December 3, 2007

Mark Liberman, Director
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Dear Professor Liberman,

The United States Postal Service has sent me the green card indicating that you have now received my letter dated November 23, 2007, in which I replied to your Web postings regarding sex differences in **hearing**. In this letter, I will respond to your Web post attacking my discussion of sex differences in **vision**, http://itre.cis.upenn.edu/~myl/languagelog/archives/003473.html.

Your discussion focuses almost entirely on measurements on retinal thickness, as though my argument rested critically on measurements of retinal thickness, or on the magnocellular/ parvocellular distinction. Once again you miss the forest for the trees. In chapter 2 of *Why Gender Matters* I tried to present, and perhaps to account for, some fairly robust findings regarding sex differences in vision. The basic finding is that while many young girls seem more interested in looking at people, pets, flowers, and trees, i.e. colorful and interesting objects, whereas many young boys (though certainly not all) prefer to look at dynamic action scenes. In chapter 2, I discussed findings by Simon Baron-Cohen and Jennifer Connelan regarding newborn babies: the newborn baby girls preferred to look at a woman's face (which was stationary), while the newborn baby boys preferred to look at a spinning mobile. One reason I chose that example was because the editorial team at Doubleday encouraged me to stick to studies involving human subjects. For purposes of our discussion, however, a more cogent example would involve non-human primates. For example, Gerianne Alexander and Melissa Hines (2002) found that juvenile female monkeys preferred to play with dolls while juvenile male monkeys preferred to play with toy trucks. Gerianne Alexander suggested that this difference might derive from some pre-eminence of the parvocellular system in females and of the magnocellular system in males. I have attached Professor Alexander's review suggesting that the magnocellular/ parvocelluluar distinction might be relevant here.

I am well aware that there are many shortcomings in the original report by Alexander & Hines. And, perhaps the magnocellular/parvocellular hypothesis will turn out to be incorrect. If so, then we need to find another explanation for findings such as those reported by Alexander & Hines. Professor Alexander herself, as noted above, regards the magnocellular/parvocellular distinction as the most persuasive explanation for her findings. If you think she is mistaken in this regard, I would be curious to know how you would explain her findings; and I am sure Professor Alexander would be interested as well.

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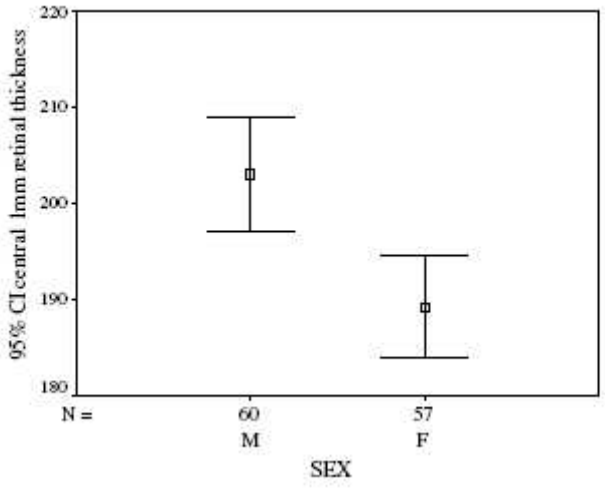
Specifically with regard to measurements of retinal thickness: your own posting is guilty of precisely the same crime of which you accuse me, namely, selecting only those studies which support your position and ignoring others. When writing a popular book for a lay audience, an exhaustive review of all relevant scholarly investigations will not be allowed. I assure you that such an exhaustive review would not have been allowed by my editorial team at Doubleday, as they repeatedly insisted that I write in a less scholarly, more accessible fashion. Many of my discussions and citations were rejected on the grounds that "this is supposed to be light reading for parents, not a medical textbook." However, you are writing a (purportedly) scholarly post, specifically attacking me on a matter of empirical fact, namely, retinal thickness in humans. Accordingly, I am sure you will agree that you had an affirmative duty (as a lawyer might say) to cite all the relevant evidence, not merely the one study which happens to support your position.

As near as I can tell, six studies of retinal thickness in living humans have found a statistically significant difference between males and females, with males having thicker retinas. Those six studies are (alphabetically, by first author):

1) Asrani S, Zou S, d'Anna S, Vitale S, Zeinier R. Noninvasive mapping of the normal retinal thickness at the posterior pole. *Ophthalmology* 1999; 106: 269-73.
2) Hee MR, Puliafito CA, Duker JS, et al. Topography of diabetic macular edema with optical coherence tomography. *Ophthalmology* 1998;105:360–370.
3) Huynh SC, Wang XY, Rochtchina E, Mitchell P. Distribution of macular thickness by optical coherence tomography: findings from a population-based study of 6-year-old children. *Investigative Ophthalmology & Visual Science,* 2006;47:2351-2357. (This study is of particular interest because it is the only one which surveyed children rather than adults.)
4) Massin P, Erginay A, Haouchine B, Ben Mehidi A, Paques M, Gaudric A. Retinal thickness in healthy and diabetic subjects measured using optical coherence tomography mapping software. *European Journal of Ophthalmology,* 2002;12:102-108.
5) Wakitani Y, Sasoh M, Sugimoto M, Ito Y, Ido M, Uji Y. Macular thickness measurements in healthy subjects with different axial lengths using optical coherence tomography. *Retina*. 2003;23:177–182.
6) Wong AC, Chan CW, Hui SP. Relationship of gender, body mass index, and axial length with central retinal thickness using optical coherence tomography. *Eye*. 2005;19:292–297.

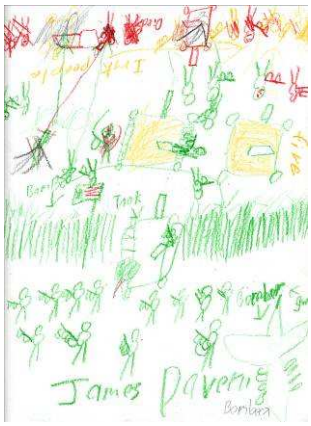
The sex difference is least striking in Wakitani et al., which is – curiously – the only one of these six references which you cite. By contrast, Wong et al. (2005) found no overlap at all between male and female confidence intervals: see figure 2 of their paper, which is attached at right. This figure is of course reminiscent of the figure from Salyer, Lephart et al. (2001) which I cite on p. 21 in *Why Gender Matters,* and which you attacked with such fury. Wong et al.'s paper is just as



accessible as Wakitani et al., yet you cite Wakitani while ignoring Wang. It is hard to imagine any justification for such cherry-picking on your part, other than the motivation to include only those papers which support your position. Surely you should make more of an effort to play by the same rules which you accuse me of violating.

As I stressed in my previous letter: my primary interest and motivation is to understand what's really going on here. Maybe sex differences in retinal thickness are important; maybe they are not. The important questions are (for example): why do baby girls prefer faces over mobiles, while baby boys prefer mobiles over faces? Why do so many young girls prefer to draw colorful pictures of people, pets, and flowers, while many boys prefer to draw action scenes with less color contrast and less detail (such as facial features)? I cite this research in chapter 2 of *Why Gender Matters,* but you never respond to these substantive issues, preferring to devote almost your entire post to the question of retinal thickness, which is certainly secondary to the main question.

If you show the two drawings on this page to any adult, and ask which was drawn by a girl and which was drawn by a boy, you can be confident that most adults will correctly answer the question. Why is that? You never even begin to address this very important question. Presumably, you would assert that these differences are "socially constructed," i.e. boys draw dynamic action scenes because they are rewarded for doing so, while boys are punished, one way or another, for drawing colorful pictures of people and flowers (or so you would presumably argue). If that position – which B. F. Skinner might have been proud to adopt – is not your position, then what *is* your explanation for these robust findings? You never offer one, preferring instead to post derogatory comments about me personally.



Likewise: What accounts for the robust sex difference we observe in color naming and color discrimination? If you are not familiar with this topic, you might begin by reading Katherine Green and Malcolm Gynther, "Blue versus Periwinkle: color identification and gender," *Perceptual and Motor Skills,* 80:27-32, 1995, a paper which I cited in *Why Gender Matters.* This again is a topic you never address; but these are the primary questions.

Professor Alexander suggests that hardwired differences in the visual system, with an M-cell predominance in males and a P-cell predominance in females, may be part of the answer to some of these important questions. I find her argument persuasive. However, I am wholly open to the possibility that she, and I, may be mistaken. **The important question here is: what really accounts for these sex differences in vision? What is the underlying mechanism at work here?** You never engage that question at all, preferring instead to post misleading and incomplete accounts which can be understood only as *ad hominem* personal attacks. Such attacks bring us no closer to any real understanding of the phenomena.

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A team of German investigators has recently reported that there are striking differences in the organization of human visual cortex (Amunts et al., 2007). These differences are quite concordant with the hypothesis proposed by Professor Alexander, namely that the M-system (the "where" system) predominates in males. How would you account for the remarkable findings reported by Amunts et al.? Her paper is attached.

As I said at the close of my previous letter: The best way to determine the truth, I believe, is not to engage in a politically-charged exchange of insults, but rather to consider all the relevant research as dispassionately as possible. I hope you will consider modifying your web postings accordingly. And I would welcome a response to the questions posed in this letter, and in my previous letter.

Sincerely,

Leonard Sax, MD, PhD

Enclosures (4):

1) Huynh et al., 2006
2) Wong et al., 2005
3) Amunts et al., 2007
4) Alexander, 2003