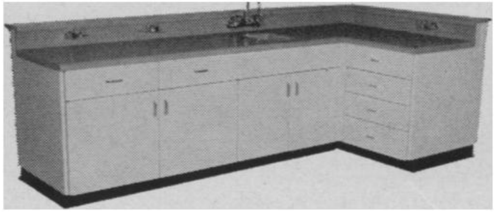
But are there really two approaches? Only by defining Watt's engine as the first "true" one can one transform Wetherington's structural statement into "a fact of history." Even with that definition, one would have difficulty justifying the date 1769 as the year of the engine's development. The sort of statement which Wetherington employs to illustrate the structural approach is, more often than not, simply a mistake. On the other hand, to say that discoveries normally evolve with time is not to say that individuals have a negligible role in their evolution. Nor is it to say that the evolution of each and every discovery must be studied against the entire cultural ambiance of the day.

Given any particular discovery, it is a historical problem—perhaps *the* historical problem—to discover whether and how the individual interacts with his milieu, and with which parts of the milieu he interacts, in the evolution of novelty. There is no single answer. Studying different discoveries, the historian will find many different sorts and degrees of dependence. But it is the discovery that varies, not the method of study. Call that structural, functional, neither, or both, the historian can neglect the individual or his milieu only after research has shown that one or the other played a negligible role in the development of the episode that concerns him. Structure and function will emerge together from that research. They are not to be had separately.

THOMAS S. KUHN
University of California, Berkeley

Interpretation of Cation-Exchange Mechanisms

In their report, "Course of cation absorption by plant tissue" [*Science* **136**, 1051 (1962)], E. Epstein, D. W. Rains, and W. E. Schmid discuss in terms of an "enzyme-kinetic" model experimental data of constant absorption rates depending exponentially on temperature. The authors conclude: (i) the overall mechanism of cation absorption (not further defined) possesses a "high degree of irreversibility"; (ii) absorption is metabolically mediated; (iii) the cation-exchange capacity (labeled general, nonselective) is largely satisfied by calcium under physiological conditions; (iv) the time course (the rate?) of the absorption of monovalent cations under these conditions is precisely like that of anions; (v) "overt, nonselective cat-



ion exchange" plays no role; and (vi) there is no evidence of "measurable instantaneous absorption" (zero time intercepts) ostensibly reflecting specific ion-carrying sites.

I shall pass over the fact that clarification of some highly individual expressions used by the authors is needed, to point out that it is difficult to see the logic of drawing such sweeping generalizations from the 12 data points (from two absorption experiments, at 4.5° and 30°C) presented. On a strictly thermodynamical basis the only result seems to be establishment of constant diffusion rates for rubidium from outside solution to interior phase of the tissue, varying with temperature.

In particular, no irreversible nature of the absorption process (conclusion i) is proved by constant reaction rates when a rate-limiting diffusion process may certainly be present. It should be valuable to follow the process up to the inevitable diminution of absorption rate and final saturation; infinite capacity of the interior phase cannot be postulated. The "enzyme-kinetic" model referred to is thermodynamically and kinetically identical to the mechanism of ion transport across an ion-exchange membrane; therefore, the possibility that this is simple nonmetabolic transport cannot be excluded until conclusion ii is supported by actual correlation with metabolic measurements. The analogy with enzyme mechanics depends also on proof of conclusions i and ii. Conclusion iii follows from the laws of ion exchange and mass action, while no evidence is presented in the report itself concerning conclusions iv and v. There is no need to fight "instantaneous absorption" (conclusion vi), which does not exist in chemistry. This should not be equated with zero time intercepts drawn on paper, which will result in all cases of initial higher absorption rate (a phenomenon quite usual in ion exchange and physical absorption and reflecting not so much differences in concentration as differences in environment and reactivity of exchange sites).

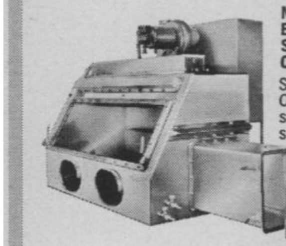
Diffusion, selective ion exchange, and aging processes are not even mentioned in the discussion of these experiments on the diffusion of a rather special ion in biologic material, though these factors combined may actually confer a "degree of irreversibility" to the process.

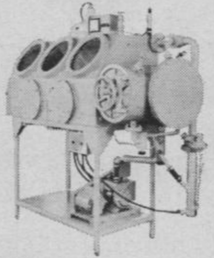
I wish to enter a strong plea against the use of well-defined terms like *irreversibility* and *equilibration* without close regard to their actual meaning; it should be completely clear that an irreversible process can never reach equilibrium.

PAUL S. ANTAL
Institute of Marine Science,
University of Miami, Miami, Florida

With respect to conclusions i and ii, the enzyme-kinetic model of ion transport by carriers is not thermodynamically and kinetically identical with the mechanism of transport across an ion-exchange membrane. Both rubidium and chloride are transported, at about equal rates, and accumulate as free ions in an interior aqueous phase at concentrations far in excess of their external concentrations. These well-documented facts imply that metabolic energy is expended, and indeed transport is inhibited by factors that interfere with normal metabolism, including low temperatures. Simple nonmetabolic transport is therefore excluded.

Diffusion and even isotopic exchange across the osmotic barrier are extremely sluggish as compared with net rates





of absorption. Simple diffusion and ion-exchange mechanisms are inadequate for this and other reasons, and a carrier mechanism analogous to enzyme mechanisms has been proposed not by way of "proof" of anything, but as a working hypothesis or conceptual model. According to this model, the rate of absorption would be expected to decline as the accumulation ratio rises with time if we were dealing with a readily reversible process. Instead, the rate remains constant.

As for conclusions iii, iv, and v, the contrast between the time course of the absorption of cations and that of anions has been known since the 1920's; in the absorption of cations there is a marked initial, rapid phase of uptake representing cation exchange, while anion exchange is negligible. In our experiment, the simple cation-exchange uptake of rubidium was abolished by the presence of calcium, as evidenced by the absence of any measurable rapid initial phase of uptake, and the time course of rubidium absorption was like that of anion absorption.

As for conclusion vi, saturation of the active transport mechanism (carriers) occurs without a measurable time lag after immersion of the tissue in a solution: the process is "instantaneous" (Webster defines *instantaneous* as "occurring . . . without any perceptible duration of time"). In our experiment, a perceptible lag would show up as a negative intercept on the *Y* axis.

We are not acquainted with any theory of biological ion transport combining "diffusion, selective ion exchange, and aging processes." Selectivity was not discussed in our report because the experiment did not deal with it. However, selective ion binding is an integral part of the enzyme-kinetic model of ion transport by carriers, and much evidence on selectivity has been published.

Rubidium is indeed a "rather special ion" in biological material, but is Antal unaware of its extensive use in studies of biological ion transport? This and other well-known facts mentioned here were not elaborated upon in our report, which, like other short technical reports, dealt with certain specific points and was not meant to be a primer on the entire subject.

EMANUEL EPSTEIN
D. W. RAINS
WALTER E. SCHMID

Department of Soils and Plant Nutrition, University of California, Davis