



UNIVERSITY OF MINNESOTA  
TWIN CITIES

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*copy 4/83 - Prineas*

May 2, 1986

*done 5/19/86  
HR info. x*

TO: Director, Division of Epidemiology  
FROM: Ronald J. Prineas *RJP*  
RE: Job Evaluation for R. Prineas, 1985-86

Henry,

As always this is a gracious and thorough summary of my performance.

My references to job offers elsewhere are not meant to create anxiety and indeed the offers from major departments have come unsolicited. I mention them for the very practical point of salary. The academic positions have all carried salaries starting at \$90,000 plus, and one at \$120,000. I am well aware that, within the department, my salary is next only to yours. The frustration comes from the knowledge that we have both brought in our salaries many times over from outside the university in all the years that we have been here. Coupled with this is my belief that your own salary should be much higher. Additionally, in my position as reviewer of national grant research applications, I am aware that most of our colleagues and peers elsewhere in the country have salaries \$15,000 to \$20,000 more than ours without any noticeable superiority of their contributions.

I have not in the past pushed for salary raises and have been pleased with your efforts on my behalf and the rest of the faculty. However, I am pushed by the moment by my burgeoning stable of college students. Salary is a fairness and marketplace issue that I think should be addressed forcefully with and by the Dean.

Thank you.

RJP:jh

*done 5/16/86  
pc: w/ HB's Soc. eval materials to Dean's office*



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Prineas

TO: Ron Prineas

FROM: Henry Blackburn [dictated but not read]

DATE: June 12, 1986

RE: Comments on review article on high blood pressure in children and adolescents

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I'm delighted with the idea of the review which seems to me most needed. Though you identify the source of the blood pressure data in the tables, you do not in the text and I think it would be useful to acknowledge the source and give the reference in the text. Otherwise, the credibility of your excellent review statements is uncertain.

You make the general statement on the first page that sex differences disappear when adjusting for body size. The naive reader, and even the informed reader, needs to know what you mean by that, or rather, needs to know what that means. Does it mean that we should always do that and that the difference is thereby unimportant? Does it mean that boys and men bear a higher arterial blood pressure absolutely and that's important? You and I have never really shared our ideas on this and I think it's the old issue that we have talked about in adjusting for Japanese blood pressures. It seems to me that it's absolute blood pressure that's more important than relative blood pressure by build, at least that's what the risk associated with blood pressure would suggest. So I'm questioning making that statement there without some accompanying clarifying statement.

I apologize -- I see at the bottom of page 1 that you do indicate where the data are derived in a very complete fashion. I guess that it would be a little better if at least the reference were included in the first sentence of the paragraph.

I'm interested that the children's studies use the first seated measure rather than the last or the average. It is necessary to comment on that in reference to comparison to adult values?

Your conclusion about the age rise in blood pressure being a natural phenomenon is an important generalization to be forcefully made, I suppose.

In the last paragraph on page 2, you indicate that 40% of the variability is explained by multiple variables. This is an interesting general statement which would be much richer in meaning if you gave us a few "for examples."

I'm glad to see your statement about the overestimation of the gene effects in twin studies, but you don't say why and that immediately raises a flag.

Memo to Ron Prineas  
June 12, 1986  
Page Two

Though it's easy to assume, I suppose, when you speak of 40% of the variability, you don't qualify by saying variability between individuals within a population. I find it always useful to do that. Does the fact that 60% of the variance remains unexplained really mean that there's more we don't know than we do? I'm a little uncomfortable with this general concept as I have the sneaking suspicion that elimination of perhaps even of one primary causal factor, and certainly of two or three, might blow the bottom out of the variance. In the first paragraph on page 3, I believe "nomograms" was intended rather than "normograms."

The European comparison would be valuable and even more so might be the Japanese comparison. I agree with you that the difference in these comparisons is more likely technical than biological, but you don't really provide the explanatory sentence to justify that sure conclusion.

Your explanation for the systematic difference with the random zero device isn't clear in one sentence on one reading. A lag, in other words a drag or inertial effect on the mercury column due to a higher column of mercury, it seems to me, would cause a systematically higher value. What am I missing?

Your statement at the bottom of page 4 about the phenomenon of regression to the mean being biased is very well explained at the top of page 5.

There is a very abrupt transition somehow between a discussion of standard tables to methodology to blood pressure variability to cuff size. It seems a general title and a sub-title would be useful so we could follow the map of what you are talking about. It's not even obvious to the naive observer, of course, why you should discuss cuff bladder size at all, whereas it's a very important part of the issue with children.

You make the shocking statement with an exclamation point at the top of page 7 but don't point out to the uninitiated the impact of that variability of ratio. In one situation, you say it's advantageous to use the arm circumference but then you show that length and arm circumference are unrelated and again, you leave the naive reader puzzled as to whether it is an advantageous procedure. The discussion of arm length and circumference just doesn't quite put it all together. Try to put yourself in the position of a naive reader and give a few transitional statements and a few concluding remarks.

The review is a constant mixture of reportage of facts and opinions. I suppose this is quite acceptable for a review, but it's a little disconcerting to have them juxtaposed. For example, you give the opinion that the latest American Heart recommendations are inappropriate at the bottom of page 7. You should tie the sentences together to indicate why they are inappropriate. The cuff bladder issue most definitely needs sub-title, as it goes on for many pages and it is not immediately apparent why this is relevant to the epidemiology of high blood pressure in children.

In fact, it might be better to sequester the whole methods section and to reconsider the order of presentation.

Memo to Ron Prineas  
June 12, 1986  
Page Three

I'm always uncomfortable referring to colleagues' findings as "he claimed," as if an adversarial relationship existed. I guess I prefer the term "he concluded" or "he stated" or "he found" or whatever. Finally, on page 10, you get to the epidemiology of hypertension, talking about prevalence. Again, consider order. Your sentence in the middle of page 10 and starting, "Among more than 10,000 schoolchildren ..." really should be redone. Again, you use the term "normograms" rather than "nomograms" and I'm not familiar with the former. Your discussion of the effect of using upper five percent cutoff is very useful.

Have you explored how much the reduction in variance is achieved by two, three, and four measures, and even by averaging fourth and fifth-phase. It seems to me we've learned so much about the value of reducing variance by multiple measures that it should now become much more standard procedure.

The sub-title, "Determinants of Hypertension" is a little off-putting. At any rate, it doesn't include body size, only obesity.

It's also a little abrupt that you switch from the concept of body size to the concept of height in the last two sentences at the top of page 12. Why would not nomograms based on body size be more useful? Again on page 12, when you compare black and white adolescent blood pressures and talk of confounding by obesity and weight, you don't give us a clear handle of what that means. Are you implying that the blood pressure is an artifact of weight and obesity, or an accompaniment of weight and obesity, in which case the confounding is biologically important and the absolute differences should not be adjusted for the weight differences. At the bottom of page 12, you actually imply that cross-cultural comparisons, or population comparisons, for that matter, require the same investigative team. I'm inclined to agree with you, but why not state it and justify it rather than just imply it.

Your statements that environmental differences, including diet, are major causes of black/white differences is not justified by the previous facts given. It is justified that environmental differences are major causes, but you have not, with the statement so far, justified the statement about diet.

Again you jump between the facts and opinion, and on page 13, you jump immediately to recommendations concerning adolescent blood pressure-taking. I think it's nice to have these issues all together in the same section, but you should be consistent throughout and identified by sub-titles and succinctly summarized. Otherwise, inconsistency of presentation arises. There could be a little summary, conclusion, relationship, and recommendations at the end of each section on raised diet and so forth. A contrasting model would be to put all the recommendations at the end. I find the first paragraph on diet reasonably accurate, but a bit diffuse. The jump between descriptive studies and trials in sodium intake again is a little shocking and there is no mention of individual correlations within cultures in well-designed studies to cover a wide range of values.

There is no reference to the adult data on fatty acid composition and so forth and so forth, which I take it to mean that such studies have not been done in youth, however, you do mention the importance of alcohol for youth and yet you don't address most of the other dietary factors, including that bugaboo, calcium.

Memo to Ron Prineas  
June 12, 1986  
Page Four

Again on page 15 you indicate that blood pressure differences between black and white girls are explained by differences in body mass. Here is where an interpretation would be useful and we don't get it.

It doesn't follow that obesity in black girls "must have strong environmental causes" from the previous information you have cited. It needs a qualifier or linking statement. You say that there have been no randomized control trials on weight reduction and obesity on children, but have there been any non-randomized control trials?

The first paragraph on page 16 is confusing: "The acceleration of the increase in blood pressure." That whole sentence is difficult. I'm sure it must be profound, but I don't quite get it. You give the correlation coefficient for repeat blood pressures of 0.6 in the middle of page 16 but you don't indicate at all the intervals or whether it refers to multiple occasions or a correlation between two points.

The first through third sentences at the top of page 17 would be helpful to have rewritten. You don't define Lauer's index in a way that's comprehensible on first reading and then you suggest that only a small proportion of children track. Then you say that genetic environmental influences are operational. I suggest that you re-read that whole paragraph critically. The last two sentences seem very clear. I'm not sure why there's so much hurry in this review article, but it might be nice to have Craig Burke read it and ask him to make suggestions, since he's up on this field and since you quoted him. He is in town and should appear next week.

The statement about obese children dropping in relative body mass while remaining relatively more obese while having relative lowering of blood pressure is fascinating, but horribly obscure. Please, a little more explication. You need sub-titles for sections on mechanisms and so forth.

Page 18: "sought" is misspelled, and "sought for" is redundant. In the next sentence you satisfactorily split an infinitive. The close comes terribly abruptly. One feels the overwhelming need for a discussion, for conclusions, for a summary, and for an abstract. One gets none of those in the closure.

"Child-specific" is individual; do you mean that, or "children-specific." The cuff size recommendation seems out of place with the dietary recommendations. There is no recommendation for screening or periodic blood pressure taking.

I am uncomfortable to hear that you are in such a hurry with this manuscript. It is great. It is needed. It could use a couple of more revisions and another editor or two to meet its potential to be a classic.

I believe that Fererich's name in reference 8 is probably misspelled, but I'm not sure. I'm sure Gail Frank's name is misspelled in reference 5. These two things that I've spotted in a cursory reading suggest to me that the references need very careful proofing. Now I see that in reference 22 that we've misspelled a name of our own post-doctoral fellow here. So you have a major problem, and you should not send this manuscript off hurriedly.

Memo to Ron Prineas  
June 12, 1986  
Page Five

In Table 1, your footnoted legend saying that the data are "adapted from" creates mild concern. What have you adapted: the data, the format, the title, the age groups? Wouldn't it be better to put the source and the capitalized title at the top of the page, "From Nine Studies of the Second Task Force Report"? There's no explanation in the table for why K5 values are not given below age 13. I think I know why, but again, it would not be obvious to the naive reader. I believe you should put the source and main title for all Tables 1, 2, and 3. Table 4 needs references in the table for credibility.

Bravo! Keep it going.

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/cmh