual acuity and eye movements during fixation. *J. Opt. Soc. Am.* 50(6):569–71

Nachmias J. 1961. Determiners of the drift of the eye during monocular fixation. *J. Opt. Soc. Am.* 51(7):761–66

Nachmias J. 2011. Shape and size discrimination compared. *Vis. Res.* 51(4):400–7
Nachmias VT, Huxley HE, Kessler D. 1970. Electron microscope observations on actomyosin and actin preparations from *Physarum polycephalum*, and on their interaction with heavy meromyosin subfragment I from muscle myosin. *J. Mol. Biol.* 50(1):83–IN16
Sachs MB, Nachmias J, Robson JG. 1971. Spatial-frequency channels in human vision. *J. Opt. Soc. Am.* 61(9):1176–86
Sperling G. 1960. The information available in brief visual presentations. *Psychol. Monogr. Gen. Appl.* 74:11
Westheimer G. 2016. The road to certainty and back. *Annu. Rev. Vis. Sci.* 2:1–15