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Cc:

Date: Tuesday, December 11, 2007 05:11 pm

Subject: Your 2007 IJEH article

Dear Drs. Houweling, Kunst, Huisman, and Mackenbach:

I recently came upon the 29 Oct 2007 provisional PDF of your article “Using relative and absolute measures for monitoring health inequalities from cross –national analyses on maternal and child health,” on the site of the *International Journal for Equity in Health*. The article is in part a response to my 2000 article “Race and Mortality.” Probably in the near future I will respond to various points in your article, possibly on a site called JournalReview.org, where, as reflected in section D of <http://www.jpscanlan.com/homepage/measuringhlthdisp.html>, I lately respond to quite a few articles on health inequalities measurement issues. And I certainly will give your article some attention in a paper to be presented on 18 January at the 7th International Conference on Health Policy Statistics (ICHPS)

(<http://www.amstat.org/meetings/ichps/2008/index.cfm?fuseaction=onlineprogram>). Regardless of any disagreements we may have, the recognition by such prominent authorities on health inequalities measurement that some observed patterns of change over time may be functions of changes in overall prevalence rather than reflections of meaningful changes in the risk distributions of different groups, as well as that some effort must be made to consider the implications of overall prevalence in health inequalities research, is a matter of considerable importance. As your conclusions bear on the views of each of the other participants in the panel at the ICHPS conference (see item 7 below), as well as my own, I will circulate your paper to each of them.

I’ll provide you with any comments I publish online or otherwise as soon as they are available. But, inasmuch as your article is still in provisional form and there is a submitted paper (your reference 28) addressing related issues, I wanted to immediately bring a number of points to your attention. Let me add that I recognize that the provisional draft (published online upon acceptance on 29 October 2007), which cites no work later than 2005, was, even when submitted on 3 August 2006, probably an unrevised version of a draft that had been circulating. So I understand why there would be no references to my article “Can we actually measure health disparities?,” which appeared in the Spring 2006 issue of *Chance*, and which discusses the way absolute differences change as prevalence changes (reference 1 after the signature), or any of the other 2005-2007 items on the same issue discussed below). I assume that some attention is being given to generally updating the article before publication, and possibly attention to some of the matters I address may already be in progress.

1. I notice there is yet no formal citation to Race and Mortality, which I assume is because you have relied on the copies I sent to Drs. Mackenbach and Kunst in 2000 or 2001. Below is the formal citation, along with a link to a version of the article I maintain on my web site.

Race and mortality. *Society* 2000;37(2):19-35 (reprinted in *Current* 2000 (Feb)):
http://www.jpscanlan.com/images/Race_and_Mortality.pdf.

I think it is useful generally to include links to items when available online. But I am inclined to think that readers of your article would not appreciate the attention Race and Mortality gives to the way other factors may interact with the tendencies it describes or the reasons to expect departures from those patterns.

I refer specifically to the three paragraphs starting with “Two other aspects..” at the bottom of page 2 of the online version and the “To be sure ..” paragraph at the bottom of page 6. And with respect to the discussion in paragraph beginning at the bottom page 13 of your article, you might even want to think about the implications of the statement that your study “implies that” RR is “not entirely determined by overall levels” as a response to an article that stated that RR is “obviously” not so determined. In any case, however, a link to my article puts the reader in a better position to appraise such matters.

(Another, somewhat different, point pertaining to same paragraph: Regardless of how you might regard my position as to the strength of certain patterns, it has not do with “rising inequalities,” which I think is the thrust of various other writings that point to correlations between, say, declining mortality and rising mortality RRs. My main point in Race and Mortality, and a few score other writing on the issue, is that increases in RR that are solely driven by statistical tendencies do not reflect rising inequality in any meaningful sense.)

Also, as discussed below, whereas you may not think that Race and Mortality provides an explanation for the patterns it describes (as Dr. Kunst once indicated to me), I think that it does. Regardless of who is correct, a link to the article gives the reader a ready opportunity to consider the issue. See item 4 below.

2. At page 4 (2nd para, 3rd sent), your article cites Race and Mortality with respect to the debate over whether relative or absolute difference provide the best measure of health inequalities. Also, the last full paragraph at page 13, suggest that the author of Race and Mortality is unaware of that absolute differences are associated with the level of the outcome.

With regard to the first point, Bostrom and Rosen (your reference 6) cites Race and Mortality as showing that that relative differences in infant mortality increased while absolute differences declined. I also notice that the draft Dr. Kunst sent me in July 2001 cited Race and Mortality as support for that same pattern. And, of course, the data I discuss in Race and Mortality do in fact show just such a pattern. But Race and Mortality, which discusses such data solely in the context of illustrating patterns of ways relative differences change as an outcome changes in prevalence, says nothing whatever about absolute differences, either at the point of that discussion or anywhere else.

And with respect to both of the above points, in the 20 years I have been describing the pattern addressed in Race and Mortality as they bear on a range of issues (see section A of the referenced page) I do not think I have ever mentioned contrasting interpretations based on relative and absolute differences – except, that is, in the context of pointing out that absolute differences, like relative differences, would be expected to change in certain ways as the overall prevalence of an outcome changes. And I have been pointing that out for quite a long time, as in the 1992 unpublished paper that I sent Dr. Kunst on July 12, 2001 (and as also pointed out in the e-mail of that date). A version of that paper may be found at:

http://www.jpscanlan.com/images/Relationship_Between_Decl_Mort_and_Incr_Disparities.pdf

It might be noted, moreover, that such discussion (at 4-5) specifically explains why it would be a

mistake to regard declines in absolute difference between black and white infant mortality rates as meaningful improvement in the relative condition of black infants. The 2001 Oslo presentation I sent Dr. Kunst in 2001 (see item 6 below) addressed absolute differences only in that context and the same point was made even in the abstract to the ICHPR conference later that year: [http://www.jpscanlan.com/images/Abstract on Difficulties.pdf](http://www.jpscanlan.com/images/Abstract_on_Difficulties.pdf).

Further, of course, in addition to the 2006 *Chance* article, many times between 2005 and 2007, in discussing the patterns described in *Race and Mortality*, I have also discussed the way absolute differences (and odds ratios) tend to change in certain ways as the prevalence of an outcome changes. Each of the ten 2006 and 2007 conference presentations listed in section B.2 of the referenced web page presented illustrations of the pattern of absolute differences that is typically observed across the spectrum of overall prevalence, as did the previously mentioned 2006 *Chance* piece (item 1 below). Over 20 of the comments published online between September 2005 and November 2007 (Items D - 2, 6-9, 15, 17, 18, 20, 23, 25-27, 30, 31, 33-36, 38, 40-42) also addressed the ways absolute differences tend to change as prevalence changes. Some of the more notable of these references, several of which I already brought to Dr. Kunst's attention in April 2007 (though I did so with respect to other issues, and without at that point seeing a need to mention that they dealt with the patterns of the changes in absolute differences that we had discussed in July 2001) are listed at items 5-12 after the signature.

Thus, I do not think any suggestion either that I have been party to the standard debate between relative and absolute differences or that I am unaware of that absolute differences would be expected to change as prevalence changes is warranted.

3. Though a shorter piece, the 2006 *Chance* article tends to supersede *Race and Mortality* with respect to the issues addressed in *Race and Mortality* and then goes beyond it; and the *Chance* article does so with respect to absolute difference patterns (which is a key aspect of your article) and odds ratios (which, though only briefly discussed, your article offers as a solution). It was unfortunate that your piece was already submitted when the *Chance* piece came out (though in the Spring 2006 issue, it was not actually out until August). But there is a question whether it makes sense to finalize your article as a response to *Race and Mortality* (even with reference to the *Chance* article) rather than to make your piece more a response to the latter than the former.

I am, however, pleased that the current piece has been published, and would be happy to respond to it in something approximating its current form. But the process would be neater if the response is to a piece that recognized the treatments in the *Chance* article and other recent works (see item 11 below). (By that I do not mean to suggest that I would never address the instant piece, even if it is materially revised; any such work would be more about the processes of getting particular ideas accepted than the substantive question of how one sorts out meaningful patterns from those that are functions of prevalence.) Most of the points below, however, are directed to the piece as it currently stands.

4. To my mind, *Race and Mortality*, particularly when amplified by the three additional readings it referenced (items 2-4 after the signature below), provides a detailed rationale for the patterns it describes – said rationale, illustrated by reference to distributions of income or test scores, being that the patterns tend to flow naturally from the relationship of different, more or less normal, risk distributions. And, while you may disagree with that rationale, I question whether you should entirely ignore it in a work that directly challenges *Race and Mortality* but then, on finding patterns similar to those discussed in *Race and Mortality*, offers a different rationale for such

patterns. The recent, more detailed, articulations of the rationale Race and Mortality provide additional reasons not to ignore it.

5. At the conclusion, your article discusses the odds ratio as resolving the various problems in measurement previously discussed in the article, and also mention that it is the subject of a submitted piece (your reference 28). I know that Steven Walter – whose article you reference as number 27 (though I think you reference it mainly for the proposition that odds ratios are not so easy to understand) – is of the view that odds ratios solve the problems I raised in Race and Mortality (though I am not sure he has considered that position in light of the illustrations in the recent *Chance* article). But, as discussed repeatedly in the various works referenced in the preceding paragraphs (and as early as 1991 in note 1 of reference 2, and at some length in the 1992 paper), because odds ratios – like relative differences in experiencing and avoiding an outcome as well as absolute differences between rates – tend to change as prevalence changes, odds ratios do not solve the problem of distinguishing meaningful changes in inequalities from those that are simply the consequence of changes in prevalence. And even if you do not agree with any of the reasoning in these works, I would suggest that you run the same analyses illustrated in your figures with odds ratios and consider how the results bear on your point at the end. And, once again, inasmuch as there is now a substantial body of work out there attempting to explain why odds ratios do not solve the problems raised by the effects of prevalence on measures of difference, if I am mistaken in that regard, it would be useful to explain why.

6. Relying on the same Oslo presentation I sent to Dr. Kunst in 2001, in late 2005 a UK health inequalities measurement handbook authored by Roy Carr-Hill essentially accepted the reasoning underlying Race and Mortality. (See Carr-Hill R, Chalmers-Dixon P. The Public Health Observatory Handbook of Health Inequalities Measurement. Oxford: SEPHO; 2005: http://www.sepho.org.uk/extras/rch_handbook.aspx (accessed May 19, 2006).) This is discussed at page 13 of reference 6 below, as well as in references D-, 8, 10, 12, 22, 28, 33-35, and 38 on the previously mentioned web page. As discussed in reference 6 and a few of the other places, to my mind, the remainder of the lengthy work does not seem to recognize the implications of that acceptance with respect to the other measurement issues it addresses. But Dr. Carr-Hill's discussion of the matter (at 172) does reflect recognition of the way the patterns I described should be seen “as the simple consequence of the statistical distributions.” This would seem more reason for you to address the reasoning offered in Race and Mortality and elsewhere for the patterns it describes, even if you do not agree with the reasoning.

7. Your article gives a fair amount of attention to the way disparities in things like immunization tend to be measured in terms of the favorable outcome (the main basis for which I assume is the discussion of the same issue in Race and Mortality). In light of the attention given to the matter in your article, it would be useful for you to be aware that the US National Center for Health Statistics (NCHS), prompted by Race and Mortality and reference 3 below, has given a good deal of attention to the same issue in recent years. While recognizing that one might get different results depending on whether one examines relative differences in the favorable or the adverse outcome – though neither acknowledging nor addressing the claim that the two differences tend to move in opposite directions as the prevalence of an outcome changes – NCHS has simply recommended that all disparities be measured in terms of relative differences in adverse outcomes. As discussed in references 1 (at 49), 5, 6 (at 13), and 8, I think this is a sorely misguided approach.. Nevertheless, it is the approach of the US government, and one co-authored by some of the more prominent US commentators on measurement issues, and you might want to be aware of it in addressing this issue yourselves. The main document is:

Keppel K., Pamuk E., Lynch J., et al. Methodological issues in measuring health disparities. *Vital Health Stat* 2005;2 (141): http://www.cdc.gov/nchs/data/series/sr_02/sr02_141.pdf.

See also Keppel KG, Percy JN. Measuring relative disparities in terms of adverse events. *J Public Health Manag Pract* 2005;11(6):479–483 (to which reference 5 is a response).

(The principal NCHS statistician responsible for that approach, Kenneth Keppel, will be defending the approach on the same panel referenced in the first paragraph. Also, Adam Wagstaff will be addressing the subject of your reference 22. The discussant, Alan Zaslavsky, is the principal statistician for a group at Harvard Medical School that has done a great deal of work in measuring disparities in healthcare, typically measuring them in terms of absolute differences, and without regard to overall prevalence, as discussed in references 9-11.)

8. Regarding the mathematically defined ceilings on which you, in part, rely to explain certain observed patterns: I have occasionally seen discussions of a ceiling effect with regard to certain patterns that I explain rather differently. I have not give the matter great attention, but my understanding of the way that effect is generally characterized involves a notion that, as the rate of the advantaged group approaches 100%, it becomes difficult for such rate to increase very much and that the disadvantaged groups are thereby allowed to catch up. An example may be found in Chen et al. *Understanding Health Disparities: The Role of Race and Socioeconomic Status in Children's Health*. *Am J Pub Health* 2006;96-702-708. I have not attempted to review just how ceiling effects are more generally discussed. But it seems to me that the role of ceilings in your analysis may be something a little different. For, by dividing the population into halves, you, for example, necessarily make the mathematically defined ceiling close to zero when the overall prevalence is close to 100 or close to zero. But the situation is quite different when the groups compared are not each half the population. In the US, for example, the overall prevalence of some favorable outcome could approach 100% - let's say 90% - with said rate being largely a function of a very high white rate. Thus, there still could be a very large absolute (or relative) difference between the black and white rates of experiencing the outcome (say, black 60 and white 95). For various reasons, I would not expect there in fact to be such a difference. But I don't think the mathematically defined ceiling described in your article would have much role in the matter. Similar issues obtain for very low overall rates where the advantaged group is a small part of the overall population.

I do note that by dividing your population as you do, you avoid confusion over what one means by the "overall level," and there is some advantage in that. I recognize that my usage of terms like "overall prevalence" is inexact and tends to be broadly tied to rate of the advantaged group – and I from time to time feel a need to explain that inexactness (as in the end note to reference 10). Nevertheless, I suggest that you might want to consider the extent to which some of your reasoning about the role of the ceiling might be limited to the comparisons of top and bottom halves. Also, to the extent that your usage might be depart from a common understanding (and, again, I don't really know if that's the case), you might want to address that in some manner.

9. Regarding the diffusion of innovation theory offered as a rationale for the observed patterns when not accounted for by ceilings: First, the point of that discussion would seem the place where one would address competing rationales (whether or not to agree with them). Second, whereas the diffusion theory is certainly intuitively plausible enough, I suggest that you review reference 12 to see whether the theory in fact adds anything meaningful to expectations based on normal distributions. Further, I think that you'll find that – again by and large, though not in every case – that pretty much across the spectrum of the diffusion of something beneficial (except

for from day zero to day one, of course), the pattern will tend to be one of greater relative increases in favorable outcome rates for the disadvantaged group and greater relative declines in the unfavorable outcome rates for the advantaged group.

10. With respect to the observed reverse u-shape curve for two outcomes, but only the left side or right for two other outcomes, that your paper discusses at page 7, references 8-11 provide an explanation for such pattern, though, again, on the basis of a theory with which you might fundamentally disagree. See item 11.

11. Assuming you deal with the *Chance* article in some manner, there is one aspect to it of which you should be aware. The illustration in Figure 3 of the ratios of rates of experiencing the favorable outcome is based on the ratio of the rate of the disadvantaged group to that of the advantaged group. I did it that way because that is how it is done with respect to US employment discrimination issues (as discussed in note 1 of item 6). Subsequently, I changed the approach to one of presenting the favorable outcome ratios in terms of the success rate of the advantaged group to that of the disadvantaged group (as in figure 2 of item 6). And I then went on to put all the patterns on the same figure (as in references 8 and 11). It is the last approach that tends to reveal the way that the absolute difference tends to reach highs (and odds ratios lows) at approximately the intersection of the two ratios of experiencing or avoiding the outcome, as discussed in reference 8-11.

Perhaps, more than with respect to any other thing I say, your reaction to this analysis may be that there will be numerous departures from any such pattern. And, of course, there will be – for the reasons described in *Race and Mortality* and various other places. But, as your piece seems fully to recognize with regard to the patterns it identifies, the underlying tendencies will have enough of a role in every setting – or, if you will, potentially have enough of a role in a large enough number of settings – that it is pointless to try to analyze patterns of group differences in dichotomous variables without thinking about whether the tendencies are playing a role and trying to figure out the implications of such role. At any rate, with respect to some issues, the *Chance* piece has been somewhat superseded as well, and it might be wise to consider the subsequent descriptions of these patterns in addressing the *Chance* piece.

Further, reference 11 illustrates some aspects of the ways the patterns I describe may or may not exist in situations of irregular distributions. And it emphasizes the problems such situations raise for interpretation of patterns of changes. That is merely to say that, the fact there are departures from the patterns described in *Race and Mortality* is not a response to the problems it raises about the interpretation of group differences, but an exacerbation of those problems.

12. The following is a small point not particularly relevant to our areas of disagreement.

Presumptuous of me even to mention it. Nevertheless, it is the kind of thing I tend to address in commentaries that involve statistical methods, but which could be easily addressed in the finalized version of your article. At page 11, the article states:

“Although there is some uncertainty around the precise estimates for individual countries, it seems unlikely that this explains the systematic patterns observed.”

This is a more or less standard observation that studies make to the effect that data problems are unlikely to affect the study’s findings. And there of course can be many cases where, as with non-response bias for example (and as addressed, say, in the 2000 *BMJ* article on educational

differences in smoking on which Dr. Kunst and Mackenbach were authors), there are conceivable ways that data problems might lead to the identification of spurious patterns and hence a need to address such possibility. But here, it seems, even though you may not regard the tendencies underlying the patterns to be as strong as I do or agree with the reasons I offer for those patterns, the main finding of the article is that there indeed are certain patterns of correlations between overall prevalence and various measures of difference (indeed “systematic patterns”). And this occurs in circumstances where it would be absurd to think that those patterns could possibly arise from imprecise data. Rather, if problems with the data have any effect, such effect is likely to cause there to be more departures from the general pattern than would be observed with more precise estimates.

I do not mean to suggest that the patterns would be far more consistent with my theories but for the imprecision of the data. I have no idea how much of an impact the imprecision would have, and I think I have belabored enough that I expect to see many departures from the typical patterns. But I do think that in addressing the implications of the imprecision, your point ought not to be to refute a facially implausible suggestion that imprecise data may have caused the systematic patterns, but to point out that any imprecision in the data would more likely cause the patterns to appear less pronounced than otherwise would be the case.

In a related vein, I note that the child mortality RR of 1.01 for Kazakhstan and to a somewhat lesser degree the RR of 1.15 for Uzbekistan are sufficiently close to 1.0 to give one pause about the reliability of the data (even though bad data tend more often to suggest spurious difference than spurious sameness). And I note that there is much on the web about the revisions of child mortality estimates for these countries. Thus, you might want to consider examining the extent to which these estimates have held up before relying on them as your examples.

I apologize for the length of this note. Apart from some unnecessary repetition, I have strayed from commenting on the treatment of my article to uninvited commentary on all sorts of issues. But, inasmuch as I will probably write something about your piece, I find it useful to record my thinking in one place. And I would rather not later say too many things that I could have said here, especially when the issue is potentially addressable in a final version (though I will likely end up doing do that in any case).

Inasmuch as I will be discussing your article in the very near future, I would appreciate your letting me know as soon as possible whether you plan to address any of the above – or any other issues – in the final document and when you think that document will be posted.

Finally, I typically would write solely to the corresponding author on a matter like this. But given the prior exchanges with Dr. Kunst (and Dr. Mackenbach) it seemed to make more sense to include all authors.

Sincerely,
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1. Can we actually measure health disparities? *Chance* 2006;19(2):47-51:
http://www.jpscanlan.com/images/Can_We_Actually_Measure_Health_Disparities.pdf
2. Divining difference. *Chance* 1994;7(4):38-9,48:
http://jpscanlan.com/images/Divining_Difference.pdf.
3. Comment on "McLanahan, Sorensen, and Watson's 'Sex Differences in Poverty, 1950-1980.'" *Signs* 1991;16(2):409-13: http://www.jpscanlan.com/images/Signs_Comment.pdf
4. The perils of provocative statistics. *The Public Interest* 1991;102:3 14:
http://jpscanlan.com/images/The_Perils_of_Provocative_Stat.pdf
5. Measuring health disparities. *J Public Health Manag Pract* 2006;12(3):293-296 (responding to Keppel KG, Percy JN. Measuring relative disparities in terms of adverse events. *J Public Health Manag Pract* 2005;11(6):479-483:
http://www.nursingcenter.com/library/JournalArticle.asp?Article_ID=641470
6. The Misinterpretation of Health Inequalities in the United Kingdom, presented at the British Society for Populations Studies Conference 2006, Southampton, England, Sept. 18-20, 2006. http://www.jpscanlan.com/images/BSPS_2006_Complete_Paper.pdf
7. The Misinterpretation of Health Inequalities in Nordic Countries, presented at: 5th Nordic Health Promotion Research Conference, Esbjerg, Denmark, June 15-17, 2006:
http://www.jpscanlan.com/images/Esbjerg_Oral.pdf;
http://www.jpscanlan.com/images/Esbjerg_Presentation_Final.ppt
8. Measurement Problems in the National Healthcare Disparities Report, presented at American Public Health Association 135th Annual Meeting & Exposition, Washington, DC, Nov. 3-7, 2007: http://www.jpscanlan.com/images/ORAL_ANNOTATED.pdf;
http://www.jpscanlan.com/images/APHA_2007_Presentation.ppt
9. Effects of choice measure on determination of whether health care disparities are increasing or decreasing. *Journal Review* May 1, 2007, responding to Trivedi AN, Zaslavsky AM, Schneider EC, Ayanian JZ. Trends in the quality of care and racial disparities in Medicare managed care. *N Engl J Med* 2005;353:692-700 (and several other articles):
http://www.journalreview.org/view_pubmed_article.php?pmid=16107620&webenv=00P_2r_IHBKZPkExnEkCR_j5-u8waNcJ-87aLnoSJWxvN_ljFKstOR3CAx%402B600907661FF950_0034SID&qkey=1&rescnt=2&retstart=0&q=%22vaccarino+v%22+%22rathore+ss%22 (as corrected with respect to the discussion of AHRQ at:
http://www.journalreview.org/view_pubmed_article.php?pmid=16107620&specialty_id=
10. Understanding patterns of correlations between plan quality and different measures of healthcare disparities. *Journal Review* Aug. 30, 2007 (responding to Trivedi AN, Zaslavsky AM, Schneider EC, Ayanian JZ. Relationship between quality of care and racial disparities in Medicare health plans. *JAMA* 2006;296:1998-2004):
http://www.journalreview.org/view_pubmed_article.php?pmid=17062863&specialty_id=
11. Methodological Issues in Comparing the Size of Differences between Rates of Experiencing or Avoiding an Outcome in Different Settings, presented at the British Society for Populations

Studies Conference 2007, St. Andrews, Scotland, Sept. 11-13, 2007:
http://www.jpscanlan.com/images/2007_BSPS_Oral_Presentation.pdf;
http://www.jpscanlan.com/images/2007_BSPS_Presentation.ppt

12. Recognizing the statistical basis for advances in health care to cause larger relative reductions in mortality in groups with lower base rates. *Journal Review* June 9, 2007:
http://www.journalreview.org/view_pubmed_article.php?pmid=17213209&specialty_id=0&sdesc=&emsg=

Attachments: