



Now you can use self-lubricating TEFLON® STOPCOCKS on an everyday basis!

**Kontes saves you money
three ways on chemically-
inert* Teflon items!**

Increased production has lowered prices substantially on all Kontes Teflon Stopcocks and apparatus which incorporate them. You save, in fact, three ways by getting Teflon products from Kontes: by reduced individual prices, by lower quantity prices, and by combining Teflon items with your regular orders for Kontes Technical Glassware for even greater discounts.

COMPARE KONTES QUALITY! Teflon Stopcock barrels are ground and polished, providing the ultimate in smooth, even turning without lubricant. Special spring-loading device keeps plug properly seated and leak-free without dangers inherent in over-tightening. Wall thickness is appreciably greater than other brands. Sidearms increase gradually in o.d. and decrease in i.d. at seals to barrels for greater strength and improved flow characteristics.

Order now for immediate delivery! Write for your free copy of our new Bulletin TP-1. It describes all Kontes Teflon ware including beakers, burettes, pipettes, separatory funnels, stopcocks, etc.

K-81100 STRAIGHT STOPCOCK
prices below are typical—

Size	Bore, mm.	Plug size, mm.	Each
2 A	2	10/25	4.50
3 A	3	10/25	4.50
2	2	12/30	6.90
4	4	17/40	9.20

*Teflon has almost universal chemical inertness.
©Reg. T.M., E. I. du Pont de Nemours & Co., Inc.



**KONTES
GLASS COMPANY**

First Choice For Quality Technical Glassware
Vineland, New Jersey

Midwest Distributor: Research Apparatus, Inc., Wauconda, Ill.

Letters

Protein Behavior

In a recent article [*Science* **128**, 815 (1958)], I. M. Klotz admirably points up the fact that electrostatic interactions involving proteins and small ions have generally had to be treated in an oversimplified way, and he calls attention to the many observations which would suggest that the properties of the water in the immediate vicinity of the protein molecule may be rather different from those of the water in the bulk solvent. His proposal regarding the effect of an ice structure would explain the greater difficulty of protonation of an $-\text{N}(\text{CH}_3)_2$ group but, it appears, would still not solve the problem of reduced reactivity of the $-\text{SH}$ group with Ag^+ , with which he initiated the discussion. In this case, of course, there would be no change in the charge to modify the structure of the ice lattice.

The proposal seems even more troublesome when one considers the $-\text{COOH}$ groups in the protein. In this case a charge appears on the group when the hydrogen ion is lost. Consequently, the loss of the proton would be hindered, and the apparent pK should be increased as compared with the expected intrinsic pK . There seems to be little evidence for any considerable number of $-\text{COOH}$ groups with unexpectedly high pK 's in proteins.

The lack of any evidence for electrostatic effects on the titration curve of the azomercurial would still be disturbing, however—unless one assumes that the reaction with hydrogen ion is leading to displacement of the azomercurial. If this were true, there would be no change in the charge, and one would not anticipate an electrostatic effect. Such a displacement of methylmercury would have been anticipated on the basis of the results of Hughes [*Cold Spring Harbor Symposia Quant. Biol.* **14**, 79 (1949)].

JOHN W. MEHL
*University of Southern California,
Los Angeles*

Each of the three paragraphs of Mehl's letter raises essentially one question. The responses may be grouped, therefore, into the same arrangement.

1) "Maskedness" in the behavior of $-\text{SH}$ groups, as Mehl would undoubtedly agree, is a problem of rates rather than equilibria. For example, Ag^+ is usually taken up even by masked $-\text{SH}$ groups of proteins if we wait long enough. In terms of my model, the explanation (clearly implied if not explicitly stated in the article) is that Ag^+ would diffuse through "ice" much more slowly than through liquid water. While no actual data are available for diffusion

of Ag^+ in ice, measurements with a similar monovalent ion, Li^+ [M. Eigen and L. DeMaeyer, *Proc. Roy. Soc. (London)* **247A**, 505 (1958)], certainly bear this point out.

2) The most careful theoretical analyses of the titration curves of a protein with, for example, 100 carboxyl groups have limited themselves to a single intrinsic dissociation constant. The spread of the titration curves has been accounted for by the assumption that there is a variable electrostatic factor, plus necessary additional assumptions in specific cases. Clearly, if deviations from ideality are attributed to these additional factors, and if we permit the possibility of only a single intrinsic constant, we cannot possibly find more than one intrinsic constant. As I have emphasized, however, the titration curves can also be accounted for by the assumption that there is a broad spectrum of pK 's for the carboxyl groups.

3) The explanation suggested by Mehl is not really tenable. We have not studied the titration curve by some general method which follows gross H^+ ion uptake but rather by a special spectrophotometric method which reveals specifically the uptake of H^+ by the $(\text{CH}_3)_2\text{N}-$ group of the dye, not by the mercaptide group of the protein. It is the unusual pK and the unexpected shape of the curve for the optical titration of this particular $(\text{CH}_3)_2\text{N}-$ group which must be interpreted; a displacement mechanism postulates changes in state of a different group at the opposite end of the molecule. Furthermore, one should also recall that if the dye had been displaced from the mercaptide linkage, no optical titration could have been obtained in the first place, for as we have mentioned previously [*Arch. Biochem. Biophys.* **63**, 77 (1956)], the dye is essentially insoluble in water alone.

I. M. KLOTZ

*Northwestern University,
Evanston, Illinois*

On Eschewing Teleology

A. J. Bernatowicz' stern admonitions [*Science* **128**, 1402 (1958)] to his biological colleagues and to all other scientists to eschew anthropomorphism and avoid even the appearance of teleological thinking (lest their students be not saved from corruption) confuses me. He wants mechanism, and thus also surely determinism, recognized as the language of science, and he wants the teacher of science not to depart from it. Does he mean *never*? Not with the student at the luncheon table? Not with his wife at the breakfast table? How rigorously must righteousness be applied? And must all scientists observe the canon? I think