

178. *Mechanism of Substitution at a Saturated Carbon Atom. Part XIII. Mechanisms operative in the Hydrolysis of Methyl, Ethyl, isoPropyl, and tert.-Butyl Bromides in Aqueous Solutions.*

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Contrary to versions of our theory published by Taylor (1937 and 1938), the variation of rate with structure in such a series as that indicated in the title should be linked with mechanism of substitution, and not with reaction order, where through the buffering of a reagent concentration, the reaction order cannot change when the mechanism changes. The theoretical connexion with mechanism is S_N2 :—Me > Et > Pr^β > Bu^γ and S_N1 :—Me < Et < Pr^β < Bu^γ.

In alkaline aqueous solutions the mechanisms are associated with different reaction orders, and rate inequalities consistent with the above can be demonstrated by reference to reaction order. A more systematic experimental demonstration has been given of this than was available heretofore. In acidic aqueous solution the water concentration is buffered, and all reactions are in close approximation of first order; but the rate variation, which also has been illustrated, could be understood in the light of the above two sequences if we may assume that the bimolecular mechanism operates at the beginning of the series and the unimolecular at the end, with an overlapping of mechanisms at the *isopropyl* member, just as Gleave, Hughes, and Ingold originally demonstrated (1935) for the decomposition of sulphonium salts. That we may make this assumption is rendered probable by the consistent indication of several different sources of evidence, *viz.*, (1) the sensitivity of the reactions of the earlier members of the series, and the insensitivity in the later members, to acceleration by powerful bases such as OH⁻; (2) the relative insensitivity in the earlier members, and sensitivity in the later, of the reaction rates to solvent changes; (3) salt effects on reaction rate; (4) the stereochemical criterion. Tests (1) and (2) are illustrated in this paper. The salt effects characteristic of the unimolecular mechanism are reported in accompanying papers. The stereochemical criterion has to be applied by elaborating the alkyl series so that it can include optically active compounds, and then noting where a change occurs in the stereochemical result of substitution: the necessary observations are in previous papers (1937). All this evidence coheres, as does the larger whole from which it is taken, and neither Taylor nor anyone else who has objected to the ionisation mechanism has succeeded in effecting such a synthesis on any other basis.

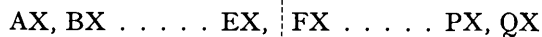
[Added 20.1.40.] In an addendum (section 1a), two further, recent papers are criticised on the grounds that Taylor's methods and data are incorrect, that widely divergent data are published in different places without adequate cross-reference or withdrawal of invalidated conclusions, that erroneous methods of calculation are employed, and that (as with papers elsewhere criticised) demonstrably unjustified conclusions are supported by numerical agreements which cannot at present be explained in view of the errors made in their derivation.

(1) *Comments on Two Papers by W. Taylor.*

THIS paper contributes to the subject of structural effects on nucleophilic substitutions in the analogous forms, MeX, EtX, Pr^βX, Bu^γX. But it seems necessary to preface the description of results (Section 2) with a brief, recapitulatory, theoretical discussion, for the

reason that, subsequently to our original publications on the subject (Hughes, Ingold, and Patel, J., 1933, 526; Hughes and Ingold, J., 1935, 244), Taylor has twice (papers 3 and 8; references, this vol., p. 900) misrepresented our views, in mutually contradictory ways which have been found highly confusing by other investigators.

We commence by supposing that for some one nucleophilic substitution under fixed conditions of reagent-concentration, solvent and temperature, it is found by kinetic observation that a change of reaction order occurs in the neighbourhood of the dotted line in the series



the change being in principle gradual. The alkyl groups A, B . . . Q, are supposed to be arranged in order of increasing electron release. We shall find second-order reactions predominating increasingly towards the left, and first-order towards the right, of the transition region. Our conclusion will then be that the mechanism is bimolecular (S_N2) when the reaction order is second, and unimolecular (S_N1) when it is first; and on theoretical grounds we shall expect each mechanism to have a characteristic structural effect on the reaction rate, as follows. For mechanism S_N2 the rate will fall slowly, possibly with small irregularities, along the series (in some cases the "curve" may flatten out and even rise slightly, near the transition region—cf. Hughes, Ingold, and Shapiro, J., 1936, 225); and for mechanism S_N1 the rate will rise rapidly from the point at which this mechanism takes control. Now, of course, we can only expect to make observations of kinetic order, locating the transition region, if both interacting species are kept in small and controllable concentrations. If the concentration of one reagent is "buffered" by making it many times larger than that of the other, all our reactions will, from an observational point of view, be necessarily of first order, and the observed order will then give no information concerning mechanism.* For the solution of the mechanistic problem we shall now be thrown back on other less direct methods. If the variation of rate with structure is such as we should expect from a change of mechanism in some particular position in the series, that fact itself might be taken as evidence; but we should desire confirmation.

Taylor's paper 3, the first of two dealing with the relative rates of hydrolysis of the bromides MeBr, EtBr, Pr^{*β*}Br and Bu^{*γ*}Br in neutral or acid aqueous solvents, opens with the following passage, in reading which it must be noted that he was using "unimolecular" and "bimolecular" in the experimental sense for which we reserve the terms "first order" and "second order": "According to Hughes and Ingold, for the hydrolysis and alcoholysis of alkyl halides in the series where Alk = Bu^{*γ*}, Pr^{*β*}, Et, Me, the velocity of the unimolecular" [*i.e.*, first order] "reaction should continuously decrease, and this reaction should, in fact, be unrealisable for Et and Me. Neither of these deductions, which these authors make from their ionisation theory, can be upheld, since it is now found that, not only do both methyl and ethyl bromide suffer unimolecular" [first order] "reaction in aqueous alcohol, but the velocities are in the order Bu^{*γ*} ≫ Me > Pr^{*β*} > Et. As this result is not in agreement with their theory, it casts doubt on their assumption . . ." that certain other reactions, which Taylor specifies, have a duplex mechanism like hydrolysis (the passage is too long to quote in full). Our comment on this must be that there has been a misunderstanding: Hughes and Ingold did not make either of the two deductions imputed to them. In particular, they did not claim that, when one reagent is taken in such large excess that the reaction is experimentally of the first order whatever the mechanism, then that first-order rate would decrease in the series Bu^{*γ*} > Pr^{*β*} > Et > Me independently of mechanism; on the contrary, the position was (and is) that, *if* there is a change of mechanism (which has to be proved), the rate should go through a minimum (which it does). Neither did Hughes and Ingold claim that the first-order reactions of methyl and ethyl halides would be unrealisable under conditions in which no other reaction order can possibly be realised. They did suggest that, where a bimolecular mechanism appears as a fast second-order reaction, it would be difficult to observe a simultaneous, slow first-order reaction representing a unimolecular mechanism; but, of course, if the bimolecular

* We shall show in accompanying papers (this vol., p. 960, *et seq.*) that this is only a rough truth, the deviations from which are of great importance.

mechanism is *made* to give a first-order reaction by buffering one of the concentrations, one naturally observes a first-order reaction. In view of Taylor's misconception we will repeat (because it is just as true in the light of our much wider knowledge of to-day as when we advanced the theory) that, whenever either kinetic evidence, or, when that is unavailable, consistent indirect evidence can be adduced to show that a reaction is unimolecular, then rates conforming to such series as $\text{Bu}^\gamma > \text{Pr}^\beta > \text{Et} > \text{Me}$ are invariably observed. This statement, already extensively illustrated, is further exemplified in a number of different ways in the present group of papers (*e.g.*, this vol., p. 945).

Taylor's paper appeared as one of us was passing proofs for this Journal; in which, therefore, a footnote was inserted (J., 1937, 1187) protesting against his travesty of our theory and briefly remarking that reaction order and mechanism are not synonymous. The point appears to have been noted, because some months later Taylor returned to the hydrolysis of the same four halides in acidic aqueous solvents (paper 8); and, without directing attention to his previous version of our views, allowed us an entirely different, but equally incorrect, set of conclusions. In this paper it appears that "according to these authors" (Hughes and Ingold) "the second-order hydrolysis velocity coefficients should be $\text{Me} > \text{Et} > \text{Pr}^\beta > \text{Bu}^\gamma$." He continues, "The purpose of this investigation was to discover whether or not the coefficients vary in this manner. As shown in Table I, they do not, but are, in fact, $\text{Me} > \text{Et} < \text{Pr}^\beta \ll \text{Bu}^\gamma$." The statement quoted contains two remarkable inversions. First the series $\text{Bu}^\gamma > \text{Pr}^\beta > \text{Et} > \text{Me}$ previously assigned to us, is completely reversed to $\text{Me} > \text{Et} > \text{Pr}^\beta > \text{Bu}^\gamma$. This point need not be elaborated, for we have already remarked that there is no direct connexion between the expected sequence and the reaction order; that, indeed, a sequence cannot be predicted unless the question of mechanism is first settled. Secondly, in order to give plausibility to the inverted alkyl series, reactions such as were previously called "unimolecular" are now called "second order"; although, *as can be seen from the experimental section of the same paper, they are first order*—as they must be, since one of the reagent concentrations is buffered.

Before presenting our own results on this subject (Section 2), we should state that there are considerable discrepancies between Taylor's rate constants and ours, although the differences do not leave any doubt about the main qualitative relationships. It is true that rate constants for hydrolysis in mixed solvents are not always exactly reproduced by different workers, because some rates are sensitive to small differences of solvent composition; but this consideration hardly affects the Arrhenius critical energies, which we have found to be rather slightly sensitive to solvent composition. When Taylor's paper 3 appeared recording critical energies for the acid hydrolysis of *tert.*-butyl bromide in aqueous alcohol, Cooper and Hughes had in the press a paper (J., 1937, 1183) which indicated discrepancies with Taylor's values of 3—4 kg.-cals. in 20. We have since repeated and extended these determinations. The results closely confirm the values of Cooper and Hughes, and the magnitude (2.3—4.5 kg.-cals.) of the discrepancies. There are also discrepancies with some of the less sensitive rate constants (*e.g.*, those for ethyl bromide) which are quite out of proportion to any uncertainty in the specification of the solvent.

[Added 20.1.40.] (1a) *Comments on Two Further Papers by W. Taylor.*

Paper 11 by Taylor and Read has just appeared. It duplicates and extends some of the errors of paper 10 (references, this vol., p. 900), which we had intended to pass by without comment. It now seems necessary to deal with these papers.

In the first place, paper 11 contains data which, if accepted, show that Taylor's observations of paper 8, which we did not systematically repeat, on the hydrolysis of methyl, ethyl, *isopropyl* and *tert.*-butyl bromide in aqueous acetone are in just as serious error as are his observations of paper 3, which we have repeated, on the hydrolysis of the same halides in aqueous alcohol. Scrutiny is necessary in order to discover that the discrepancies are serious, that they change the order of the alkyl groups, and that they thereby cut away the basis of Taylor's case as it was presented in paper 8. In the new paper the significant values are embodied with many others in a table, a footnote to which states that "these values correct those in J., 1938, 841," without any further indication of the magnitude or implications of the corrections. The necessary collation of data is given in Table Ia. The

experiments were done at 50° in acetone containing 5 and 10 vols. % of water. Second-order rate-constants, $k_2 = (dx/dt)/[H_2O](a-x)$, were calculated in accordance with Taylor's thesis that all the hydrolyses are bimolecular; of course, $[H_2O]$ is constant. The rate constants noted are $10^5 k_2$, with k_2 in hr.⁻¹g.-mol.⁻¹.

TABLE Ia.

Taylor's Data for Rates of Hydrolysis of Alkyl Bromides in Aqueous Acetone.

Medium.	$[H_2O]$.		MeBr.	EtBr.	Pr ^β Br.	Bu ^γ Br.
5 Vols. % aq.	2.7778	{ Paper 8	9.84	3.02	4.81	5,990*
		{ Paper 11	7.15	2.39	1.68	5,310
10 Vols. % aq.	5.5556	{ Paper 8	16.5	5.22	7.30	18,400*
		{ Paper 11	12.6	4.72	3.91	14,400

* These values were quoted in paper 8 from paper 5, wherein they contributed to a remarkable numerical agreement, which is a subject of comment in our accompanying paper (p. 913).

With 60—80% aqueous alcohol as solvent, Taylor had (paper 3) obtained the order Bu^γ>Me>Pr^β>Et, and claimed it to be contrary to our theory. With 90—95% aqueous acetone as solvent, he then (incorrectly, as it appears) reported the same sequence (paper 8), thus supporting his argument, in opposition to our idea of a change of mechanism, that the sequence was a fundamental characteristic of the alkyl groups in reactions with such reagents as water and alcohol: "The minimum at Et in the series Alk.X is attributable, not to a break from a uni- to a bi-molecular mechanism, but solely to the varying nature of the group Alk." Although the new figures lead to the different order Bu^γ>Me>Et>Pr^β, this order is not actually set down, and the previous argument is not mentioned.

It may be pointed out that such a change of order with change of solvent is quite consistent with our theory, and, indeed, is part of a general consequence of the theory that can be further illustrated. The rate minimum in the series Me, Et, Pr^β, Bu^γ is attributed to a change in the mechanism of substitution, the bimolecular mechanism taking charge at the left hand end, and the unimolecular mechanism at the right hand end, of the series (on account of the inductive effect *). As we change from less ionising to more ionising solvents, the unimolecular mechanism, which depends on ionisation, gains in relative importance, and hence the minimum shifts to the left. In weakly aqueous acetone the minimum has been found at Pr^β. In strongly aqueous alcohol it is found at Et. We shall show (this vol., p. 945) that in aqueous formic acid it shifts to Me, *i.e.*, that the order is Me<Et<Pr^β<Bu^γ with no intermediate minimum.

Much of paper 11 is concerned with the catalysis of the hydrolysis by mercuric bromide (cf. Nicolet and Stevens, *J. Amer. Chem. Soc.*, 1928, 50, 135, 212; Bodendorf and Böhme, *Annalen*, 1935, 516, 1; Roberts and Hammett, *J. Amer. Chem. Soc.*, 1937, 59, 1063), but the figures given do not support the statements made about them. Thus catalysis is said to increase in the series Me<Et<Pr^β<Bu^γ, and theoretical arguments are based on this statement; but actually the reported excess rates due to mercuric bromide yield various sequences, Et<Me<Pr^β<Bu^γ, Pr^β<Et<Me<Bu^γ, Me<Et<Pr^β<Bu^γ, according to the experiment selected. Again, it is stated that the accelerating effect of mercuric bromide is reduced by hydrogen bromide, but the data show both reductions and increases in different experiments. Furthermore, there seems to be little foundation for the claim that hydrogen bromide alone exerts a notable accelerating influence, much greater, for example, than that of sodium bromide. Moreover, the statement that added sodium bromide alone, even in concentrations up to 0.8M, does not sensibly modify the rate of hydrolysis of *tert.*-butyl bromide seems quite inconsistent with our observations (this vol., pp. 960, 966, 971, 974, 979) on salt effects in a number of unimolecular hydrolyses, including that of *tert.*-butyl bromide in aqueous acetone, where we find quite large changes of rate (*e.g.*, 40%) with salt concentrations no larger than 0.1M. Finally, the remarkable claim that *tert.*-butyl bromide catalyses its own hydrolysis remains unsupported, since, owing to their peculiar method of making up their solutions, the authors change their

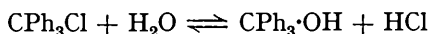
* This too simple statement is expanded in accompanying papers (this vol., pp. 899 and 949).

acetone-water ratio quite considerably when they alter the concentration of *tert.*-butyl bromide.

The authors' figures as a whole are of dubious significance for another reason. They state that, as "no olefin formation could be found", their kinetic experiments are "uncomplicated by side reactions" and therefore "more easily interpreted" than those of other investigators. But, in order to test for propylene and *isobutylene*, they first neutralised the acidic solutions with calcium carbonate (thus removing the olefin by generating a gas in the solution), then filtered (presumably on an open filter and possibly with a suction pump), and only *then* tried to distil the olefin (which had by that time disappeared) into standard bromine. A revision of their described test for *isobutylene*, but with precautions to avoid its loss (this vol., p. 934), has shown that, under these particular conditions, about 42% of the total reaction results in olefin production. This alone renders the authors' conclusions worthless.

The authors' methods of calculating their results are remarkable. For the forward reaction (assumed to be entirely hydrolysis), they used a second-order formula, allowing one kinetic order for water, the active mass of which is constant, but making no allowance for hydrogen bromide, which, according to their statement, accelerates the reaction more than water, and is generated in the process; and there are similar inconsistencies in the treatment of mercuric bromide. For the retrograde reaction, which Taylor also regards as bimolecular, they use the *first*-order formula, $kt = \ln a/(a - x)$, where a is the concentration of *both* alcohol and hydrogen bromide—a meaningless procedure, unless it be assumed that the concentration of one of the reagents is immaterial, which is certainly not the case. Excellent constants are reported. (The proper formula for a bimolecular reaction between equivalent amounts of reactants contains no logarithm.)

Errors analogous to a number of those discussed above, and also to many pointed out in the two immediately preceding papers, appear also in Taylor and Read's paper 10, which deals with the hydrolysis of triphenylmethyl chloride in moist dioxan. Good second-order rate and equilibrium constants are recorded for the reversible system



although there are two other systems, neglected by the authors, *viz.*,

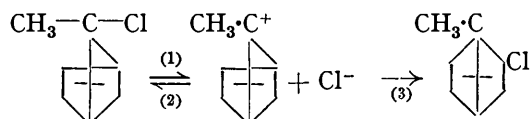


which must vary as to composition with the progress of the hydrolytic reaction and with the initial concentration of water or hydrogen chloride, because, over considerable portions of the reactions followed, the quantities of these materials were comparable. There are also other reasons of principle why the observation of any simple kinetic order is surprising. Thus, we have shown for the hydrolysis of benzhydryl halides that the liberated hydrogen halide has a marked retarding influence on the forward reaction (independently of any incursion of the back reaction), and we have given theoretical reasons for expecting a still stronger manifestation of the same phenomenon in the case of the triphenylmethyl halides (this vol., pp. 960 and 979). It is difficult to understand why Taylor and Read do not consider this effect in their example in view of the circumstance that we had made a preliminary report of its discovery (*J. Amer. Chem. Soc.*, 1938, **60**, 3080). Apart from the neglect of these general chemical matters, the authors' data are affected by an extensive series of corrections, every one of which can be shown to be incorrect in principle. The methods of calculation are likewise incorrect. Claiming on this basis to have established that the hydrolysis is a reaction of the second order, the authors conclude, just as Taylor had already done in other examples (cf. the two preceding papers), that the kinetic results establish the bimolecular mechanism and cannot be accommodated by the unimolecular mechanism. Reference to the papers just cited will show that the reasoning is unsound independently of what truth may attach to the claim on which it is based.

Finally we may comment on Taylor and Read's discussion (paper 11), in which the idea is developed that the rate minimum in the series Me, Et, Pr^{*β*}, Bu^{*γ*} may be due to a switch from nucleophilic to dominating electrophilic functions on the part of the reagent—our duplexity, S_N2 + S_N1, being now replaced by the duplexity S_N2 + S_E2. This is pushed

to the logical, but unacceptable, conclusion that even a powerful base such as diethylamine must behave in a "partly" electrophilic manner, *e.g.*, in the Menshutkin reaction; *i.e.*, that it must really be trying to combine with electrons when it in fact finds itself combining with an atomic kernel. We refer to these proposals, because, although such a theory is unlikely to appeal, it could cause confusion by reason of its association with a correct classification of mercuric bromide as an electrophilic reagent. The point to be noted, however, is that mercuric bromide is here a catalyst, *not the substituting agent*, which must be nucleophilic since it combines with a carbon kernel. Cowdrey, Hughes, Ingold, Masterman, and Scott have already given a theoretical treatment of this type of case (J., 1937, 1236, 1243, 1252) in the example of the heterogeneous catalysis of nucleophilic substitution by electrophilic silver ions: these assist the ionic fission of the alkyl halide, thereby facilitating a mechanism of substitution that may be regarded as an elaboration of mechanism S_N1 . Mercuric bromide acts similarly, probably through the tendency to form $R^+(HgBr_2)^-$, and so likewise do many other heavy-metal halides (Bodendorf and Böhme, *loc. cit.*). Cowdrey, Hughes, Ingold, Masterman, and Scott pointed out that the S_N1 mechanism as modified by electrophilic catalysis will follow the uncatalysed S_N1 mechanism in its dependence on structure; so reaction rate should increase in the series Me, Et, Pr^β , Bu^γ . Thus the "electrophilic" series, $Me < Et < Pr^\beta < Bu^\gamma$, which Taylor claimed (unjustifiably) to have illustrated by his experiments with mercuric bromide, elucidates no new point of theory, but is merely the sequence expected for the S_N1 mechanism in both its fundamental and its elaborated forms.

In order to generalise these conclusions, reference may be made to another important class of electrophilic catalysts, *viz.*, the non-ionised halogen acids (of course, in solvents for which these acids have little or no affinity). Their action, depending on their intense, polar, electrical field, is most often encountered as the autocatalysis of hydrogen halide elimination from alkyl halides in non-hydroxylic solvents (or in the absence of a solvent); but a well-investigated example of a similarly catalysed substitution is available in the "intramolecular substitution" in which camphene hydrochloride is converted into *isobornyl* chloride (Meerwein *et al.*, *Ber.*, 1920, 53, 1815; 1922, 55, 2500; *Annalen*, 1924, 435, 174, 207; 1927, 453, 16; Bartlett and Pöckel, *J. Amer. Chem. Soc.*, 1937, 59, 820; 1938, 60, 1585; Nevell, de Salas, and Wilson, *J.*, 1939, 1188). This has the catalysis described (*e.g.*, in chloroform), and involves a Walden inversion; chloride ions as such do not affect the rate, but exchange with isotopically distinguished chloride ions proceeds faster than the isomerisation. The effect of solvent variation strongly suggests ionisation as the rate-controlling step; and contrary to an assumption of Nevell, de Salas, and Wilson, we suppose that the rate-controlling process is the ionisation (1), [$v_1 \ll (v_2 + v_3)$], which is accelerated by electrophilic catalysts such as heavy-metal chlorides and non-ionised hydrogen chloride; and that, otherwise, the formed cation is simply partitioned very unequally ($v_2 \gg v_3$). This might be expected, since of the two succeeding processes, (2) and (3), only process (3) involves a rearrangement:



The rearrangement is really a bimolecular substitution at a saturated atom, and therefore necessitates inversion, even though inversion produces *isobornyl* chloride, the thermodynamically less stable stereoisomeride. The latter changes in ionising solvents, although relatively slowly, into bornyl chloride, thus confirming the view that only the "camphene hydro-" cation (formula above), and not the bornyl (= *isobornyl*) cation, is produced in the isomerisation; for when the bornyl cation *is* formed, then it associates with an anion to produce a bornyl, and not an *isobornyl*, compound.*

* We think that further experiment is needed to determine definitely whether hydrogen exchange, which Nevell, de Salas, and Wilson also investigated, is not to be correlated essentially with the reversible formation of camphene from the cation. There seems to us to be no sufficient reason against this simple explanation.

In general, we may conclude that there is nothing in the known authentic observations on the electrophilic catalysis of nucleophilic substitution that requires Taylor's theory or that cannot be interpreted on the model already given in the example of catalysis by silver ions.

(2) *Mechanism of Hydrolysis of Alkyl Halides in Acidic Aqueous Solvents.*

We now take up the question of whether or not there is a change of mechanism in the series MeBr . . . Bu^γBr for hydrolysis in acidic aqueous solvents. Reaction order tells us nothing, and we are therefore dependent on less direct indications, which we accept as evidence if they are mutually confirmatory. We justify this attitude on the ground that, although Taylor, for example, has produced such items of theory as are cited in this and in preceding papers, nobody has yet succeeded in harmonising completely the large mass of known results concerning structural influences on rate for reactions of many types, with kinetic order, with effects due to changes of reagent, effects due to solvent changes and salt additions, and with stereochemical phenomena, by means of any one simple theory except that which we have proposed and here employ: every apparent discord is shown in one or other of these papers to be traceable to faulty experiment or misleading record.

Our comparative data are in Table I. The figures for methyl and ethyl bromide represent new measurements. Those for *tert.*-butyl bromide are obtained by temperature-extrapolation of mutually confirmatory new results and published measurements by Cooper and Hughes (*loc. cit.*). Those for *isopropyl* bromide are derived by temperature-interpolation from data by Hughes, Ingold, and Shapiro (*loc. cit.*). Those parts of the total first- or second-order rates which represent olefin formation have been removed from the measured total rates, so that the figures in the Table relate to substitution only. The allowance to be made for olefin formation is negligible for ethyl bromide and for the first-order reaction of *isopropyl* bromide. For the second-order reaction of the *isopropyl* bromide it was exactly measured by Hughes, Ingold, and Shapiro. For *tert.*-butyl bromide the proportion of olefin was found by Cooper, Hughes, and Ingold (J., 1937, 1280) to be 12.6% in our solvent at 25°, and we have allowed 19% because the temperature of our comparison is higher. We do not exactly know the temperature effect on olefin proportion for *tert.*-butyl bromide, but we know it for *tert.*-butyl chloride (Cooper and Hughes, unpublished) and *tert.*-amyl chloride (Hughes and MacNulty, J., 1937, 1283), and have used these results in arriving at the figure 19%. This may well be 2% out, but exact value makes no difference to the discussion given below.

TABLE I.

Rates of Substitution of Alkyl Bromides in 80% Aqueous Ethyl Alcohol at 55.0°.

k_1 = first-order constant in sec.⁻¹. k_2 = second-order constant in sec.⁻¹ g.-mol.⁻¹.
[Taylor's values (papers 1 and 3) are given in parenthesis.]

	MeBr.		EtBr.		Pr ^β Br.	Bu ^γ Br.	
Solvent alone: $10^5 k_1$ (obs.)	0.349	(0.479)	0.139	(0.200)	0.237	1010‡	(240)‡
Solvent + NaOH: $10^5 k_2$ (obs.)	2140	—	171*	(108)†	4.75	—	—
Solvent with [NaOH] = 1: $10^5 k_1$ (calc.)	2140	—	171	—	4.99	1010	—

* This value is a little lower (as it should be) than that which MacNulty, Masterman, Hughes, and Ingold (this vol., p. 899) found for the same reaction at 55° in absolute alcohol, *viz.*, 197. The last figure may be compared with an interpolated value, 213, derived from Grant and Hinshelwood's results (J., 1933, 258). Taylor's directly determined value for absolute alcohol as solvent was 142.

† Graphically interpolated from a series of Taylor's values applying to different percentages of water in aqueous alcohol at 55°. Taylor worked with *ca.* 0.2N-alkali, a somewhat higher concentration than we have employed (cf. Table II, p. 934).

‡ Extrapolated for temperature. Most of the discrepancy between Taylor's value and ours arises from the considerable difference between Taylor's temperature coefficient and ours. As he disregards olefin formation, we have applied the 19% correction to his value to make it comparable with ours.

Examining the first line of figures in the Table, we find, from MeBr to EtBr a small fall in the rate; from EtBr to Pr^βBr a still smaller rise; and from Pr^βBr to Bu^γBr a very large rise. This is the variation characteristic of a change of mechanism at, or somewhat

to the right of, the rate minimum—the latter alternative arising in a refinement of theory given by Hughes, Ingold, and Shapiro. We infer that the reaction may be bimolecular for MeBr, essentially so for EtBr, partly so for Pr^βBr, and essentially unimolecular for Bu^γBr.

The simplest type of confirmation we may seek is that obtained by studying the effect of adding a strongly basic reagent to the solvent: we added sodium hydroxide. Wherever the reaction with water or alcohol is bimolecular, that with hydroxide or ethoxide ions is sure to be; and is sure to be much faster. The second-order rate constants are given by the second-row figures in Table I. If we imagine the sodium hydroxide concentration to be buffered to *N*, the figures of the first and second rows become directly comparable, the latter now becoming first-order constants under the mass-law; and thus the total reaction should be represented by constants which are numerically the sum of the two, as is shown in the bottom line of figures. Comparing these totals with their components, we find pure second-order reactions for MeBr and EtBr, a reaction predominantly of second order for Pr^βBr, and an inappreciable second-order reaction for Bu^γBr. Taking account of the concentration of the solvent for the purposes of an order-of-magnitude calculation, we see that the specific rate which would have to represent the activity of water and alcohol in the assumed bimolecular reaction with methyl bromide is 10⁻⁵ times smaller than the corresponding rate for the reaction with hydroxide and ethoxide ions. This is entirely consistent with the known relationship between specific rates and basic strengths, which Brønsted has so extensively illustrated. The same is true for ethyl bromide, for, although the rate disparity is reduced by a factor of 4, the ratio is still of order 10⁻⁵. We emphasize that this does not exclude the possibility that some small part of the total rate of the reaction of ethyl bromide with the solvent is contributed by a unimolecular reaction. A duplex constitution for the reaction between *isopropyl* bromide and the solvent is rather definitely indicated, since the rate ratio here is only of order 10⁻²—10⁻³; whilst for *tert.*-butyl bromide the absence of a measurable second-order reaction with sodium hydroxide shows the solvent reaction to be essentially unimolecular.

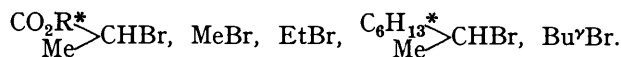
Reverting to Section (1) of this paper, we may note the confirmation that, when the reaction really is a second-order one, and demonstrably bimolecular, the rates (Table, row 2) diminish through the series as they should.

Unfortunately the evidence to be obtained from the effect of solvent variation is not qualitatively diagnostic; for theory requires (see Hughes and Ingold's Table, J., 1935, 252, cases 3 and 4) that a change to a more highly solvating solvent should increase the rate both of the unimolecular process and of the bimolecular reaction *with the solvent*; quite unlike the bimolecular reaction with hydroxide and alkoxide ions, the rate of which should be decreased (case 6), as it is known to be for methyl and ethyl iodide (*loc. cit.*) and *isopropyl* bromide (Hughes, Ingold, and Shapiro). Nevertheless, it is worth seeing whether methyl and ethyl bromides, whose behaviour from a kinetic standpoint is different from that of the higher bromides, exhibit the required effect in their solvent reactions: at least if they did not, we should be proved wrong. We find, in agreement with Taylor, that more aqueous solvents increase the rate of hydrolysis of ethyl bromide, as they should. However, it is interesting and probably significant that, as Taylor's results also show, the solvent effects for methyl, ethyl and *isopropyl* bromide in the reactions with solvent alone are similar to one another and considerably smaller than for *tert.*-butyl bromide; and, furthermore, that for the most highly aqueous solvents, which should bring out most prominently the unimolecular reaction of *isopropyl* bromide, this halide separates itself distinctly from the two lower homologues, its solvent effect rising towards that of *tert.*-butyl bromide. From a rather different point of view, it has already been shown (Bateman, Hughes, and Ingold, J., 1938, 881) by means of a comparison of rates and product compositions for the solvolysis of *tert.*-butyl chloride in aqueous methyl or ethyl alcohol that these reactions are definitely not bimolecular in mechanism.

We do not discuss salt effects here because the evidence relating to these is sufficiently important to deserve separate publication: a part of this evidence is reported in accompanying papers (this vol., pp. 960 and 979).

There remains the stereochemical criterion. It is true that we cannot apply this directly

to the simple compounds here treated, but we can make substitutions in, and extensions to our series, thus permitting the conclusions to be checked stereochemically at certain points, *e.g.*, those marked * :



Here we have replaced *isopropyl* bromide by *sec.*-octyl bromide, which is known to display a closely similar behaviour, not only in the kinetic form of its hydrolytic reactions, but also in their absolute reaction rates (Hughes and Shapiro, J., 1937, 1192). It has been shown that the solvent reactions of *sec.*-octyl bromide exhibit in considerable amount the racemisation which is diagnostic of the unimolecular mechanism (Hughes, Ingold, and Masterman, J., 1937, 1196). In α -bromopropionic acid, or its methyl ester, we have added a strongly electron-attracting group, thus extending our series to the left. By way of contrast, it has been found that this acid and ester exhibit no racemisation in their solvent reactions, but only the quantitative inversion of configuration characteristic of the bimolecular mechanism (Cowdrey, Hughes, and Ingold, J., 1937, 1208).

We fully admit that the evidence presented in this paper on the subject of the solvent reaction is circumstantial; but it is wholly consistent, not only internally, but also with the varied evidence of our previous papers and those of the present group (this vol., pp. 899, 935, 940, 945, 960, 979). On account of the consistency and variety of the evidence we are strongly inclined to accept it as proving that there is a change of mechanism in the investigated solvent reactions of the series MeBr Bu^{*}Br.

It was not surprising to find, after the publication of Taylor's papers, that some authors had doubts as to whether our theory of hydrolyses and other substitutions possessed the diagnostic quality and guiding power we claim for it. We illustrate these doubts with a single quotation from a recent paper by Hinshelwood, Laidler, and Timm (J., 1938, 849), who write : " Reactions have been classified as class A or class B according as the reaction is facilitated by accession of electrons to or recession of electrons from the point of attack (Ingold and Rothstein, J., 1928, 1217). This terminology is convenient in classifying data, but gives no insight into actual mechanism. In particular, it is impossible to predict to which class the reaction belongs, as was clearly pointed out by Oxford and Robinson for the case of hydrolysis (J., 1926, 384) . . ." (Taylor is cited later). Now no such simple statement as this can really summarise the situation fairly. It is true that, if we knew no facts *at all*, we could not predict any. But, if in the theoretically constructed series, AX QX, we are told that the already discussed change of kinetic type has been observed at, say FX, then at once we can assert that all members to the right of this point will belong to Ingold and Rothstein's class A, and that (possibly after a short gap) all members to the left will belong to class B. Thus a considerable element of prediction is present in our theory. It should be emphasised that what Oxford and Robinson wrote in 1926 was perfectly true. The diagnostic quality referred to entered only with Hughes, Ingold, and Patel's paper of 1933, though it had been widely illustrated before 1938, the date of the above quotation.

EXPERIMENTAL.

Except for the experiments at low temperature which were done by withdrawing aliquot portions of the reaction mixture as usual, the sealed bulb technique was employed. Portions of, *e.g.*, 5 c.c. of reaction mixture were enclosed in sealed bulbs, which were placed in the thermostat for known times, and then broken under 100 c.c. of either cold acetone or cold ethyl alcohol, according as the solution was acid or alkaline, to stop the reaction. Lacmoid was used as indicator for the acid-alkali titrations. Second-order rate constants obtained by the bulb method were corrected for thermal expansion.

In the acid hydrolyses of methyl and ethyl bromide, analysis during the end part of the reaction is disturbed by reaction between the hydrogen bromide and the solvent; however, this did not prevent the determination of good constants from the earlier portion of the reaction (cf. Table III). In order to avoid having to weigh methyl and ethyl bromide accurately, the initial concentration of this material was obtained by mixing samples of the reaction mixture with alcoholic potassium hydroxide and estimating the bromide ion by Volhard's method.

Owing to the volatility of methyl bromide it was felt desirable in experiments at 55° to make sure that no error was being caused by some part of the material "hiding" in the gas phase. To this end bulbs of two different sizes were used for the duplicate runs of our usual routine, the gas-space ratio being 1 : 2.3. The results agreed. The unlikely possibility that the discrepancies with Taylor's values could be attributed to an uncontrolled peroxide effect was also checked by doing a few runs in the presence of quinol; however, this made no difference.

The new rate constants are listed in Table II. In all cases the initial concentration of the organic halide was of the order of 0.05M (cf. Table III). By "80% EtOH" we mean the solvent obtained by mixing at room temperature 80 vols. of absolute ethyl alcohol with 20 vols. of water; the other indications of solvent composition in Table II have a like significance. It should be noted that the constants of Table II are *total* first- and second-order constants. As we have already pointed out, an appreciable part of the first-order rates for *tert.*-butyl bromide (e.g., 12.6% for 80% EtOH at 25°) corresponds to olefin formation. Some individual runs are illustrated in Table III, each recorded reading being a mean of duplicate determinations.

TABLE II.

Summary of First- and Second-order Rate Constants.

(k_1 in sec. ⁻¹ ; k_2 in sec. ⁻¹ g.-mol. ⁻¹ l.)					
Halide.	Medium.	Initial [NaOH].	Temp.	$10^5 k_1$.	$10^5 k_2$.
MeBr	80% EtOH	—	55.0°	0.349	—
"	"	0.0709	"	—	2140
EtBr	"	—	"	0.139	—
"	"	0.0697	"	—	171
"	80% Me ₂ CO	—	50.6	0.0264	—
"	90% Me ₂ CO	—	"	0.00583	—
Bu ^γ Br	60% EtOH*	—	24.95	376	—
"	"	—	0.1	13.3	—
"	80% EtOH	—	24.95	36.3	—
"	"	0.140	"	35.7	—
"	"	—	0.1	1.16	—
"	90% EtOH	—	24.95	7.14	—
"	80% Me ₂ CO	—	50.6	176	—
"	90% Me ₂ CO	—	"	20.7	—

* $E = 21.7$ kg.-cals. Taylor (paper 4) gave 17.2 kg.-cals.

TABLE III.

Illustrating Determination of First- and Second-order Rate Constants.

Hydrolysis of MeBr in 80% EtOH at 55.0°. $[\text{NaOH}]_{t=0} = 0$. $[\text{MeBr}]$ expressed in equivalent c.c. of 0.0104N-alkali per 5 c.c. sample. k_1 in sec.⁻¹.

t (hrs.)	0.0	4.5	9.0	15.75	24.5	39.0	47.0	88.0
$[\text{MeBr}]$	31.10	29.47	27.98	25.22	22.74	19.20	17.22	9.88
$10^5 k_1$	—	3.34	3.26	3.71	3.55	3.44	3.50	3.62

Hydrolysis of EtBr in 80% EtOH at 55.0°. $[\text{NaOH}]_{t=0} = 0.0698\text{N}$. $[\text{EtBr}]_{t=0} = 0.0299\text{M}$. $[\text{EtBr}]$ expressed in equivalent c.c. of 0.0498N-acid per 10 c.c. sample. k_2 in sec.⁻¹g.-mol.⁻¹l.

t (hrs.)	0.00	0.50	0.75	1.02	1.97	2.51	3.99
$[\text{EtBr}]$	6.00	4.84	4.45	3.96	2.88	2.45	1.48
$10^5 k_2$	—	1.79	1.69	1.76	1.70	1.67	1.76

Hydrolysis of Bu^γBr in 60% EtOH at 24.95°. $[\text{NaOH}]_{t=0} = 0$. $[\text{Bu}^{\gamma}\text{Br}]$ expressed in equivalent c.c. of 0.0421N-alkali per 5 c.c. sample. k_1 in sec.⁻¹.

(secs.)	0	55	117	184	255	420	542	729
$[\text{Bu}^{\gamma}\text{Br}]$	8.86	7.21	5.72	4.44	3.33	1.80	1.07	0.56
$10^5 k_1$	—	3.75	3.74	3.75	3.84	3.80	3.90	3.79

[Added 20.1.40.] *isoButylene from the Reaction of tert.-Butyl Bromide with Aqueous Acetone containing Mercuric Bromide.*—A solution of freshly distilled *tert.*-butyl bromide (13.7 g.) and mercuric bromide (1.8 g.) in a mixture of water (10 c.c.) and acetone (100 c.c.)—these were the proportions used by Taylor and Read for their reported estimation of *isobutylene*—was heated at 50.0°, whilst the *isobutylene* was led away in a stream of air. A slight modification of our usual trapping system was introduced in order more effectively to cope with acetone solvent. Any acetone not returned by the well-cooled reflux was held by means of a trap containing saturated aqueous sodium

hydrogen sulphite at 0°, and the dried gases were then passed on through two bubbling tubes containing methylene chloride at - 80°. The *isobutylene* thus collected was estimated by means of standard bromine solution as usual. Blank experiments, with omission of the *tert.*-butyl bromide, were conducted similarly. The quantities of *isobutylene* formed were close to 42% of the theoretical. We confirmed by further experiments, what is already clear from results published elsewhere (J., 1937, 1280; this vol., p. 899), *viz.*, that the proportion of olefin is a somewhat sensitive function of conditions; but we have not yet discovered any conditions in which the proportion approximates to the zero value reported by Taylor and Read.

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