Mr. Townsend should know, by now, that the incompetent-perforating-vein concept, otherwise known as the St. Thomas's Hospital theory, or the "ankle-blow-out" syndrome, as *the* cause of chronic leg ulceration, has long since been "blown". In fact, despite the enthusiastic paper in 1955,[2] when the hypothesis was first advanced, there has, to date, never been a *published* follow-up of the original series.

It would sadden me to think (vide Mr. Townsend's letter) that ever since I first became interested in leg ulcers in 1950, my treatment has been both "inadequate in conception and faulty in execution", did I not realise that Mr. Townsend's sweeping didacticism only emphasises the great and, I fear, almost unbridgeable chasm which separates the general surgeon from the specialist in the treatment of venous diseases of the lower limb. Let us hope that, in the bright new decade of the 1970s, some effort will be made to bridge this gap.

London N.W.1. STANLEY RIVLIN.

INSULIN STIMULATION OF CHOLESTEROL SYNTHESIS BY ARTERIAL TISSUE

SIR,—I refer to the article by Dr. Stout, published last August.[3] What evidence has the author that some of the precursor, [^{14}C]-1-acetate, has not been left in the chloroform/methanol extract and travelled with cholesterol on thin-layer chromatography? Alternatively, diglyceride or monoglyceride, or some other sterol travelling with cholesterol may have been the lipid whose synthesis was increased, rather than cholesterol. Also, Dr. Stout does not make it clear whether he means that total cholesterol, free cholesterol, or cholesterol ester synthesis was increased. If it was total cholesterol or cholesterol ester, then the increase may have been in fatty-acid synthesis and/or esterification with cholesterol rather than in synthesis of cholesterol itself.

The form in which [^{14}C]-4-cholesterol was injected intravenously was not physiological. If it associated with plasma-lipoprotein, then at 1 hour after injection it may have been associated only in the free form.[4] The handling of free and ester cholesterol by the arterial wall is known to be different.[5] There is no evidence at the moment that insulin increases cholesterol synthesis from [^{14}C]-1-acetate in arterial tissue in vitro.[6]

The conclusion that insulin increases cholesterol synthesis in the arterial wall appears to be based on insufficient evidence.

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*** This letter was shown to Dr. Stout, whose reply follows.—ED. L.

SIR,—The chloroform-methanol extract of aortic lipid was prepared and washed with calcium chloride solution, by the method of Folch et al.[7] These workers have shown that this method removes at least 99·9% of water-soluble radioactive precursors, including sodium acetate, from the lipid extract. Significant contamination is therefore unlikely.

The cholesterol fraction studied was free cholesterol. Insignificant radioactivity was found in the cholesterol-ester fraction—a finding similar to that of Rao and Rao[8] in in-vitro studies. It therefore seemed reasonable to use serum-free-cholesterol to study the origin of this lipid in the aortic wall. On the thin-layer plate, the cholesterol fraction occurred as a discrete band enclosed, on either side, by a zone which neither stained with iodine nor contained any radioactivity. This band corresponded with the cholesterol of the standard solution which was run on each plate, and can therefore be assumed to represent aortic free cholesterol.

It is difficult to comment on unpublished observations. However, the inability to demonstrate an action of insulin on aortic cholesterol synthesis in in-vitro experiments, which are of course highly unphysiological, does not invalidate the results of studies in vivo. It is well known that arterial tissue does not respond to insulin in vitro but does so in vivo.[9]

I agree that further work on this subject is essential. I feel, however, that my observations are valid, and, in view of the epidemiological evidence,[10] that they are of potential significance in the pathogenesis of atherosclerosis.

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MAN AND HIS PESTICIDES

SIR,—The assertion in your leading article with the title (Dec. 27, p. 1406) that no sign has been found that D.D.T. has reduced populations of peregrines and other birds, is in fact dead wrong—as a large and growing research literature shows.[11–14] In fact, D.D.T.—and especially its metabolite D.D.E.—severely compromise reproductive function in these birds, resulting in population decline and, in some species in some areas, extinction. The U.S. Government commission whose report is the basis for the announced U.S. ban on D.D.T. explicitly declares: "These experiments appear to forge the last link in the chain of evidence that D.D.T. and its derivatives have been the direct and principal cause of widespread and significant reductions in bird populations. The full extent of the damage cannot yet be determined."[15]

New York,
N.Y. 10025. DAVID R. ZIMMERMAN.

*** Paragraph 275 of the report[16] on which our leader was based reads:

"Observations in Great Britain and North America recently revealed that a decrease in eggshell thickness and an increase in egg breakage by parent birds, especially among predators, occurred in the late 1940s, about the time that D.D.T. was introduced commercially on a substantial scale. Some decrease in the success of rearing young occurred about that time but the populations of peregrine falcons and sparrowhawks remained steady until about 1955, when a steep decline was attributed to dieldrin. There is other evidence of D.D.T. affecting eggshell thickness, egg breakage, and breeding success, but none that relates to reduced populations. Nevertheless, it might hinder the recovery of a population reduced by other causes."—ED. L.

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