CORRESPONDENCE

Plagiarism

To the Editor:

We wish to draw your attention to a serious case of plagiarism.

The article is titled "High Insulinogenic Nutrition—An Etiologic Factor for Obesity and the Metabolic Syndrome" by Wolfgang Kopp and was published in the July issue of *Metabolism*. It draws heavily on a similar hypothesis which we published in the journal *Diabetologia* in 1994. Yet, inexplicably, the latter paper is not cited at all.

I have enclosed both papers highlighting 21 separate instances of almost word-for-word plagiarism.

Moreover, the proposal that high glycemic index foods are involved in the etiology of the metabolic syndrome and obesity is one in which we have published extensively (see attached list of papers) and thus the idea is not novel.

We believe that this matter deserves an apology and a correction in the journal.

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Editor's Note

Drs Brand-Miller and Colagiuri submitted the above letter to *Metabolism* regarding the paper "High Insulinogenic Nutrition—An Etiologic Factor for Obesity and the Metabolic Syndrome" by Wolfgang Kopp, which was published in *Metabolism* 52:840–844, 2003. As is the custom when such letters are received, they are sent to the author of the paper in question with the request that a response be prepared and which would then be published in the "Letters to the Editor" section of the journal. The response from Dr Kopp is thus published with the letter from Drs Brand-Miller and Colagiuri. The paper by Drs Brand-Miller and Colagiuri, "The Carnivore Connection: Dietary Carbohydrate in the Evolution of NIDDM," was published in *Diabetologia* 37:1280–1286, 1994. Both of these papers postulated a critical role for the quantity and quality of dietary carbohydrate in the pathogenesis of obesity and insulin resistance in the paper by Dr Kopp and NIDDM in that of Brand-Miller and Colagiuri. While they both have relied on changes in nutrition going back in geologic time and human evolution, they differ in their postulation and interpretation of the mechanism which might be involved in explaining the current situation in regard to the prevalence of obesity, NIDDM, and the metabolic syndrome. Despite this difference, more than 20 of the statements and sentences in the paper by Dr Kopp are exactly or almost exactly the same as those in the paper by Brand-Miller and Colagiuri, but yet they were not attributed to that paper and it is not cited in the bibliography.

As Dr Kopp acknowledges in his reply, it would have been appropriate to include the paper by Brand-Miller and Colagiuri in the discussion of his paper since the subject matter was quite relevant. This omission is not as serious as the use of verbatim statements and sentences from a previous publication without proper citation. Although Dr Kopp indicates in his letter that he had planned to rephrase these sentences, in actual fact, he did not do it. Such plagiarism is not acceptable in the scientific literature. As a consequence, *Metabolism* will no longer consider any further manuscripts which are authored or coauthored by Dr Kopp.

James B. Field, MD
Editor-in-Chief

REPLY: High Insulinogenic Nutrition—An Etiologic Factor for Obesity and the Metabolic Syndrome

To the Editor:

When I received your letter last week and recognized the similarities of several sentences, I was so in shock that my first intention was to take the blame and get away from this awful situation. Now, after a couple of days, I finally was able to take a close look at the marked lines in both publications and I am able to answer your letter appropriately. The reason why I did not include the paper into my bibliography is this:

The pathomechanism proposed by Brand-Miller et al[1] is completely different from my hypothesis. Brand-Miller et al postulate that *insulin resistance is a genetic program* that has developed in our Paleolithic ancestors to cope with a shortage of dietary glucose. Brand-Miller et al propose that an insulin resistance genotype would have offered a survival advantage to specific populations consuming a low-carbohydrate, high-protein diet. They propose that the low carbohydrate nutrition of our Paleolithic ancestors has positively selected for individuals with insulin resistance. According to Brand-Miller et al, natural selection would therefore result in a high proportion of people with genetically determined insulin resistance.

In complete contrast to their hypothesis, I have proposed that insulin resistance (as well as obesity) develop as a result of high-carbohydrate nutrition: high-insulinogenic nutrition represents a chronic stimulus to the β cells that may induce an adaptive hypertrophy and a progressive dysregulation of the cells, resulting in postprandial hyperinsulinemia, and that *hyperinsulinemia promotes the development of insulin resistance* as well as of obesity. This definitely is in contrast to the hypothesis of Brand-Miller et al who have proposed a genetic selection for insulin resistance.

Brand-Miller et al hypothize that Europeons have the lowest incidence of diabetes because the selection pressure for genes producing insulin resistance was relaxed first in Caucasians as a result of the increasing amount of dietary carbohydrate after the agricultural revolution. They expect the prevalence of genes producing insulin resistance to be lower in the European population and any other group exposed to high carbohydrate intake for sufficiently long. They further hypothise that ethnic groups have a very high incidence in diabetes, because they are (still) genetically insulin-resistant, and their β cells are not able to secrete enough insulin to overcome the insulin resistance.

In contrast, I have proposed that ethnic groups have a high incidence in obesity and diabetes, because their β cells are more susceptible to hypertrophy and dysregulation due to high-insulinogenic nutrition, while Europeans have lower rates, because their β cells have become less susceptible.

Thus, these are two completely different hypotheses, having in common only dietary changes in human evolution (facts that are common knowledge). I did not see the need to cite a hypothesis that is completely different from mine. Though, I probably should have put this into the discussion. But to the best of my knowledge the pathomechanism I have proposed has not been published before elsewhere.

The fact that numerous formulations and sentences are similar has several reasons. First, I would like to state that there are similarities, because a part of both papers deals with the same topic: Paleolithic nutrition and nutritional changes due to the introduction of agriculture, and that several of the sentences are cited almost verbatim from the same reference (Garn: "What did our ancestors eat.")[2]—by Brand-Miller et al as well as by me (like: "Neanderthals were cold-climate hunters of large game and subsisted primarily on game during the coldest periods," "Homo erectus was a hunter"). Some phrases like "postulate a critical role," or "lines of evidence" I have used intentionally because I thought that these were typical American English phrases.

Another reason for the similarities is that I have made a mistake. When I read the publication of Brand-Miller et al, I found their hypothesis to be different from what I wanted to propose but I found some of the facts concerning the dietary changes in human nutrition very helpful in the article and therefore extracted them, intending to rephrase them later. But after some time, going through the manuscript again and again, the words began to sound more and more familiar to me. I simply have forgotten about the fact that I had taken these lines from their publication. I did not intentionally use these sentences; this obviously would be a really stupid thing to do, that must be revealed immediately (which has been the case), and I certainly am capable of writing a paper by using my own formulations. So, I can only say that I am really sorry for the mistake I have made and that I am sorry for not having cited the publication of Brand-Miller et al and discussed the differences.

Wolfgang Kopp

From the Diagnostikzentrum Graz, Graz, Austria.

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REFERENCES

1. Brand Miller JC, Colagiuri S: The carnivore connection: Dietary carbohydrate in the evolution of NIDDM. Diabetologia 37:1280-1286, 1994

2. Garn SM: What did our ancestors eat? Nutr Rev 47:337-345, 1989

Inflammation and Pathogenesis of Diabetic Nephropathy

To the Editor:

We have read with great interest the article by Moriwaki et al[1] published in the May 2003 issue of *Metabolism.* In this study, the authors analyzed the serum levels of the pro-inflammatory cytokines interleukin-18 (IL-18) and tumor necrosis factor-alpha (TNF-α) in type 2 diabetes mellitus patients. They found that serum IL-18 and TNF-α levels were increased in these patients, especially in those with nephropathy. In spite of these findings, they concluded that the cause-effect relationship between IL-18 or TNF-α and type 2 diabetes mellitus or diabetic nephropathy remain undetermined. However, the results by Moriwaki et al are of relevance.

Diabetic nephropathy, especially in the context of type 2 diabetes, has become the principal cause of renal failure, with renal disease as a major cause of morbidity and mortality in diabetic population. Metabolic and hemodynamic factors have been classically considered as the responsible for the development of renal lesions in patients with type 2 diabetes mellitus. However, nowadays, the factors determining the pathogenesis of diabetic nephropathy appears incomplete.

Recent studies have shown that chronic subclinical inflammation is an essential component of the insulin resistance syndrome.[2,3] Moreover, the findings of increased plasma levels of inflammatory parameters, including C-reactive protein (CRP), sialic acid, fibrinogen, interleukin-1, interleukin-6, and TNF-α, have led to the conclusion that type 2 diabetes includes an inflammatory component.[4-8]

Concerning the relationship between inflammation and nephropathy in type 2 diabetes mellitus, previous studies have reported that serum levels of pro-inflammatory markers are greater in patients with increased urinary albumin excretion with respect to normoalbuminuric diabetic subjects.[5,9] In a previous study, we showed a significant and positive relationship between serum TNF-α levels and urinary protein excretion in type 2 diabetic patients with overt nephropathy and chronic renal failure.[10] Furthermore, the Insulin Resistance Atherosclerosis Study has recently demonstrated a significant association between inflammatory markers (CRP and fibrinogen) with urinary albumin excretion in type 2 diabetic patients with microalbuminuria.[9]

Finally, we have just performed a study in type 2 diabetic patients to test the hypothesis that inflammatory parameters are independently associated to urinary albumin excretion.[11] In this study, only type 2 diabetic patients without potential confounding factors, including acute illness, severe proteinuria (urinary protein excretion $>$ 1 g/d), hypertension (defined as a systolic blood pressure $\geq$ 140 mm Hg and/or diastolic blood pressure $\geq$ 90 mm Hg), renal insufficiency (defined as a serum creatinine level $>$ 1.3 mg/DL), cigarette smoking, treatment with aspirin or statins, and past medical history of clinical cardiovascular disease (cardiac, cerebral, or peripheral vascular disease) were included. Our results show that urinary TNF-α levels are increased in type 2 diabetic patients with respect to normal controls, and further-