Decaffeinated coffee and serum LDL-cholesterol concentrations

Dear Sir:

The role of coffee consumption in the etiology of atherosclerotic coronary artery disease has been the subject of a long debate easily encompassing the past 30 y. This debate predates the introduction of a variety of brewing methods and coffee preparations now in use. The recent report by Superko et al (1) focuses on one of these more recent preparations of coffee, namely decaffeinated coffee, and the concern that it contains substances that might raise serum low-density-lipoprotein (LDL) cholesterol.

This area of inquiry is notable for its inconsistency of findings. It seems clear that caffeine, despite its indictment by the popular media, is not responsible for cholesterol elevation (2–5). One cholesterol-raising component of boiled coffee, is, in fact, the lipid-soluble fraction (6). Moreover, other studies have failed to identify decaffeinated coffee as being worse than regular coffee (5, 7, 8). How then might the study by Superko et al differ from these other studies?

Several aspects of the design and interpretation of the Superko et al study need further clarification toward this end. The first deals with the issue of compliance. The study design dictated that continued drinking of subjects’ usual caffeinated coffee characterized the control group, with abstinence or change to decaffeinated coffee being the experimental groups. Thus, compliance with continued consumption of caffeinated coffee is assumed, but what was the compliance in the other two groups? Was the abstinence group able to abstain? Earlier studies of coffee consumption observed the need for subjects randomly assigned to abstinence to be allowed some form of hot beverage (9, 10). Were the decaffeinated-coffee subjects able to continue their usual consumption rate, or did their rate change up or down? Apparently, at least one subject was unable to continue. The degree of crossover between groups would be essential to the interpretation of study results.

Second, the magnitude and significance of differences between study groups may also be interpreted differently. LDL cholesterol decreased by 0.11 mmol/L in the caffeinated-coffee and no-coffee groups, but increased by 0.12 mmol/L in the decaffeinated-coffee group. However, none of these differences appear to be statistically different from the baseline concentration of LDL cholesterol. Most studies of coffee consumption compare the changes in lipid concentrations with baseline values rather than with another group’s values. The results suggest that caffeinated coffee or no coffee has as large a decrease from baseline values of LDL cholesterol associated with it, whereas decaffeinated coffee has an increase from baseline values associated with it. Thus, one is left wondering whether the results represent variation between groups around an average of no change. What is the cause for the decrease in LDL cholesterol in the caffeinated-coffee and no-coffee groups? Which is the significant trend? The interpretation provided is that consumption of decaffeinated coffee or no coffee causes an increase in LDL cholesterol. It seems as worthwhile to suggest that consumption of caffeinated coffee or no coffee causes a fall in LDL cholesterol. The 6% difference, then, is half accounted for by a rise in LDL cholesterol in the decaffeinated-coffee drinkers, and half by a fall in LDL cholesterol in the caffeinated- or no-coffee drinkers. It seems that consideration of alternate explanations would provide a less one-sided interpretation of findings.

Finally, the significance of decaffeinated coffee’s effect in decreasing lipoprotein lipase and increasing hepatic triglyceride lipase is unclear. This opposite effect on lipases is unique and interesting, with other drugs usually affecting only one or both in the same direction. A clinically significant decrease in lipoprotein lipase probably should be reflected in increased serum triglycerides. A significant increase in hepatic triglyceride lipase probably should cause an increase in the HDL₂-HDL₃ ratio. Neither were observed. Thus, it seems speculative that decaffeinated coffee’s effect is mediated by these enzymes.

The report by Superko et al correctly emphasizes the complexity of this topic. To the number of cups consumed, the brewing method, and possibly the type of coffee bean, the decaffeination process should be added as a possible factor for analysis. In this regard, the method of decaffeination used in the decaffeinated coffee for the study is worth describing. Usually, the decaffeination process suggests the removal of coffee constituents. What steps can be hypothesized that add a cholesterol-raising substance? Can the phenols in certain types of coffee beans explain the differences? What would the results be if the same coffee beans were used—one sample with caffeine and one sample without caffeine?

Any recommendation of one form of coffee over another on the grounds of health concerns, should await the confirmation, expansion, and explanation of the observations by Superko et al.

Thomas A. Pearson

The Mary Imogene Bassett Research Institute
One Atwell Road
Cooperstown, NY 13326-1394

References


---

Reply to TA Pearson

Dear Sir:

I would like to respond to the comments of Pearson for the authors.

First, our randomized trial indicates that caffeinated coffee has no effect on the lipoprotein variables measured in our study. This point was emphasized in the discussion section.

Second, Pearson addresses several trial-design issues. Trial design is the foundation of any good clinical investigation. Our trial involved a randomized design with many participants. The number of participants was determined by classic power calculations. The investigators and subjects were blinded as to the results of randomization into caffeinated- or decaffeinated-coffee groups. Certainly, those subjects placed in the no-coffee group were aware of their group assignment and the decaffeinated group was suspicious of their group assignment. Questionnaires revealed that ≈60% of subjects in the decaffeinated group were correct in their suspicion of assignment to this group. The trial design was developed over 2 y and underwent intense review by the National Institutes of Health (NIH) Study Section. We believe our investigation is the only US coffee investigation funded by the federal government and not by commercial interests.

Third, the issue of compliance is an important one. Compliance was determined by the difference in weight between the coffee dispensed and coffee returned at each visit. Compliance with no coffee ingestion was determined by questionnaire and diet records. There was no difference in compliance between groups. There was no crossover between groups.

Fourth, the statistical design of the study that was addressed in this NIH-funded study was to test for significance of change between groups. This is the purpose of having control groups in clinical trials. Testing for a change from baseline can be misleading because it does not take into account the multitude of variables that can alter lipoprotein measurements (1). The power calculations were performed to design a trial that could answer the main hypothesis. We believe that studies with smaller populations may lack the statistical power to reveal subtle lipoprotein differences induced by components of coffee.

H Robert Superko
Cholesterol Research Center
Lawrence Berkeley Laboratory
University of California
Berkeley, CA 94720

Reference

---

Coffee and lipoprotein cholesterol

Dear Sir:

The article by Superko et al (1) on coffee and lipoprotein cholesterol raises many questions requiring further explanation before the authors’ conclusions can be accepted with confidence.

1) From the data presented it is impossible to understand why, in Figure 1, the caffeinated-coffee drinkers showed a decline in low-density-lipoprotein-cholesterol (LDL-C). After all, they merely continued to drink regular coffee as before. With the no-coffee group at least one could take the position that the cessation of coffee drinking led to a decline in LDL-C.

2) Under Laboratory procedures it is clearly stated that LDL-C concentration was calculated. This is the usual practice. The formula for calculating the LDL-C concentration is as follows:

\[
LDL-C = \text{total cholesterol} - (\text{HDL-C} + \text{TG}/5)
\]

where C is cholesterol, HDL is high-density-lipoprotein cholesterol, and TG is triglyceride.

In Results it is stated that “No significant differences were observed for changes in concentration of plasma triglycerides, total cholesterol, HDL cholesterol, HDL₂ cholesterol, or apo-A-I for the decaffeinated-coffee group vs the control subjects.” If there were no differences in any of the components of the equation for the calculation of LDL-C, how could the calculated value have shown a significant increase?

3) In Table 1 (Baseline characteristics of the three groups) there are definite differences in the decaffeinated-coffee group as compared with the other two groups. Their TGs are higher, their total C concentrations are higher, their HDL-C values are lower, and their apolipoprotein B values are higher. Also, their carbohydrate intakes are lower. Admittedly, the differences are small, but it is the consistency of the differences that is surprising.

4) Cups per day are described as follows: “each cup equal to 237 mL of fluid coffee” as opposed to the generally accepted definition of a cup constituting 150 mL.

5) Statistical analyses were applied to group differences between baseline and end-of-study measurements instead of allowing each subject to be his own control.

6) Only two measurements of lipids were obtained: at entry and upon completion of the study. Thus, one does not know