

# THE HISTORY OF FREE RADICALS AND MOSES GOMBERG'S CONTRIBUTIONS

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## RADICALS BEFORE 1900

The history of the "radical" concept in chemistry is one of successive rise and decline. Ever since the term was introduced by Lavoisier<sup>1</sup> in his *Traité élémentaire de chimie* in 1789 it has not only taken on variant meanings but has been in favour, then out, as new developments in the science convinced chemists that radicals exist, or are preposterous. Thus, it is not surprising that after a century of such experience, Moses Gomberg's announcement of the triphenylmethyl radical should have been greeted with disbelief, or at least, disinterest in 1900.

When he dealt with the nature of acids Lavoisier assumed them to be oxygenated substances, i.e. oxygen combined with an entity which he chose to name a radical. According to his concept, the radical might be a single element in the case of the inorganic acids (sulphur in sulphuric and sulphurous acids, phosphorus in phosphoric and phosphorous acids, carbon in carbonic acid, the "radical muriatique" in muriatic acid, the "radical boracique" in boric acid), or it might be some stable combination of carbon and hydrogen in the various organic acids. The designation of an element as a radical quickly passed out of favour, particularly after Davy<sup>2</sup> demonstrated that oxygen is not an essential part of an acid.

Use of the word radical for a frequently observed combination of elements in a series of related compounds persisted, however. Not only did the term persist, but it took on an aspect of reality following Gay-Lussac's<sup>3</sup> discovery of cyanogen. Since it would be another 45 years before chemists seriously measured gas densities in order to ascertain molecular weights and formulas, there was little evidence to suggest that cyanogen is a dimer and it was looked upon as a free radical, CN. This position was easily held since the properties of cyanogen in the free form, and of the CN group in cyanides paralleled very closely the properties of the halogens, both in the free form and in the halides.

In 1828 the radical was utilized as an organizing concept in organic chemistry by Dumas and Boullay<sup>4</sup> in connection with the nature of alcohol, ether, and related compounds. Their paper suggested etherin (ethylene, formulated C<sub>4</sub>H<sub>4</sub>) to be the parent radical associated with water, hydrogen chloride, acetic acid, etc., in compounds such as alcohol, ether, hydrochloric ether, and acetic ether (cf. *Table 1*).

Despite Dumas' analogy to ammonia and the ammonium compounds, the proposed etherin radical was not viewed with enthusiasm until 1832 when

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Table 1. Formulation of the etherin radical

Compound	Formula (C = 6)	Present formula
Etherin	$C_4H_4$	$C_2H_4$
Alcohol	$C_4H_4, H_2O$	$C_2H_5OH$
Sulphuric ether	$2C_4H_4, H_2O$	$(C_2H_5)_2O$
Hydrochloric ether	$C_4H_4, HCl$	$C_2H_5Cl$
Hydroiodic ether	$C_4H_4, HI$	$C_2H_5I$
Nitric ether	$C_4H_4, HNO_2$	$C_2H_5NO_2$
Acetic ether	$C_4H_4, C_2H_4O_2$	$CH_3CO_2C_2H_5$

Liebig and Wöhler<sup>5</sup> published their work on oil of bitter almonds. This led to introduction of the benzoyl radical, based upon the apparent integrity of the  $C_{14}H_{10}O_2$  unit through a series of conversions leading to a variety of related compounds. Berzelius accepted the benzoyl radical with enthusiasm and wrote of "... The beginning of a new day in vegetable chemistry<sup>6</sup>". Before the next decade had passed, Dumas and Peligot<sup>7</sup> introduced the methyl, cetyl, and cinnamyl radicals, Piria<sup>8</sup> the salicyl radical, and Bunsen<sup>9</sup> the cacodyl radical (cf. Table 2). This last named substance not only retained its integrity through conversion from oxide to chloride, cyanide, iodide, and fluoride, but in the form of cacodyl, appeared to have independent existence as the free radical ( $C_2H_6As$ ). Only after reintroduction of Avogadro's hypothesis in 1860 was it evident that cacodyl really exists as the dimer.

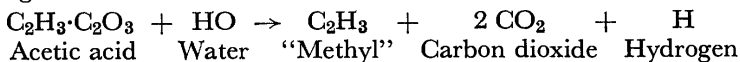
Table 2. Radicals of the 1830's

Radical	Author	Mol. formula	Modern equivalent
Ethylene	Dumas and Boullay	$C_4H_4$	$C_2H_4$
Benzoyl	Liebig and Wöhler	$C_{14}H_{10}O_2$	$C_7H_5O$
Methyl	Dumas and Peligot	$C_2H_3$	$CH_3$
Cetyl	Dumas and Peligot	$C_{32}H_{32}$	$C_{16}H_{32}$
Cinnamyl	Dumas and Peligot	$C_{20}H_{18}O_2$	$C_{10}H_9O$
Salicyl	Piria	$C_{14}H_{10}O_4$	$C_7H_5O_2$
Cacodyl	Bunsen	$C_4H_6As$	$C_2H_6As$
Ethyl	Liebig and Berzelius	$C_4H_{10}$	$C_2H_5$
Acetyl	Liebig	$C_4H_3$	$C_2H_3$

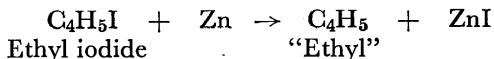
Although there was hope in the thirties that organic chemistry might soon be systematized through the introduction of a limited number of radicals, the details of the system continued to prove troublesome, even in the hands of the leading chemists of the day. The vociferous Liebig juggled atoms around to suit his momentary convenience in connection with the ethyl and acetyl radicals. The authoritative Berzelius soon soured on radicals since they did not entirely fit the postulates of his dualistic theory. Dumas was confronted by the fact of substitution of chlorine for hydrogen in his own laboratory and, while quick to detach himself from Laurent<sup>10</sup> and the nucleus theory, soon introduced the type theory in order to account for substitution<sup>11</sup>.

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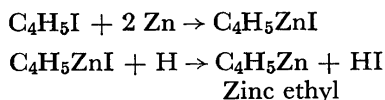
At the end of another decade characterized mostly by confusion, even though the contributions of Laurent and Gerhardt might have pointed the direction toward the solution of their problems concerning formulas, the chemical world had its attention directed toward radicals once more. Kolbe<sup>12</sup>, in the electrolysis of solutions containing salts of fatty acids, obtained gases that were interpreted to be free radicals. For example, electrolysis of potassium acetate yielded "methyl radical", carbon dioxide, and hydrogen.



At about the same time, Frankland was studying the reaction of alkyl halides with zinc. A decade before, when Bunsen had prepared "cacodyl radical" by heating cacodyl chloride with zinc, he had suggested that alcohol radicals could perhaps be prepared in the same way if the reaction were carried out under pressure. In 1848 Frankland<sup>13</sup>, working in Bunsen's laboratory, heated zinc with ethyl iodide in a sealed tube and obtained a gas which he interpreted as free ethyl. His studies also yielded white crystal-



line solids (zinc alkyl iodides) which, when heated in a current of hydrogen led to the formation of zinc alkyls, the earliest instance of the formation of organometallic compounds.

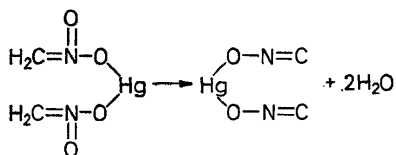


Frankland's work with organometallic substances was to lead him to the valency concept which, together with Kekulé and Couper's ideas on combination of carbon atoms and Cannizzaro's reintroduction of Avogadro's hypothesis, would lead to the abandonment of the type and radical theories. Once chemists began to compare vapour or gas densities they were able to ascertain molecular weights and therefrom, correct molecular formulas. As a consequence, the free radicals of cacodyl, methyl, and ethyl all proved to be dimers—cacodyl, ethane, and butane respectively.

The period which followed was a fruitful one for organic chemistry. Particularly significant was the development of structural theory resulting from the ideas of Butlerov and Kekulé, abetted by the work of Erlenmeyer, van't Hoff, Wislicenus, Laar, Baeyer, and Emil Fischer. Most structural problems of the organic chemist could be handled by assigning fixed valencies to the small number of elements involved. Kekulé became a foremost advocate of the quadrivalency of the carbon atom even though this had caused him problems in the case of his own benzene ring. Despite certain problem compounds, the quadrivalent state of carbon led to generally consistent formulas for the many compounds coming under study. Although assignment of single valency states to the various elements seriously handicapped the theoretical development of inorganic chemistry during this period, the same dogma proved immensely useful in the organic field. By

1890 it was virtual heresy to consider the valency of carbon other than four. (Of course, carbon monoxide proved an unavoidable embarrassment which was ignored whenever possible and considered non-organic when it could not be ignored.) Belief in the existence of free radicals had long been abandoned even though "radical" remained useful as a term referring to a group of atoms which remained joined through a series of reactions.

Probably the only significant exception to the accepted viewpoint was that of John Ulric Nef at the University of Chicago. Nef took issue with the concept that carbon must be quadrivalent, pointing not only to carbon monoxide where carbon appeared to show a valency of two, but to the fulminates as well. While studying the salts of nitromethane he prepared the mercury salt, reported to be an explosive solid. The warm liquid removed from the solid by filtration was soon observed to deposit fulminate of mercury crystals. This chance observation caused Nef to re-examine the formula of the fulminates. They had been looked upon as salts of a dibasic acid since the days of Liebig and Gay-Lussac. Nef's interpretation<sup>14</sup> of the reaction was:

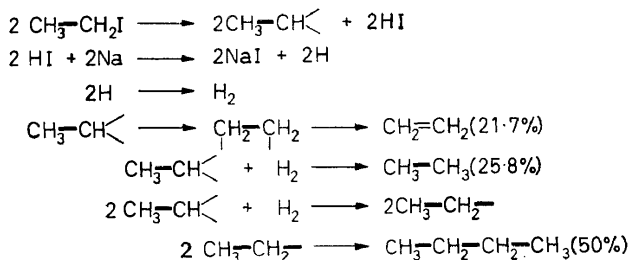


Mercury salt of nitromethane

Mercury fulminate

He looked upon the carbon in the fulminates as divalent, particularly after he was able to isolate the hydrogen chloride addition product,  $\text{HON}=\text{CHCl}$ . His ideas on divalent carbon were extended to other areas, e.g. he looked upon hydrogen cyanide as  $\text{H}-\text{N}=\text{C}$ .

In his attack on the Kekulé concept of the quadrivalency of the carbon atom, Nef suggested that the valency of carbon might be either 4 or 2. Inorganic chemists were now at the point of being willing to depart from constant valency in the case of some of the metallic elements so why should carbon not show a variable valency? Nef then utilized the concept of dual valency states of carbon to explain the mechanism of organic reactions. Since he was always deeply interested in side reactions as well as in the principal reaction, it was possible for him to design reaction mechanisms with a certain degree of plausibility. In the case of the Wurtz reaction he explained pathways and yields as follows:

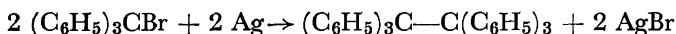


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It will be recalled that Arrhenius' theory of ionic dissociation was just then receiving considerable discussion and some acceptance in inorganic circles. Thus, a concept of organic reactions in which fleeting intermediates contained bivalent but highly reactive carbon appeared somewhat plausible. Nef sought to treat reactions by steps in which reactive centres were created, to be followed by addition reactions. However, his concepts did not stand the test of critical examination as well as Arrhenius' theory and they failed to find serious acceptance among organic chemists. It was easy to look upon his mechanisms as paper mechanisms without any evidence of physical reality. The chemical world was moving into the twentieth century quite happy with the quadrivalency of the carbon atom and was inclined to look upon free radicals as speculative inventions. It was in this environment that Moses Gomberg's paper "Triphenylmethyl, ein Fall von Dreiwerthigen Kohlenstoff"<sup>15</sup> made its appearance.

### GOMBERG'S WORK ON FREE RADICALS

Gomberg himself was unprepared for the observed results when he set out to prepare hexaphenylethane. While still in Victor Meyer's laboratory at Heidelberg he had been successful in the preparation of tetraphenylmethane<sup>16</sup>. On returning to Michigan he sought to prepare the next fully phenylated hydrocarbon by the reaction of triphenylmethyl bromide with sodium in benzene. The reaction was unsuccessful with sodium but with finely divided silver a solid product was obtained. Analysis for carbon and hydrogen gave



results that were low. A second analysis at a higher temperature and in the presence of oxygen throughout the analysis still gave low results. A new preparation showed no significant change in the analytical values and Gomberg was forced to conclude that his compound contained oxygen. A new preparation using carefully prepared silver free of oxide yielded an identical product.

Table 3. Analysis of "hexaphenylethane"

	Carbon %	Hydrogen %
Calc. for $(\text{C}_6\text{H}_5)_6\text{C}_2$	93.83	6.17
Found, 1st anal.	87.93	6.04
Found, 2nd anal.	87.79	6.46
Found, 2nd prepn.	87.77	6.23
Found, 2nd prepn., 2nd anal.	88.23	6.34
Found, 3rd prepn.	87.93	6.04
Calc. for $(\text{C}_6\text{H}_5)_6\text{C}_2\text{O}_2$	88.03	5.79

Only when the reaction was carried out in an atmosphere of carbon dioxide was it possible to avoid the oxygenated product, but now there was no solid product at all, only a yellowish solution as an evidence that a reaction had taken place. Removal of the solvent (benzene) at reduced pressure led to

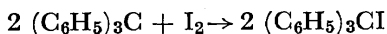
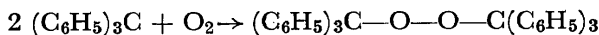
the recovery of a white solid of unexpectedly high reactivity. A solution of the product oxidized rapidly in the air, and reacted avidly with halogens, even including iodine—quite unexpected behaviour for hexaphenylethane (cf. Table 3).

In his first paper on the compound Gomberg wrote, "The experimental evidence . . . forces me to the conclusion that we have to deal here with a free radical, triphenylmethyl,  $(C_6H_5)_3C$ . On this assumption alone do the results described above become intelligible and receive an adequate explanation<sup>15</sup>". This paper opened up an area of chemistry which was received with much incredulity. The chemical world was oriented toward a disbelief in free radicals. After all, previous experience with such bodies had inevitably led to disproof. The influential Ostwald had just written in 1896, "It took a long time before it was finally recognized that the very nature of the organic radicals is inherently such as to preclude the possibility of isolating them<sup>17</sup>". Who could be sure that Gomberg's triphenylmethyl would not also be explainable as a dimer?

The failure of earlier free radicals to survive the facts of experimental chemistry did not frighten Gomberg. Once his experiments convinced him of the existence of triphenylmethyl as a free radical, he sought to explore further the nature of the substance, to seek out evidence in support of his views, to seek evidence for other stable free radicals, and to defend his position against his critics. A major part of his scientific work during the rest of his life was concerned with free radical chemistry. At the time of his retirement in 1936 he had published 35 experimental papers entitled "On triphenylmethyl" which represented the research which he and his students carried out on the subject. Actually this figure is low since numerous other papers dealt with ancillary phases of free radical chemistry<sup>18</sup>.

Not only were extensive studies made on triphenylmethyl but other aryl substituted compounds were prepared and studied. In general, these compounds confirmed the Gomberg conclusions on the presence of triarylmethyl radicals. In fact, the presence of substituent groups on the benzene rings generally augmented the tendency toward free radical formation, the greatest effect being observed when biphenyl was substituted for benzene in the radical.

Actually, the suggested presence of free radicals continued to be received with skepticism since evidence to the contrary was also present. Gomberg based his case for free radicals on the ease with which oxygen and halogens added to the compound to form the peroxide and the halides.

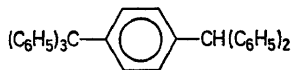


He also pointed to the colour of the compound, believing that hexaphenylethane should be colourless.

Belief in the free radical was bolstered in 1902 when Ullman and Borsum<sup>19</sup> isolated a compound which was generally accepted as hexaphenylethane. However, two years later Chichibabin<sup>20</sup> showed the compound to be *p*-benzohydril-tetraphenylmethane. This same compound was produced by

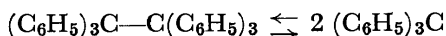
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Gomberg when his compound was treated with hydrogen chloride in benzene.



The free radical concept was soon found open to criticism, however, since cryoscopic determinations of molecular weight suggested the species in solution to be hexaphenylethane. Gomberg and Cone<sup>21</sup> reported molecular weight determinations in 1904, carrying out the studies under nitrogen in six different solvents. Observed molecular weights varied between 412 and 532 with an average of 477. The theoretical value for the free radical is 243, for the dimer, 486. It is evident that the average values fall close to that of the dimer and even the lowest value approaches the molecular weight of the dimer much more closely than that of the free radical.

These results forced Gomberg to give up the position he held during the years immediately following 1900 and to postulate the existence of an equilibrium mixture in solution.



Thus, the white solid was looked upon as undissociated hexaphenylethane, the coloured material in solution as the triphenylmethyl radical.

Although the evidence forced Gomberg to the conclusion that an equilibrium mixture existed, at least in solution, most chemists preferred to consider the compound to be hexaphenylethane, but unstable to oxygen, iodine, etc. Various attempts were made to explain the colour on the basis of quinoid structures but none of these was really convincing.

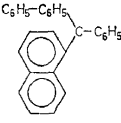
Gomberg's concept of an equilibrium mixture of free radical and dimer was supported by the behaviour of the coloured compound. When oxygen is added to a solution containing the yellow compound, peroxide forms and the solution becomes colourless, but in time the colour is restored. Addition of oxygen again destroys the colour but restoration soon follows. Repetition of this process ultimately results in removal of all of the hydrocarbon from solution. Further evidence for the equilibrium concept was found in the deepening of the colour as the temperature increased, suggesting that dissociation of the ethane was taking place.

Evidence that the coloured substance was involved in an equilibrium process was brought out when Piccard<sup>22</sup> showed that coloured solutions failed to obey Beer's law when diluted. Rather, dilution resulted in an increase in the coloured substance. These studies were later confirmed and extended by Ziegler.<sup>23</sup>

Additional evidence for dissociation of the hexasubstituted ethane was brought forth through the study of compounds containing aryl groups other than phenyl. Schlenk<sup>24</sup> prepared 1,1,2,2-tetraphenyl-1,2- $\alpha$ -naphthylethane and reported molecular weights of 363 and 372 (theoretical, 586 for undissociated, 293 for fully dissociated compound). Schlenk's laboratory at Jena also produced hexaarylethanes containing the *p*-biphenyl group where

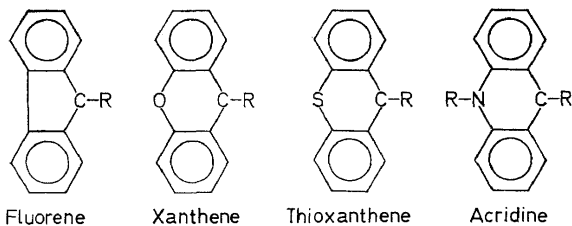
the presence of the triarylmethyl radical is strongly suggested by cryoscopic studies<sup>24</sup>.

Table 4. Dissociation of Hexaarylethanes (Schlenk)

Radical	At equilibrium (%)
$(\text{C}_6\text{H}_5)_3\text{C}$	5
$\text{C}_6\text{H}_5\text{-C}_6\text{H}_5\text{C}(\text{C}_6\text{H}_5)_2$	15
$(\text{C}_6\text{H}_5\text{-C}_6\text{H}_5)_2\text{CC}_6\text{H}_5$	80
$(\text{C}_6\text{H}_5\text{-C}_6\text{H}_5)_3\text{C}$	100
	100

On the basis of this kind of evidence, the case for free radicals was quite firmly established by 1911. Later research, however, threw grave question on the reliability of cryoscopic data. For example, in 1917 Gomberg and Schoepfle<sup>25</sup>, using Schlenk's 1,1,2,2-tetraphenyl-1,2- $\alpha$ -naphthylethane obtained molecular weights as low as 256 (theor. 293). Schlenk<sup>24</sup> reported molecular weights between 434 and 487 (theor. 471 for free radical) for hexa-*p*-biphenylethane, a substance apparently dissociated even in the solid state since it exists as greenish black crystals. Similar discrepancies with other compounds, together with the inherent uncertainties of cryoscopic measurements caused chemists to place less significance on such studies.

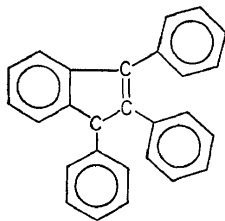
As the years passed more and more chemists became interested in compounds which dissociate into free radicals. This led to numerous variants of the hexaphenylethane type. Such studies showed that dissociation might be expected only when the ethane carbon atoms were badly overloaded. Generally, dissociation was enhanced when substituted phenyl groups (as methoxy, methyl, halogen, nitro) replaced unsubstituted phenyl groups. Even greater results were found with naphthyl and biphenyl groups, and with joined aryl groups as in fluorene, xanthene, thioxanthene, and acridine. As early as 1908, Kohler<sup>26</sup> prepared a free radical in which the third valency went to a non-aryl group. Many variants of these compounds were prepared





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particularly in the laboratories of Gomberg, Ziegler, Conant, Löwenbein,

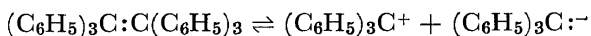


and Marvel. Conant<sup>27</sup> was successful in preparing free radicals where one of the aryl groups was replaced by an aliphatic group, i.e., methyl, ethyl, methoxy, propyl, and butyl (*n*-, *iso*-, *tert*-).

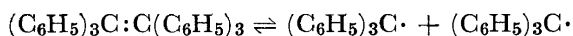
It was also possible to produce free radicals where elements other than carbon are involved. In such cases the atoms involved are overloaded with aryl groups. Wieland<sup>28</sup> prepared the free radical of tetraphenyl hydrazine in 1911 and Goldschmidt<sup>29</sup> later did extensive work with free radicals containing bivalent nitrogen. Schmidt<sup>30</sup> prepared free radicals with univalent sulphur, Goldschmidt<sup>31</sup> and Pummerer<sup>32</sup> with univalent oxygen.

It must be recalled that the early studies on triarylmethyl radicals were carried out in the days before there was a knowledge of atomic structure. As a consequence of the studies on radioactivity which were concurrent with Gomberg's work, it was possible for Rutherford and Bohr to develop the theory of the nuclear atom with its satellite electrons. Shortly thereafter, Kossel in Germany and G. N. Lewis and Irving Langmuir in the United States sought to develop an electronic theory of chemical bonding. The covalent bond, consisting of a shared pair of electrons proved to be a fruitful concept in organic chemistry.

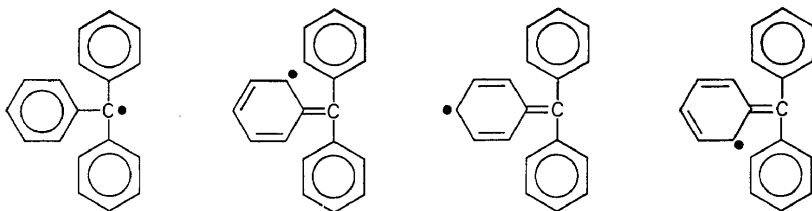
When electronic theory was applied to free radical substances there was uncertainty regarding the distribution of electrons between radicals. Since early investigators sometimes reported conductivity in polar solvents (SO<sub>2</sub>, HCN), there was a temptation to consider the formation of ions.



However, more careful investigations showed triarylmethyl radicals to be neutral. Ions were only found when there was combination with the solvent. Furthermore, G. N. Lewis associated colour in chemical substances with the presence of unpaired electrons. The colour of triphenylmethyl might best be explained by supposing a symmetrical split resulting in two identical radicals.



Resonance theory suggests a high resonance energy since resonance forms can be written with the unpaired electron in ten different positions, with consequent high stability as the free radical<sup>33</sup>.



Four of the ten resonance structures of triphenylmethyl

This viewpoint has become prevalent and free radicals are usually defined as atoms or molecules with one or more unpaired electrons. This definition, while not completely unambiguous, provides for a broad array of substances ranging from tri-biphenylmethyl to nitric oxide.

Since the electrical field associated with the spin of unpaired electrons remains uncanceled, molecules containing such an electron will respond to a magnetic field. Hence, magnetic susceptibility studies provide a fruitful method of study. Such susceptibility measurements have become a standard approach to the detection of free radicals and to measurement of equilibrium states. Extensive work along these lines was done in the laboratories of Müller<sup>34</sup>, Selwood<sup>35</sup>, Marvel<sup>36</sup>, and Sugden<sup>37</sup>.

The magnetic susceptibility method contains uncertainties connected with diamagnetic contributions and has been largely supplanted, at least for determining free radical concentrations and dissociation constants, by electron spin resonances<sup>38</sup>.

### TRANSIENT FREE RADICALS

In many quarters the triarylmethyl type free radicals were received, even as late as 1930, with skepticism or even antagonism. In 1915 V. Richter wrote, "The assumption of the existence of free radicals, capable of existing alone and playing a special role in chemical reactions, has long been abandoned<sup>39</sup>." A decade later C. W. Porter, at Berkeley, said "Negative results gradually established the doctrine that a free carbon radical was incapable of independent existence<sup>40</sup>." More commonly, Gomberg's evidence was accepted in the case of the heavily overweighted ethanes. Students in organic chemistry courses were told about the triphenylmethyl radical as if it were a curiosity to be glanced at hurriedly, then one must hurry on to topics in the main stream of the subject. The possibility of studying an ordinary radical like methane was looked upon with deep skepticism.

It was in 1929, in such a climate of opinion that Paneth and Hofeditz<sup>41</sup> reported evidence for the probable production of the free methyl radical. Using a modification of the method by which Bonhoeffer<sup>42</sup> prepared atomic hydrogen in 1924, they passed the vapour of tetramethyllead in a nitrogen, hydrogen, or helium carrier at low pressure through a glass tube as shown in *Figure 1*. When the tube was heated at A, a metallic lead mirror deposited as evidence of decomposition of tetramethyllead. When the point of heating was changed to B, a mirror deposited there but the mirror at A was removed. This was taken as evidence for the momentary existence of free methyl radicals.

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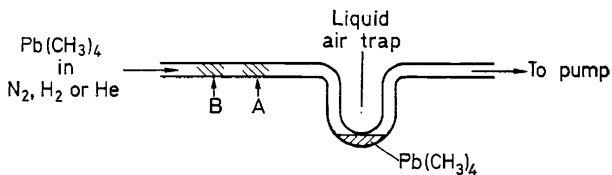


Figure 1. Paneth apparatus for preparing free methyl

Further evidence for this hypothesis was gained by identifying the liquid formed in the liquid air trap as tetramethyllead. Mirrors were formed and removed in similar fashion with the corresponding methyl compounds of antimony, bismuth, and zinc. Mirrors were not removed by hydrogen or nitrogen, or probable decomposition products of tetramethyllead such as ethane, methane, ethylene, or acetylene. When the distance between A and B was increased the rate of removal of metal at A decreased, suggesting the combination of free radicals with one another or with carrier gas before reaching A. Analysis of the exhaust gas revealed ethane to be the principal product when nitrogen or helium served as the carrier gas, but methane was predominant when hydrogen gas served as the carrier. The same technique yielded ethyl radicals when tetraethyllead was introduced into the system.

The procedures of Paneth were soon extended to a variety of organic compounds in the laboratory of F. O. Rice<sup>43</sup> at Johns Hopkins University and later at Catholic University. At temperatures of 800° to 1000° Rice found that metallic mirrors were removed by the pyrolysis products of hydrocarbons, alcohols, aldehydes, ketones, acids, and ethers. However, the only organometallic compounds that were trapped in the apparatus contained either methyl or ethyl groups, never higher ones.

### APPLICATIONS OF FREE RADICAL CHEMISTRY

During the last quarter century, free radicals have gained respectable status in chemical circles once more. The extensive and careful work of Gomberg and those who carried on in his field, together with the work on gas phase free radicals initiated by Paneth, led to a body of experimental evidence for stable as well as short-lived free radicals. It is therefore not surprising that certain venturesome chemists such as Morris Kharasch would start to introduce free radical hypotheses into their reaction mechanisms. Through such approaches it became possible to bring forth mechanisms having greater conviction than many of those hitherto in vogue.

With the growing willingness to accept free radicals there began to be closer attention to the application of analytical techniques to the detection of the presence of such radicals. Further, attention began to be given to the role that free radical hypotheses could play in handling theoretical and practical problems. Thus, free radicals no longer represent a subject for amused skepticism, but a concept that has taken its place in the growth of chemistry.

Many fields have responded to the free radical approach. While by no means an exclusive answer to all problems, free radicals figure prominently in substitution, addition, and polymerization reactions. Gomberg and

Bachmann<sup>44</sup> showed in 1930 that the triphenylmethyl radical is an intermediate in the formation of the corresponding Grignard reagent. At the stage where the magnesium was half used there was free radical and magnesium bromide but no Grignard reagent. The Wurtz reaction and many others have been shown to proceed through free radical stages. In inorganic chemistry too the free radical has taken an important place<sup>45</sup>.

Photolysis reactions were among the first to show extensive evidence of free radical formation and operation through a chain mechanism. Pearson<sup>46</sup> and Kharasch<sup>47</sup> did much pioneering work in this field although studies in this area date back to the time of Bunsen and Roscoe. High energy radiation is also a factor in free radical formation and is presumably responsible for the biological damage caused by radioactive atoms.

Despite its vicissitudes, the free radical concept has not only survived but has come to hold a needed and respected place in modern science. Not only is it now given serious treatment in the present day textbooks<sup>48</sup>, but has become the subject of an extensive monograph literature<sup>49</sup>. It is therefore proper that this historical development of the free radical be terminated and that the remainder of this symposium be placed in the hands of the active workers in the field who are engaged in making history.

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## HISTORY OF FREE RADICALS

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