Classic paper

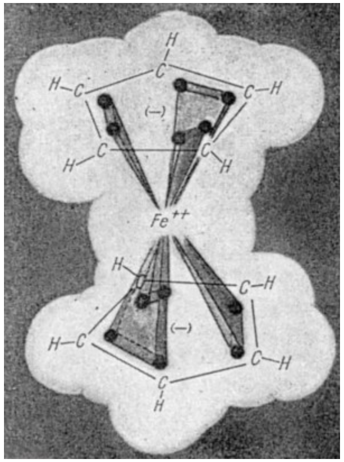
on

The classic sandwich: Ferrocene

Deepak Kr Patel
07/27/2024

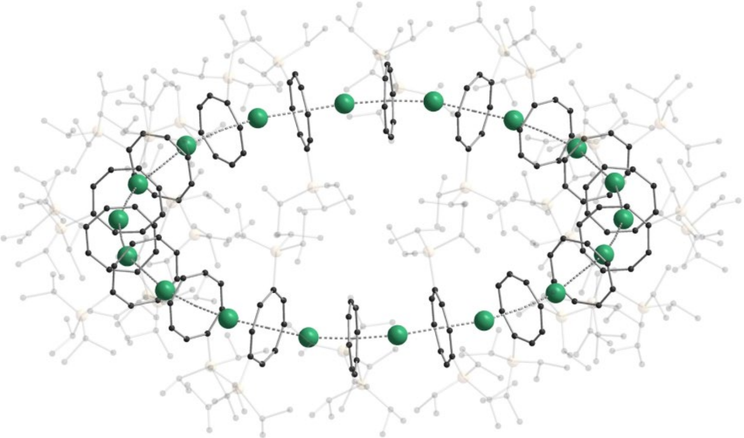
The classic sandwich: Ferrocene

Source: © Paul Reid/Ikon Images



An early drawing of ferrocene.
E. O. Fischer and R. Jira J. Organomet. Chem. 2001

First time Cyclic sandwich compounds

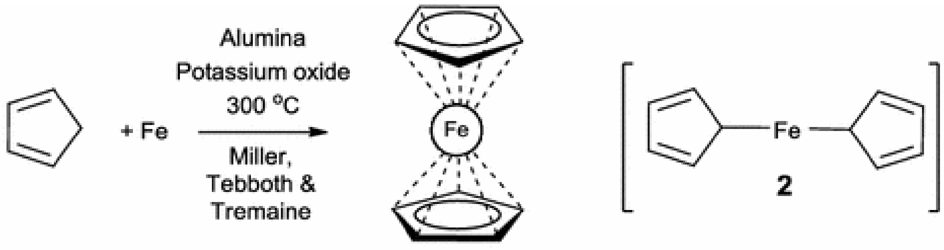


Peter W. Roesky & Manfred M. Kappes co-workers, Nature 2023

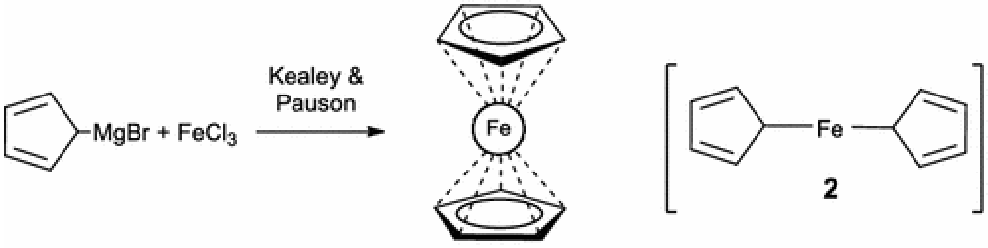
Ferrocene was discovered by accident twice

- The first synthesis occurred in the late 1940s at Union Carbide, where cyclopentadiene vapor created a "yellow sludge" that clogged an iron pipe. Eugene O. Brimm later found the sludge to contain ferrocene.

- The second time was around 1950, when Samuel A. Miller, John A. Tebboth, and John F. Tremaine, researchers at British Oxygen, were attempting to synthesize amines from hydrocarbons and nitrogen in a modification of the Haber process.



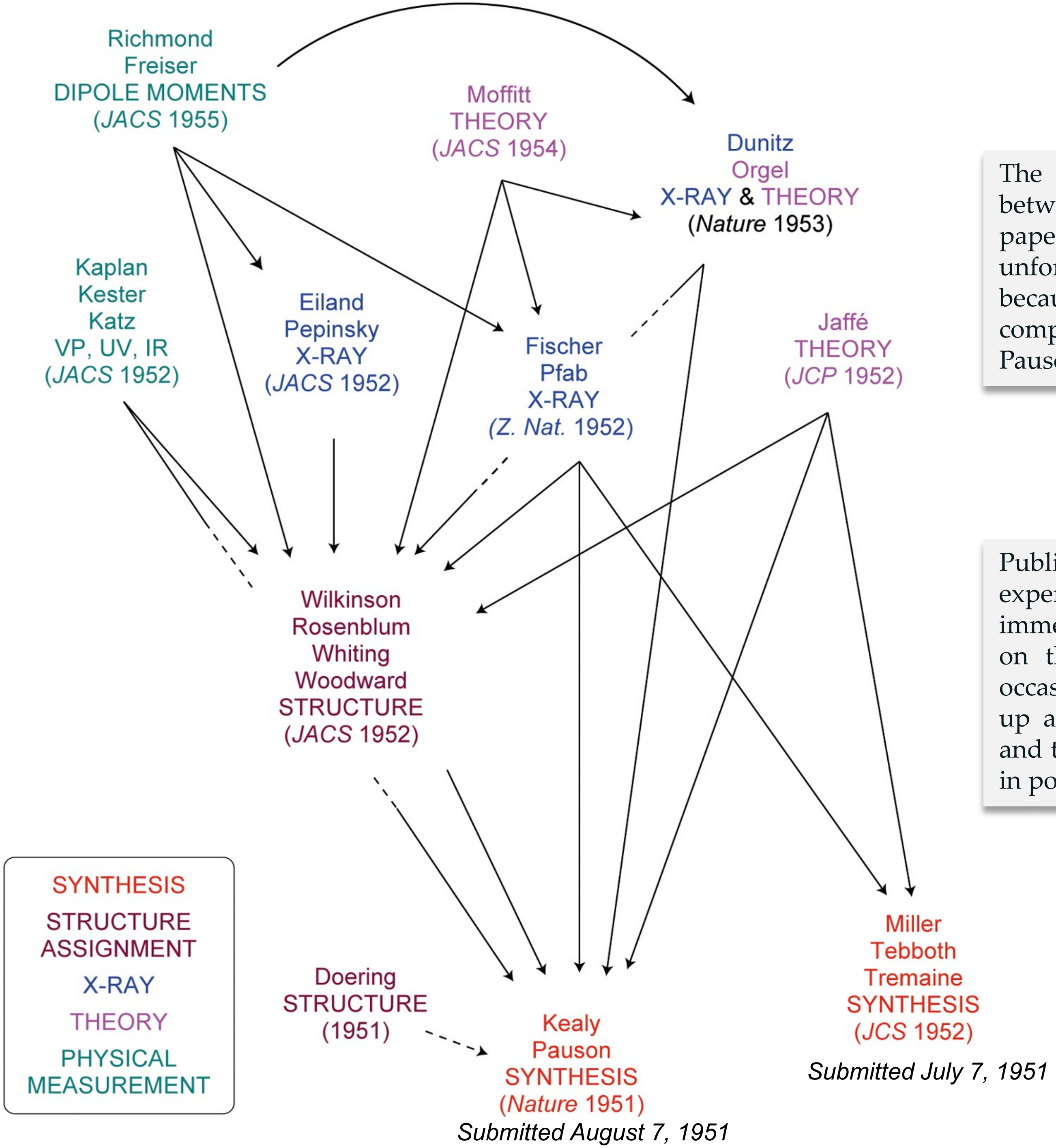
1
Ferrocene
Yellow crystals
mp 172.5-173 °C
Elemental analysis: $C_{10}H_{10}Fe$
Submitted July 7, 1951
Published in the *Journal of the Chemical Society*, February 1952



1
Ferrocene
Needle-like crystals
mp 173-174 °C
Elemental analysis: $C_{10}H_{10}Fe$
Submitted August 7, 1951
Published in *Nature*, December 15, 1951

- The first two independent syntheses of ferrocene (**1**) are noteworthy because two research groups both suggested the same incorrect formula **2**

Nature Chemistry, 2016



Richmond
Freiser
DIPOLE MOMENTS
(*JACS* 1955)

Moffitt
THEORY
(*JACS* 1954)

Dunitz
Orgel
X-RAY & THEORY
(*Nature* 1953)

Kaplan
Kester
Katz
VP, UV, IR
(*JACS* 1952)

Eiland
Pepinsky
X-RAY
(*JACS* 1952)

Fischer
Pfab
X-RAY
(*Z. Nat.* 1952)

Jaffé
THEORY
(*JCP* 1952)

Wilkinson
Rosenblum
Whiting
Woodward
STRUCTURE
(*JACS* 1952)

Doering
STRUCTURE
(1951)

Kealy
Pauson
SYNTHESIS
(*Nature* 1951)
Submitted August 7, 1951

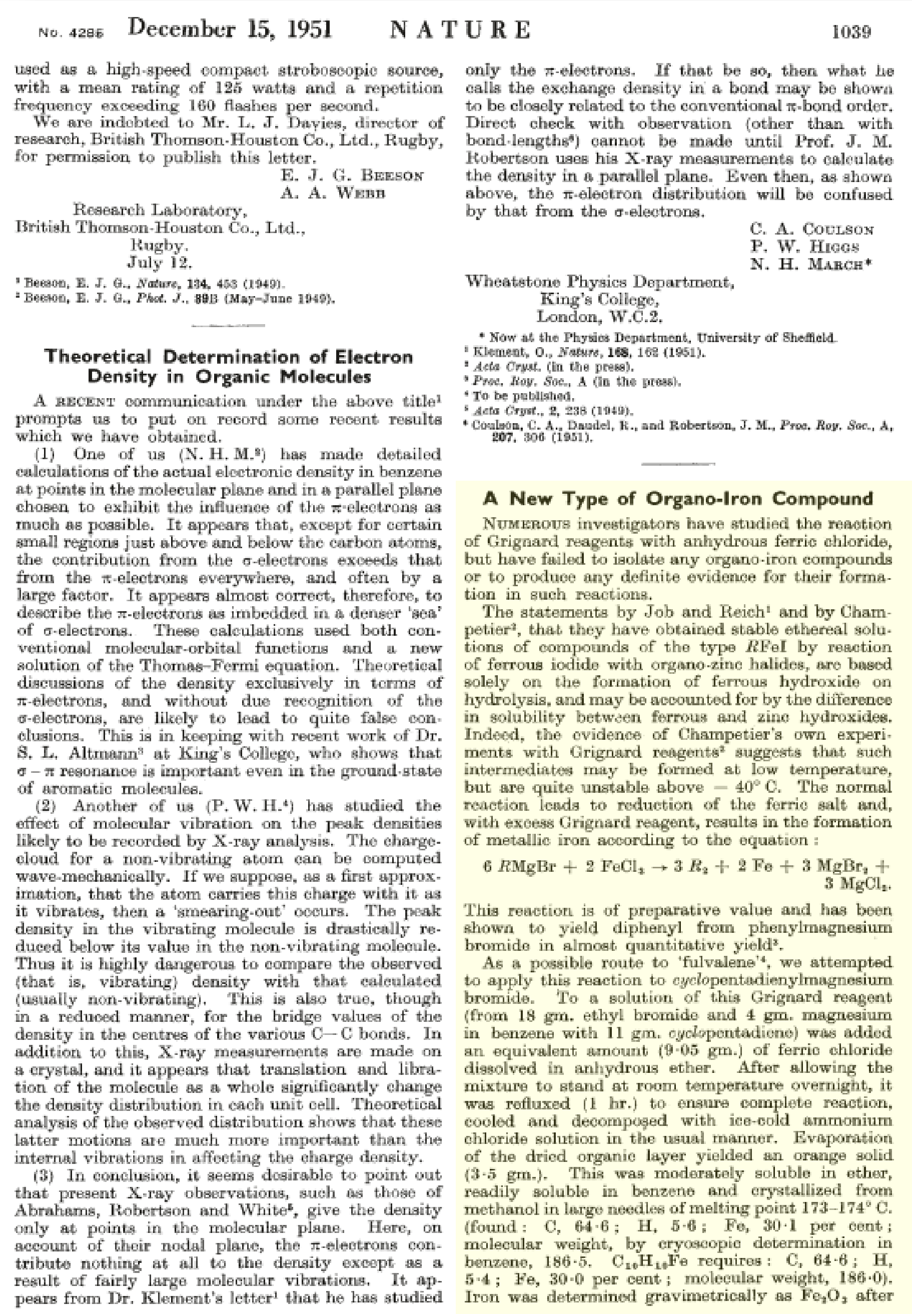
Miller
Tebboth
Tremaine
SYNTHESIS
(*JCS* 1952)
Submitted July 7, 1951

SYNTHESIS
STRUCTURE ASSIGNMENT
X-RAY
THEORY
PHYSICAL MEASUREMENT

The Miller paper was sandwiched between the 'first' paper and the 'right' paper — this was especially unfortunate for Miller and co-workers because their work had actually been completed in 1948 before Kealy and Pauson had even begun their research.

Publishing the wrong interpretation of experimental data can result in an immediate horde of chemists feeding on the error like vultures. On rare occasions, this phenomenon can open up an entire new field of science — and the structure of ferrocene is a case in point.

Nature Chemistry, 2016



No. 4286 December 15, 1951 NATURE 1039

used as a high-speed compact stroboscopic source, with a mean rating of 125 watts and a repetition frequency exceeding 160 flashes per second.

We are indebted to Mr. L. J. Davies, director of research, British Thomson-Houston Co., Ltd., Rugby, for permission to publish this letter.

E. J. G. BEESON
A. A. WEBB

Research Laboratory,
British Thomson-Houston Co., Ltd.,
Rugby.
July 12.

[1] Beeson, E. J. G., *Nature*, **134**, 453 (1949).
[2] Beeson, E. J. G., *Phot. J.*, 89B (May–June 1949).

Theoretical Determination of Electron Density in Organic Molecules

A RECENT communication under the above title[1] prompts us to put on record some recent results which we have obtained.

(1) One of us (N. H. M.[2]) has made detailed calculations of the actual electronic density in benzene at points in the molecular plane and in a parallel plane chosen to exhibit the influence of the π-electrons as much as possible. It appears that, except for certain small regions just above and below the carbon atoms, the contribution from the σ-electrons exceeds that from the π-electrons everywhere, and often by a large factor. It appears almost correct, therefore, to describe the π-electrons as imbedded in a denser 'sea' of σ-electrons. These calculations used both conventional molecular-orbital functions and a new solution of the Thomas–Fermi equation. Theoretical discussions of the density exclusively in terms of π-electrons, and without due recognition of the σ-electrons, are likely to lead to quite false conclusions. This is in keeping with recent work of Dr. S. L. Altmann[3] at King's College, who shows that σ – π resonance is important even in the ground-state of aromatic molecules.

(2) Another of us (P. W. H.[4]) has studied the effect of molecular vibration on the peak densities likely to be recorded by X-ray analysis. The charge-cloud for a non-vibrating atom can be computed wave-mechanically. If we suppose, as a first approximation, that the atom carries this charge with it as it vibrates, then a 'smearing-out' occurs. The peak density in the vibrating molecule is drastically reduced below its value in the non-vibrating molecule. Thus it is highly dangerous to compare the observed (that is, vibrating) density with that calculated (usually non-vibrating). This is also true, though in a reduced manner, for the bridge values of the density in the centres of the various C—C bonds. In addition to this, X-ray measurements are made on a crystal, and it appears that translation and libration of the molecule as a whole significantly change the density distribution in each unit cell. Theoretical analysis of the observed distribution shows that these latter motions are much more important than the internal vibrations in affecting the charge density.

(3) In conclusion, it seems desirable to point out that present X-ray observations, such as those of Abrahams, Robertson and White[5], give the density only at points in the molecular plane. Here, on account of their nodal plane, the π-electrons contribute nothing at all to the density except as a result of fairly large molecular vibrations. It appears from Dr. Klement's letter[1] that he has studied only the π-electrons. If that be so, then what he calls the exchange density in a bond may be shown to be closely related to the conventional π-bond order. Direct check with observation (other than with bond-lengths[6]) cannot be made until Prof. J. M. Robertson uses his X-ray measurements to calculate the density in a parallel plane. Even then, as shown above, the π-electron distribution will be confused by that from the σ-electrons.

C. A. COULSON
P. W. HIGGS
N. H. MARCH*

Wheatstone Physics Department,
King's College,
London, W.C.2.

* Now at the Physics Department, University of Sheffield.
[1] Klement, O., *Nature*, **168**, 162 (1951).
[2] *Acta Cryst.* (in the press).
[3] *Proc. Roy. Soc.*, A (in the press).
[4] To be published.
[5] *Acta Cryst.*, **2**, 238 (1949).
[6] Coulson, C. A., Daudel, R., and Robertson, J. M., *Proc. Roy. Soc.*, A, **207**, 306 (1951).

A New Type of Organo-Iron Compound

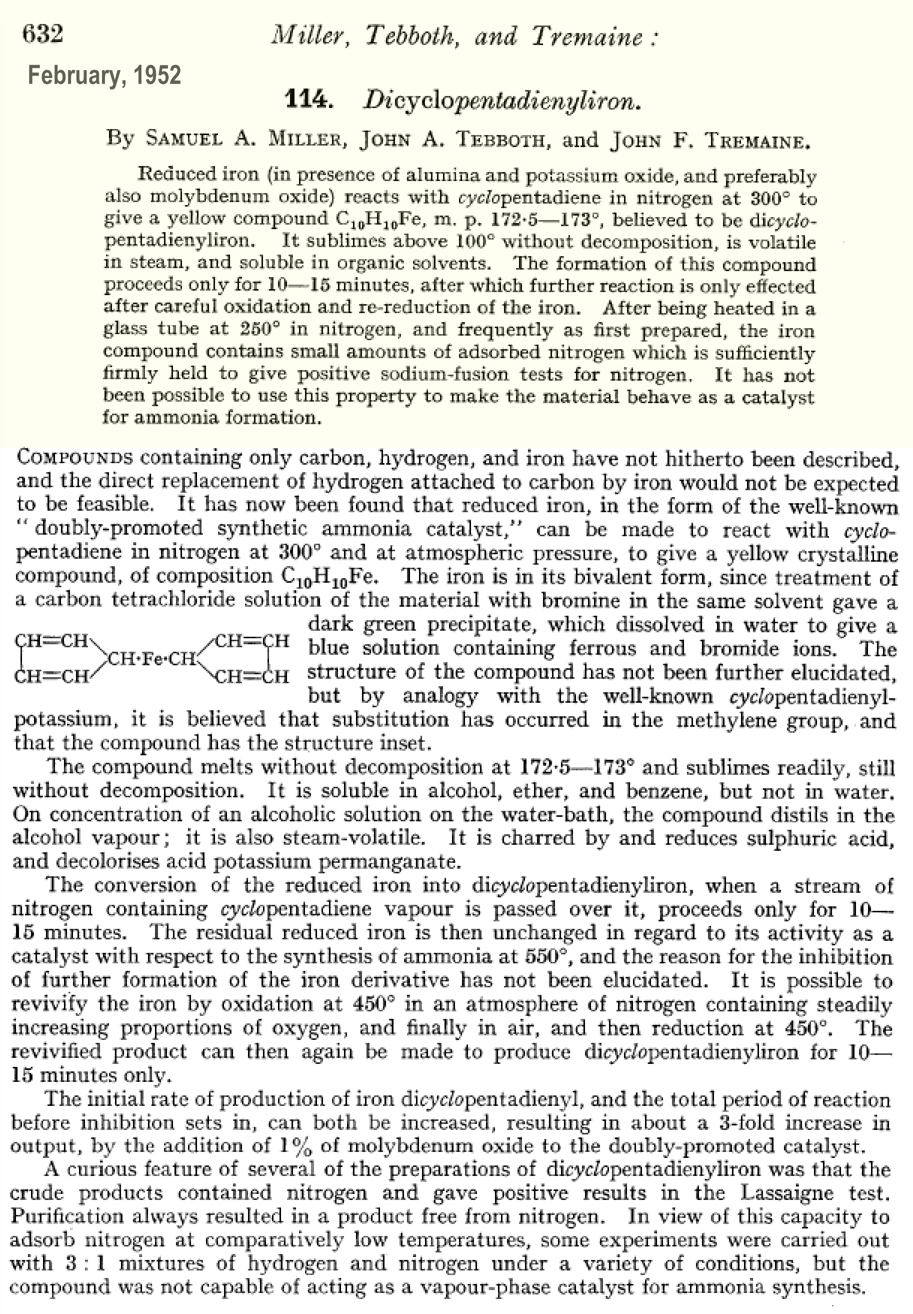
NUMEROUS investigators have studied the reaction of Grignard reagents with anhydrous ferric chloride, but have failed to isolate any organo-iron compounds or to produce any definite evidence for their formation in such reactions.

The statements by Job and Reich[1] and by Champetier[2], that they have obtained stable ethereal solutions of compounds of the type *R*FeI by reaction of ferrous iodide with organo-zinc halides, are based solely on the formation of ferrous hydroxide on hydrolysis, and may be accounted for by the difference in solubility between ferrous and zinc hydroxides. Indeed, the evidence of Champetier's own experiments with Grignard reagents[2] suggests that such intermediates may be formed at low temperature, but are quite unstable above – 40° C. The normal reaction leads to reduction of the ferric salt and, with excess Grignard reagent, results in the formation of metallic iron according to the equation :

$$6\,R\text{MgBr} + 2\,\text{FeCl}_3 \rightarrow 3\,R_2 + 2\,\text{Fe} + 3\,\text{MgBr}_2 + 3\,\text{MgCl}_2.$$

This reaction is of preparative value and has been shown to yield diphenyl from phenylmagnesium bromide in almost quantitative yield[3].

As a possible route to 'fulvalene'[4], we attempted to apply this reaction to *cyclo*pentadienylmagnesium bromide. To a solution of this Grignard reagent (from 18 gm. ethyl bromide and 4 gm. magnesium in benzene with 11 gm. *cyclo*pentadiene) was added an equivalent amount (9·05 gm.) of ferric chloride dissolved in anhydrous ether. After allowing the mixture to stand at room temperature overnight, it was refluxed (1 hr.) to ensure complete reaction, cooled and decomposed with ice-cold ammonium chloride solution in the usual manner. Evaporation of the dried organic layer yielded an orange solid (3·5 gm.). This was moderately soluble in ether, readily soluble in benzene and crystallized from methanol in large needles of melting point 173–174° C. (found : C, 64·6 ; H, 5·6 ; Fe, 30·1 per cent ; molecular weight, by cryoscopic determination in benzene, 186·5. $C_{10}H_{10}Fe$ requires : C, 64·6 ; H, 5·4 ; Fe, 30·0 per cent ; molecular weight, 186·0). Iron was determined gravimetrically as Fe_2O_3 after



632 *Miller, Tebboth, and Tremaine :*

February, 1952

114. *Dicyclopentadienyliron.*

By SAMUEL A. MILLER, JOHN A. TEBBOTH, and JOHN F. TREMAINE.

Reduced iron (in presence of alumina and potassium oxide, and preferably also molybdenum oxide) reacts with *cyclo*pentadiene in nitrogen at 300° to give a yellow compound $C_{10}H_{10}Fe$, m. p. 172·5—173°, believed to be di*cyclo*pentadienyliron. It sublimes above 100° without decomposition, is volatile in steam, and soluble in organic solvents. The formation of this compound proceeds only for 10—15 minutes, after which further reaction is only effected after careful oxidation and re-reduction of the iron. After being heated in a glass tube at 250° in nitrogen, and frequently as first prepared, the iron compound contains small amounts of adsorbed nitrogen which is sufficiently firmly held to give positive sodium-fusion tests for nitrogen. It has not been possible to use this property to make the material behave as a catalyst for ammonia formation.

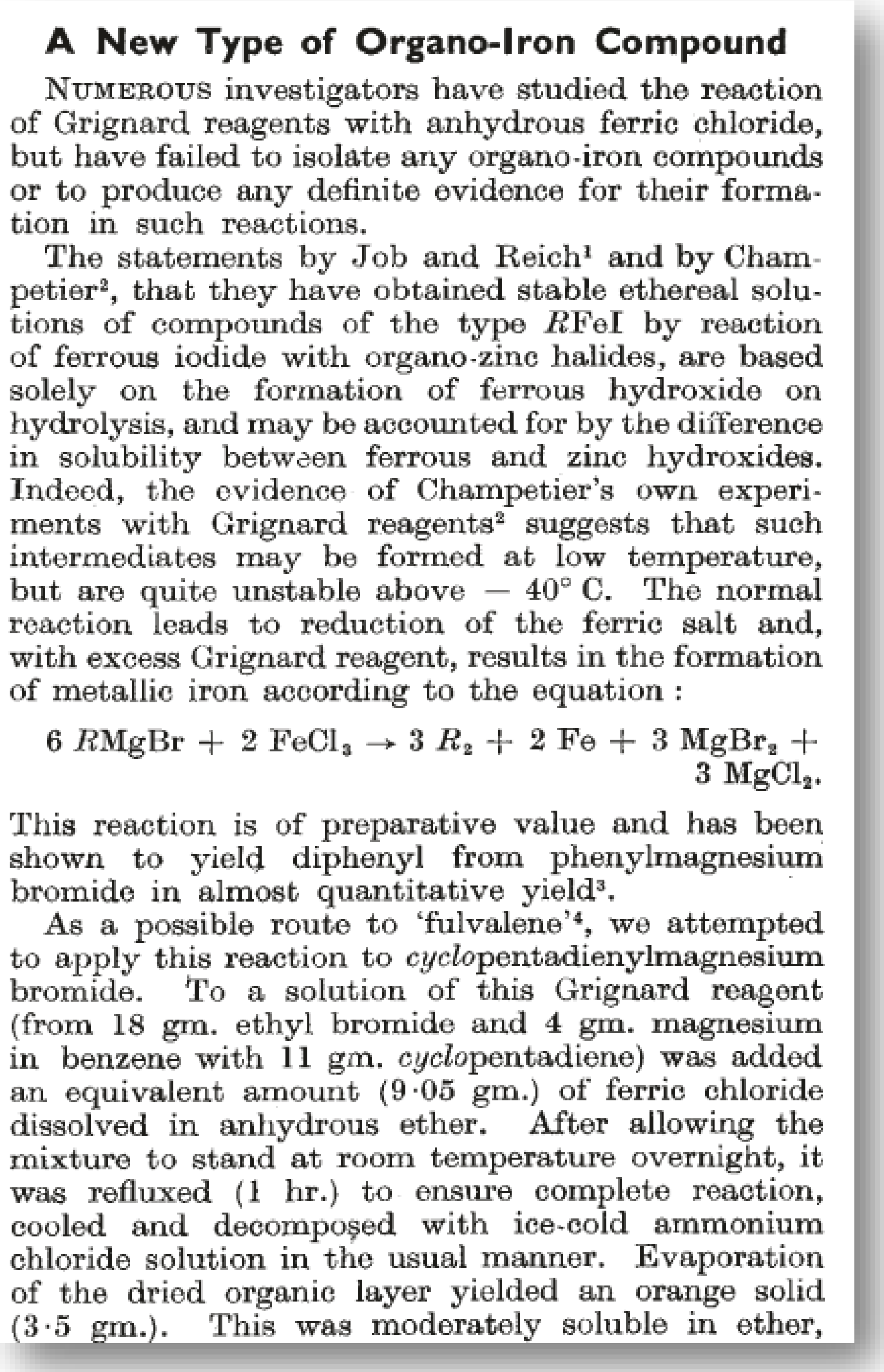
COMPOUNDS containing only carbon, hydrogen, and iron have not hitherto been described, and the direct replacement of hydrogen attached to carbon by iron would not be expected to be feasible. It has now been found that reduced iron, in the form of the well-known "doubly-promoted synthetic ammonia catalyst," can be made to react with *cyclo*pentadiene in nitrogen at 300° and at atmospheric pressure, to give a yellow crystalline compound, of composition $C_{10}H_{10}Fe$. The iron is in its bivalent form, since treatment of a carbon tetrachloride solution of the material with bromine in the same solvent gave a dark green precipitate, which dissolved in water to give a blue solution containing ferrous and bromide ions. The structure of the compound has not been further elucidated, but by analogy with the well-known *cyclo*pentadienylpotassium, it is believed that substitution has occurred in the methylene group, and that the compound has the structure inset.

The compound melts without decomposition at 172·5—173° and sublimes readily, still without decomposition. It is soluble in alcohol, ether, and benzene, but not in water. On concentration of an alcoholic solution on the water-bath, the compound distils in the alcohol vapour; it is also steam-volatile. It is charred by and reduces sulphuric acid, and decolorises acid potassium permanganate.

The conversion of the reduced iron into di*cyclo*pentadienyliron, when a stream of nitrogen containing *cyclo*pentadiene vapour is passed over it, proceeds only for 10—15 minutes. The residual reduced iron is then unchanged in regard to its activity as a catalyst with respect to the synthesis of ammonia at 550°, and the reason for the inhibition of further formation of the iron derivative has not been elucidated. It is possible to revivify the iron by oxidation at 450° in an atmosphere of nitrogen containing steadily increasing proportions of oxygen, and finally in air, and then reduction at 450°. The revivified product can then again be made to produce di*cyclo*pentadienyliron for 10—15 minutes only.

The initial rate of production of iron di*cyclo*pentadienyl, and the total period of reaction before inhibition sets in, can both be increased, resulting in about a 3-fold increase in output, by the addition of 1% of molybdenum oxide to the doubly-promoted catalyst.

A curious feature of several of the preparations of di*cyclo*pentadienyliron was that the crude products contained nitrogen and gave positive results in the Lassaigne test. Purification always resulted in a product free from nitrogen. In view of this capacity to adsorb nitrogen at comparatively low temperatures, some experiments were carried out with 3 : 1 mixtures of hydrogen and nitrogen under a variety of conditions, but the compound was not capable of acting as a vapour-phase catalyst for ammonia synthesis.



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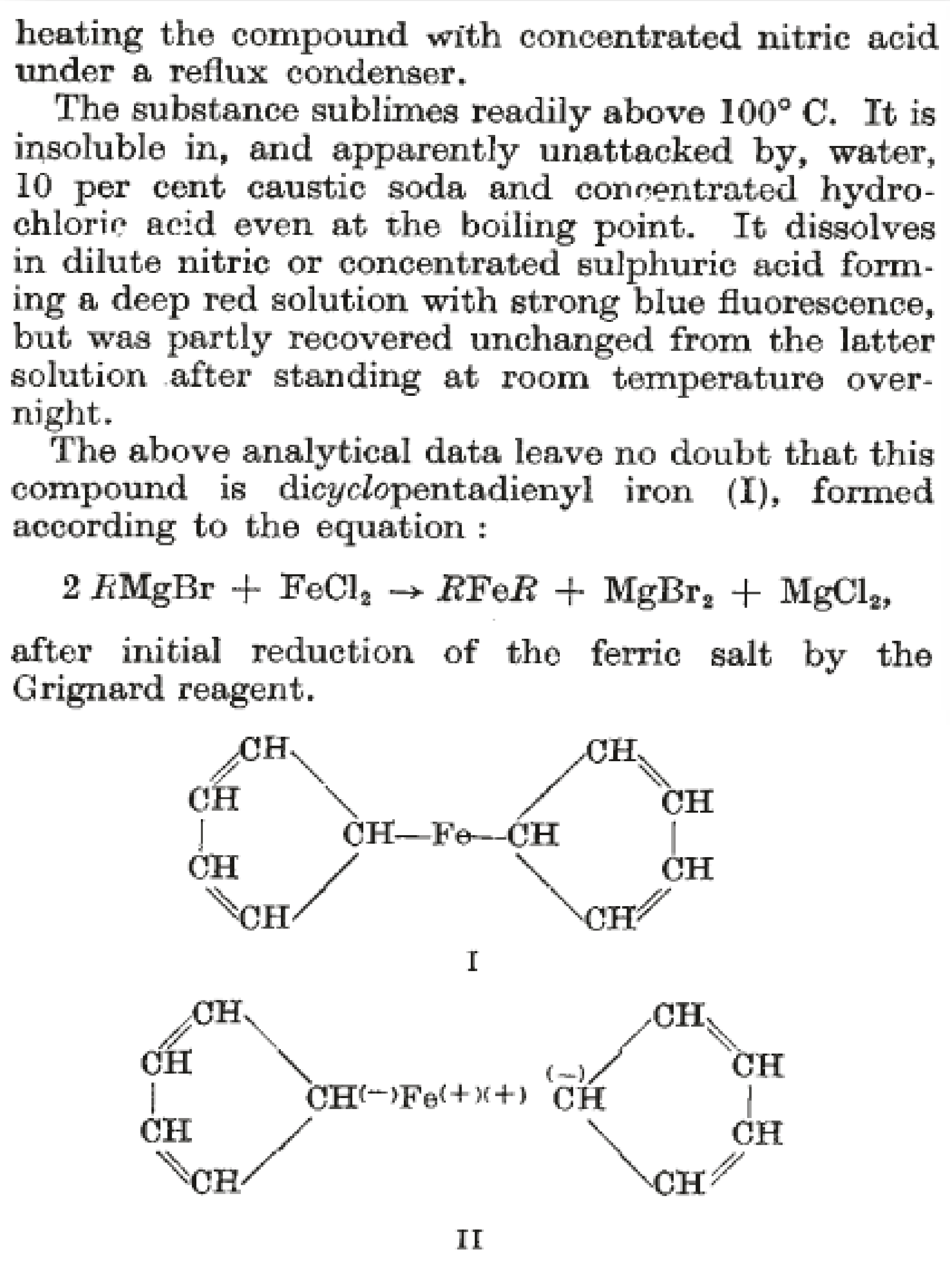


heating the compound with concentrated nitric acid under a reflux condenser.

The substance sublimes readily above 100° C. It is insoluble in, and apparently unattacked by, water, 10 per cent caustic soda and concentrated hydrochloric acid even at the boiling point. It dissolves in dilute nitric or concentrated sulphuric acid forming a deep red solution with strong blue fluorescence, but was partly recovered unchanged from the latter solution after standing at room temperature overnight.

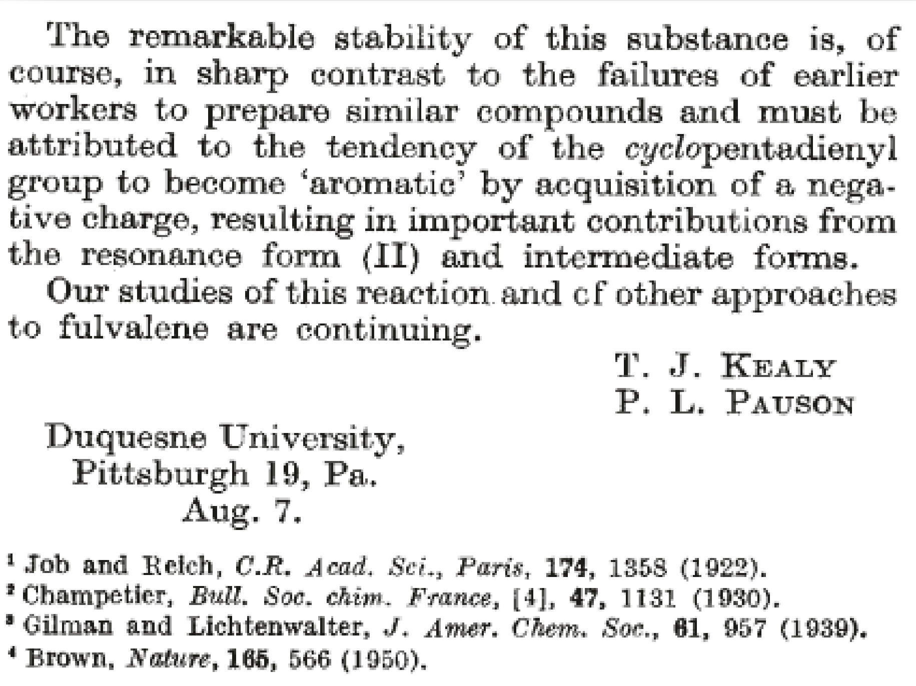
The above analytical data leave no doubt that this compound is dicyclopentadienyl iron (I), formed according to the equation :

$$2\,R\text{MgBr} + \text{FeCl}_2 \rightarrow R\text{Fe}R + \text{MgBr}_2 + \text{MgCl}_2,$$

after initial reduction of the ferric salt by the Grignard reagent.

I

II



The remarkable stability of this substance is, of course, in sharp contrast to the failures of earlier workers to prepare similar compounds and must be attributed to the tendency of the *cyclo*pentadienyl group to become 'aromatic' by acquisition of a negative charge, resulting in important contributions from the resonance form (II) and intermediate forms.

Our studies of this reaction and of other approaches to fulvalene are continuing.

T. J. KEALY
P. L. PAUSON

Duquesne University,
Pittsburgh 19, Pa.
Aug. 7.

[1] Job and Reich, *C.R. Acad. Sci., Paris*, **174**, 1358 (1922).

[2] Champetier, *Bull. Soc. chim. France*, [4], **47**, 1131 (1930).

[3] Gilman and Lichtenwalter, *J. Amer. Chem. Soc.*, **61**, 957 (1939).

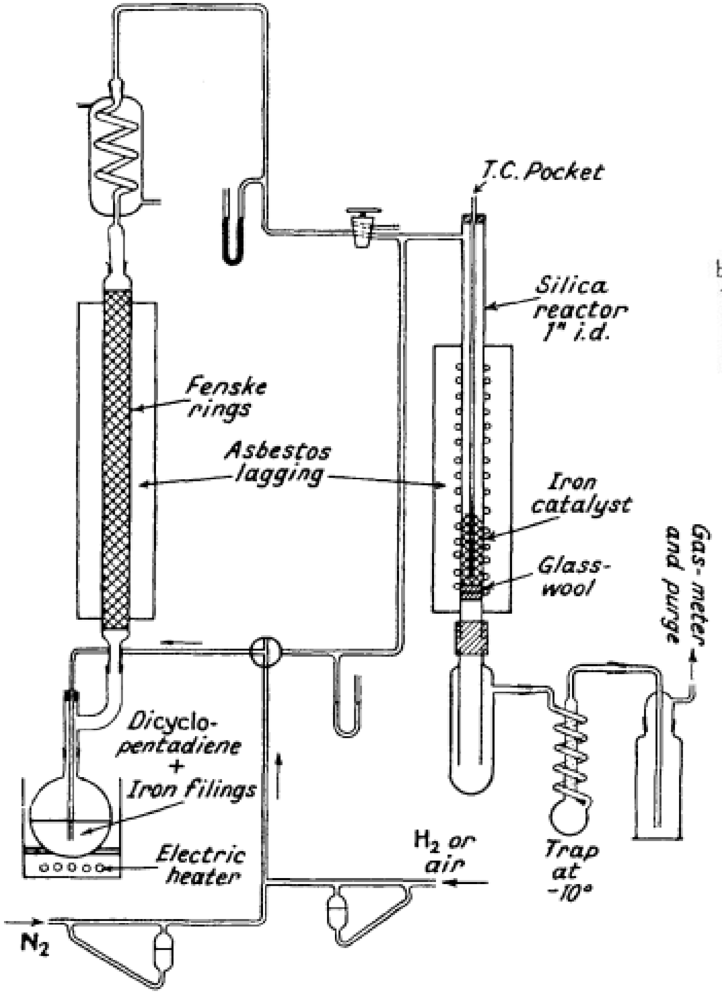
[4] Brown, *Nature*, **165**, 566 (1950).

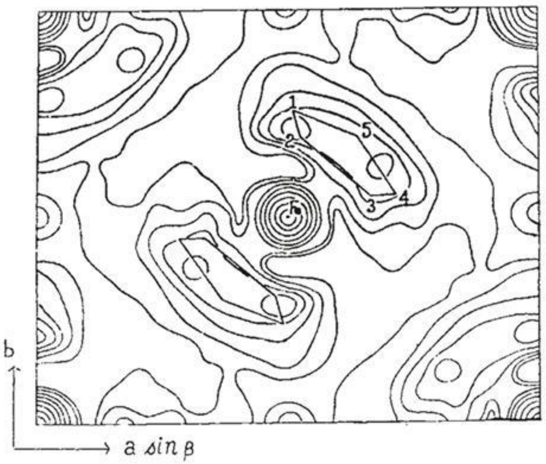
Crystals of ferrocene after purification by vacuum sublimation

Clearly, Kealy and Pauson's paper was wrong but seminal!

Little extra…

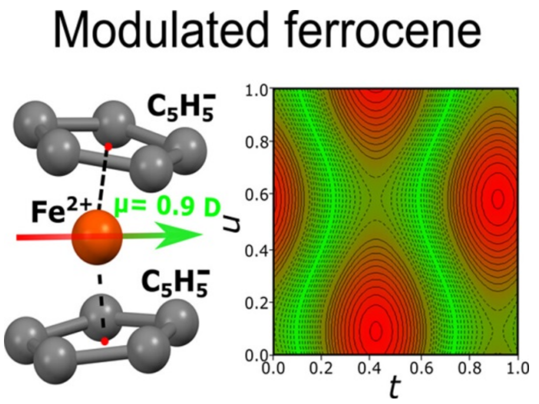
Apparatus for the preparation of iron dicyclopentadienyl.



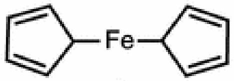


X-ray crystallography proved beyond doubt that ferrocene was a sandwich

Dipole-Moment Modulation in Ferrocene

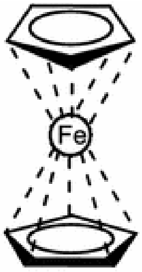


Michal Dušek



2

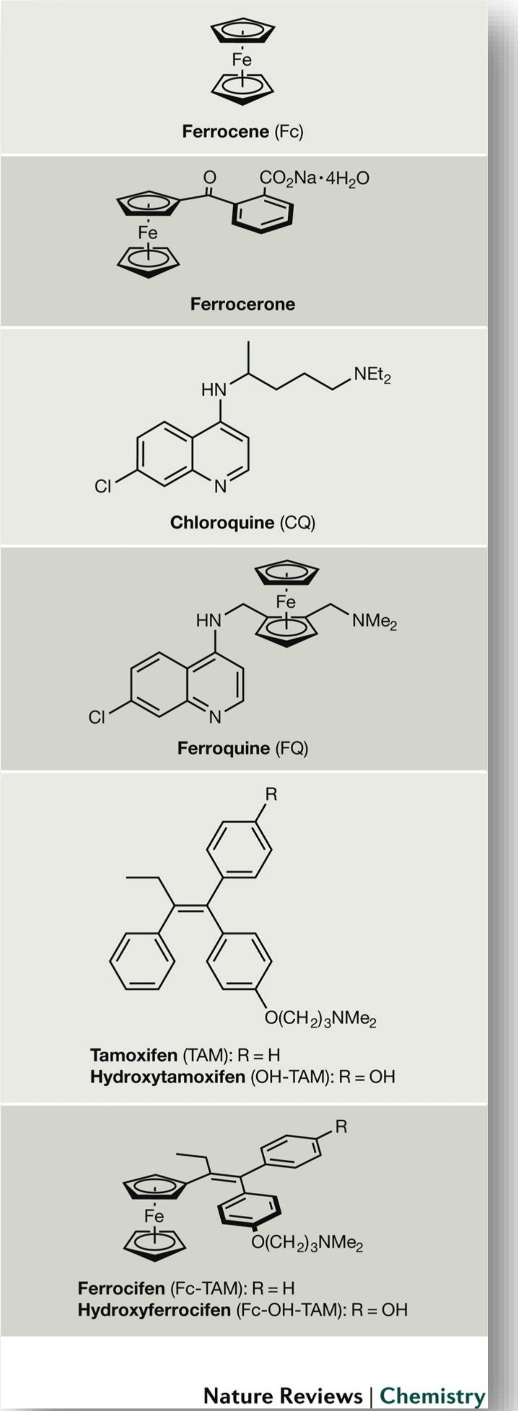
Multiple Independent Errors:
Miller, Teboth and Tremaine
(submitted July 11, 1952)
Kealy and Pauson
(submitted August 7, 1951)

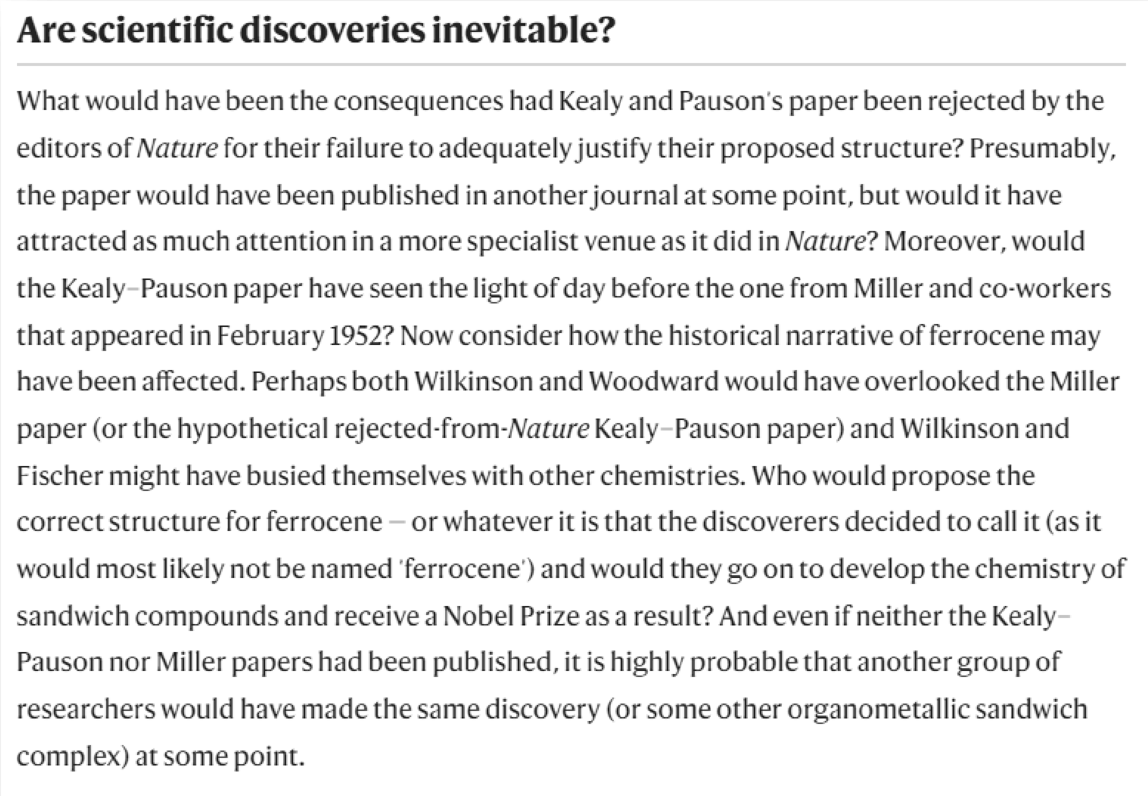


Ferrocene (**1**)

Multiple Independent Discoveries:
Doering (in private discussions with Pauson, September 1951)
Wilkinson (submitted March 24, 1952)
Woodward (submitted March 24, 1952)
Fischer and Pfab (submitted June 20, 1952)
Dunitz and Orgel (submitted July 2, 1952)
Pepinsky (submitted August 12, 1952)

The name 'ferrocene' first appeared in Woodward's second $C_{10}H_{10}Fe$ paper in 1952
(suggested by one of his postdoctoral students, Mark Whiting)



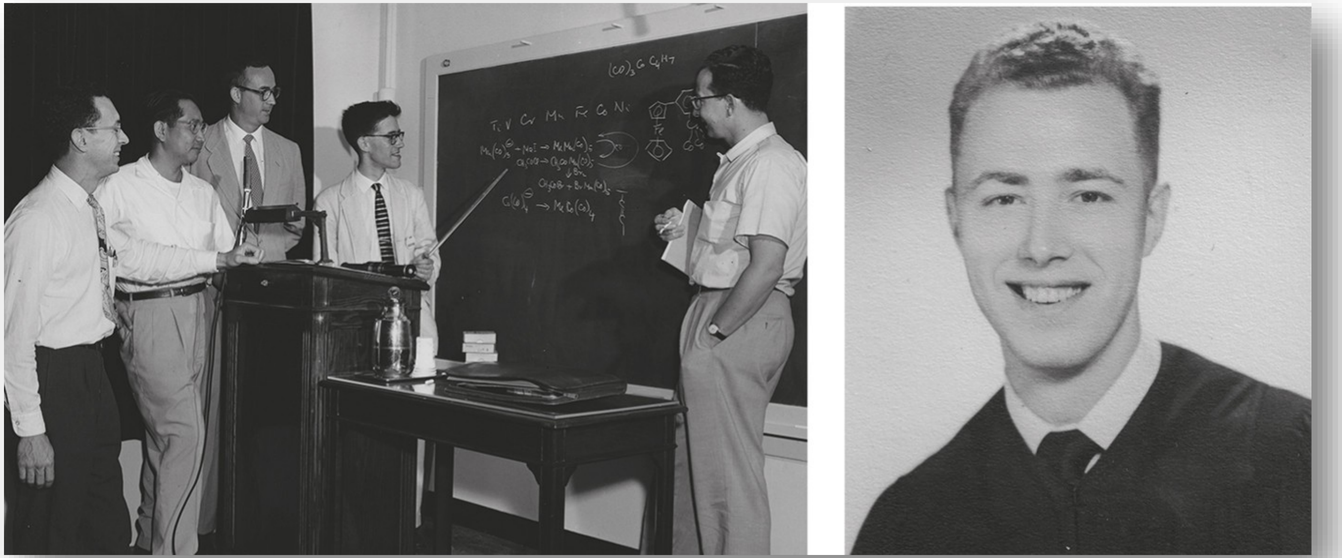


Are scientific discoveries inevitable?

What would have been the consequences had Kealy and Pauson's paper been rejected by the editors of *Nature* for their failure to adequately justify their proposed structure? Presumably, the paper would have been published in another journal at some point, but would it have attracted as much attention in a more specialist venue as it did in *Nature*? Moreover, would the Kealy–Pauson paper have seen the light of day before the one from Miller and co-workers that appeared in February 1952? Now consider how the historical narrative of ferrocene may have been affected. Perhaps both Wilkinson and Woodward would have overlooked the Miller paper (or the hypothetical rejected-from-*Nature* Kealy–Pauson paper) and Wilkinson and Fischer might have busied themselves with other chemistries. Who would propose the correct structure for ferrocene — or whatever it is that the discoverers decided to call it (as it would most likely not be named 'ferrocene') and would they go on to develop the chemistry of sandwich compounds and receive a Nobel Prize as a result? And even if neither the Kealy–Pauson nor Miller papers had been published, it is highly probable that another group of researchers would have made the same discovery (or some other organometallic sandwich complex) at some point.

Nature Chemistry, 2016

Ferrocene derivatives have been investigated as drug candidates for several medical conditions



Left: Pauson, holding the pointer, mid-1950s. Photograph courtesy of Hilary Pauson. Right: Kealy at his graduation, Manhattan College, June 1950. Photograph courtesy of Mary Kealy and Kiki née Kealy Comerford.

Protagonists of the ferrocene story: Peter L. Pauson, Ernst Otto Fischer, Geoffrey Wilkinson, and Robert B. Woodward.

Geoffrey Wilkinson and Ernst Otto Fischer was awarded 1973 Chemistry Nobel prize for Sandwich compounds

Ernst Otto Fischer and Franz Hein talking to each other at Hein's farewell symposium in Jena in September 1969 (shown in the background is Professor Rudolf Taube).

Geoffrey Wilkinson and Ernst Otto Fischer dancing at the final reception of the Conference on Organometallic Chemistry in July 1974 in Ettal (photo courtesy of Professor Wolfgang Beck).

The legacy of the ferrocene story is manifold:

(1) Publication of an erroneous interpretation of otherwise plausible and high-quality experimental data may result in a paradigm shift and novel concepts.
(2) International competition is always stimulating and accelerates the development of a new field.
(3) Application of a discovery may take some time, but eventually "a solution always finds a problem" in chemistry.

Thank you!