Reply to JL Leroy et al

Dear Sir:

Leroy et al correctly point out that the interpretation of $z$ scores can be complex. Accordingly we feel that the best way to point out the errors of their criticism is to avoid further arcane academic arguments and highlight the real-life implications of the points that we raise in our article (1).

By using exactly the same numbers listed by Leroy et al from the Consortium of Health Oriented Research in Transitioning Societies data (2), combined with the WHO Child Growth Standards 50th percentile heights at age 2 y (86 cm for girls, 87 cm for boys) and CDC values for adults (164 cm for women and 177 cm for men; NB because the WHO growth curves do not extend to adulthood it is necessary to switch to CDC values), it can easily be shown that, even if the absolute deficit increases, the proportional deficit decreases in both sexes in every country (Table 1). By using the data that Leroy et al cite from our article for Gambian boys (1), the 8.5-cm difference at age 2 y is 9.8% lower than the reference 50th percentile value and has decreased to 4.8% by adulthood, thus halving the deficit.

To make an additional point about reproductive capacity, one of the critical functional outcomes associated with stunting, we will additionally show the changes in Gambian girls. At age 2 y, the Gambian girls whose adolescent growth trajectories were shown in Figure 6 of our article (1) averaged 3.6 cm below the UK 1990 reference $[-1.63$ height-for-age $z$ score (HAZ), a higher value than the older data in Figure 5 due to gradual secular improvements], and in adulthood $(-0.3$ HAZ), they are just 1.7 cm below the same reference. These represent deficits of 6.4% and 1.0%, respectively, thus virtually eliminating the original growth insults. Putting this another way, if we had flown an average 2-y-old Gambian girl, Kadijatu, to join a 100-strong white American toddler group in Washington, she would have been the fifth shortest child there (because a $-1.63$ HAZ equates to the 5.1 percentile). If we took Kadijatu back now as an adult and reunited her with the same 100 children, there would now be $\sim 40$ persons shorter than her. By any metrics, this is impressive catch-up.

In terms of her physiologic preparation for childbearing, if Kadijatu had remained at $-1.63$ HAZ she would have been 153 cm in adulthood as opposed to 164 cm: a difference of 7%. Given that both birth size and pelvic width (pw) correlate with maternal height [pw (mm) = 42 + 0.50 height (cm) (3)] and that there is a volumetric relation between fetal head diameter and brain volume, this additional height gain could allow an $\sim 15\%$ larger brain for the same risk of dystocia due to cephalopelvic disproportion, which is a very considerable advantage.

What is clear from our Gambian data is that children take a very different path toward achieving adult size than do their first-world counterparts. By extending childhood and adolescence, they can recapture much of the ground they lose in the first 2 y of life due to infectious insults. This may also be the case for other critical organ functions, especially the brain. As noted in our article, this recovery is achieved without external intervention, making the key point that there is plenty of residual plasticity after the first 1000 d that may be amenable to nutritional enhancement. We are not claiming that such effective catch-up occurs in all low-income settings; clearly it does not. But our data are sufficient to dispel the myth of life-long size entrainment by age 24 mo; it only takes one black sheep to prove that not all sheep are white.

In summary, our empirical data clearly support the arguments set out in our article.

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Andrew M Prentice
Landing M Jarjou
Sophie E Moore
Anthony J Fulford

MRC International Nutrition Group
London School of Hygiene & Tropical Medicine
Keppel Street
London, WC1E 7HT
United Kingdom
E-mail: andrew.prentice@lshtm.ac.uk

Kate Ward
Gail R Goldberg
Ann Prentice

MRC Human Nutrition Research
Elsie Widdowson Laboratory
Fulbourn Road
Cambridge, CB1 9NL
United Kingdom

REFERENCES

### TABLE 1

<table>
<thead>
<tr>
<th>Absolute height deficit</th>
<th>Proportional deficit against 50th percentile norm</th>
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<tbody>
<tr>
<td>Age 2 y Adult</td>
<td></td>
</tr>
<tr>
<td>Both</td>
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</tr>
<tr>
<td>cm</td>
<td>cm</td>
</tr>
<tr>
<td>Guatemala</td>
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<td>India</td>
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<tr>
<td>Philippines</td>
<td>7.9</td>
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1 COHORTS, Consortium of Health Oriented Research in Transitioning Societies.


ANTIHYPERTENSIVE MEDICATION AND THE EFFECTS OF BLACK TEA ON BLOOD PRESSURE VARIATION

Dear Sir:

In a recent study, “Black tea lowers the rate of blood pressure variation: a randomized controlled trial,” Hodgson et al (1) concluded that a component of black tea, other than caffeine, affects the rate of blood pressure variation during nighttime. The study was well conducted, but after reading, some questions remain.

Participants in this study were volunteers from the general population who were at risk of developing cardiovascular disease because of a systolic blood pressure of 115–150 mm Hg. Only participants without “a recorded history of current diabetes or a recorded history of metabolic diseases, chronic gastrointestinal disorders, cardiovascular, malignancies, renal disease or psychiatric disorders” were included. Participants who used >3 classes of antihypertensive medications were excluded. The 2 aspects in the following paragraphs remain unclear to us.

First, antihypertensive drugs lower blood pressure. Blood pressure without these drugs could be higher than 150 mm Hg. That is why we wondered whether this study population is still representative of a population with a systolic blood pressure of 115–150 mm Hg, because the study included participants who use antihypertensive drugs. It might be better to regard them as participants who are not at high risk of developing hypertension but who already have this condition. If participants using antihypertensive drugs were excluded from the analyses, would the results remain similar?

Second, Hodgson et al’s (1) Figure 1 shows that participants were excluded from the study because of a change in medication. Did these changes include antihypertensive drugs? Were these changes possibly due to changes in blood pressure? And what was the reason that these participants were excluded from the study?

We think more information on medication use would be useful for the interpretation of these results, and we are very interested in the authors’ thoughts on our questions.

No federal funds were received in support of this work. No benefits in any form have been or will be received from a commercial party related directly or indirectly to the subject of this letter. None of the authors declared a conflict of interest.

Manon L Zwinkels
Hevy Hassan
Evelien IT de Schepper

Department of General Practice
Erasmus MC
University Medical Center
PO Box 2040
3000 CA Rotterdam
Netherlands
E-mail: e.deschepper@erasmusmc.nl

REFERENCE


REPLY TO ML ZWINKELS ET AL

Dear Sir:

We investigated the effects of regular black tea consumption over 6 mo on ambulatory blood pressure (BP) (1) and the rate of BP variation (2). Higher BP variation has been related to increased risk of cardiovascular disease. The rate of measurement-to-measurement BP variation was assessed at baseline and again during the intervention at day 1 and at 3 and 6 mo. Tea compared with control resulted in lower 24-h ambulatory systolic and diastolic BP from 3 mo (1) and an immediate (from day 1) and sustained lower rate of systolic and diastolic BP variation during nighttime (2).

Zwinkels et al question whether the effects on BP variation are applicable to both individuals taking antihypertensive medication and those not taking these medications. The potential for a stronger effect of tea in treated hypertensive individuals, who have a higher rate of BP variation (3), may be relevant to population health messages for black tea intake.

Of the 92 participants, 23 were taking 1–3 antihypertensive medications. The treated hypertensive participants had a higher rate of systolic (P < 0.001) and diastolic (P = 0.005) BP variation compared with participants not taking antihypertensive medication. Three participants withdrew because of changes in antihypertensive medication during the intervention: 2 as a result of an increase in dose of medication, between the day 1 and 3-mo measurements (both were in the control group), and 1 because of a change in type of medication, between the 3-mo and 6-mo measurements (tea group). A change in BP medication is likely to significantly alter BP, the primary outcome of this study (1). The withdrawal of these 3 participants is unlikely to have influenced the interpretation of the results of this study.

Differences between tea and the control in the rate of systolic and diastolic BP variation were generally similar (~10%) among the treated hypertensive participants and the participants not taking antihypertensive medication. Among participants not taking antihypertensive medication (n = 69), tea compared with the control resulted in a lower rate of systolic (P = 0.038) and diastolic (P = 0.038) BP variation. Among the treated hypertensive participants (n = 23), tea compared with the control also resulted in a lower rate of systolic (P = 0.001) and diastolic (P = 0.064) BP variation. These results do not rule out the potential for differential effects according to type of antihypertensive medication. We were not able to investigate this further because of small numbers of participants in each of the subgroups. However, our results do indicate that the effects of tea may be relevant to individuals taking antihypertensive medication as well as those not taking these medications.

The current study was supported by grants from the National Health and Medical Research Council of Australia and Unilever Research and Development, Vlaardingen, Netherlands. Unilever is one of the world’s largest commercial suppliers of black tea. JMH was a named investigator on this grant. During the past 5 y JMH has received partial support from Unilever to attend and present at international meetings in Spain, the United Kingdom, and Malaysia and at national scientific meetings in Australia. JMH has also received payment to consult on an internal Unilever review on tea and health. GAH had no conflicts of interest to report.