Jahns (1955): The Study of Pegmatites, with References to Bowen (1928, 1933)

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February 24, 2021

Introduction

"*The Study of Pegmatites*" (Jahns, 1955) appeared in the 50th anniversary volume of Economic Geology, a publication of the Society of Economic Geologists in the U.S. (available for purchase at https://www.segweb.org/Store/detail.aspx?id=EDOCAV50). In just two years from his first publications in an academic journal (Jahns, 1953a,b), Richard H. "Dick" Jahns was the recognized leader in the field. The table of contents to that volume incorrectly lists Jahns' article with the title "*The Study of Pegmatite Deposits*", as the emphasis of the volume was on the geology of mineralized systems. Jahns' article, however, was not concerned with the economic aspects of pegmatites, as was the wartime study of Cameron et al. (1949). Jahns (1955) was meant as a review and treatise on the geologic processes that make pegmatites according to those whose studies preceded him, and it served as a platform upon which to present his own evolving views on the internal evolution of pegmatites.

Jahns (1955) contains 698 citations. To those who are not part of the academic realm, and for students too young to know life before the World Wide Web, the effort to track down and read or at least review that many published works in just a few years is an achievement in itself [1]. One started with a physical review of journals in paper on stands in libraries and subject indices of books in card catalogs. From each article or book, a researcher followed citations therein to create a cascading array of references. Everything had to be found by searching the stacks, pulled, and read or photocopied for later use. It can work the same way today, except that the physical effort of finding pertinent works is greatly reduced by the use of search engines like GeoRef and GeoScienceWorld (I can find all 178 published works on the subjects "pegmatite" and "Poland" within 2 seconds), and many of the papers are immediately available in PDF. In most instances, Jahns cited authors and the number associated with that citation in the list of references. For example, from page 1057, quite a few passages read like this:

"Excellent examples have been described from Norway (Brögger, 75, 76), England (Sargent, 524), Namaqualand (Gevers, 208), Nigeria (Jacobson and Webb, 317), the Belgian Congo (Thoreau, 626), Russia (Shsherbakov, 578; Strelkin, 620), India (Biswas, 52; Roy, Sharma, and Chattapadhyay, 523), Canada (DeLury and Ellsworth, 130; Rowe, 519-521), and the United States (Maurice, 410; Chapman, 105; Hunter, 306; Heinrich, 256; Cameron et al., 94)."

In other sections, Jahns cited only the reference number in the text, not the authors, which is a method that is normally reserved for short articles with few citations. For example:

"Detailed structural analysis has figured prominently during the past fifteen years in studies of pegmatite mineral resources in North and South America by the U. S. Geological Survey (596, 462, 454, 455, 333, 127, 95, 337, 338, 320, 561, 456, 99, 562, 563, 619, 248, 482, 334, 247, 466, 332, 3, 96, 610, 628, 575)." (p. 1044)

In that case, the authorship is not immediately evident. Conversely, if one wants to know what is attributed to Cameron et al. (1949), one has to search the text for citation "94." With only 82 pages of text, this large number of citations and the use of numbers only obfuscates the reading. The large number of citations, however, served to establish Jahns' authority in the field. The sheer number of studies cited conveyed widespread interest in a subject that merited further study. Reading is learning, and learning fosters knowledge. For most of the first 50 years of pegmatite studies, authors spoke their individual opinions. Cameron et al. (1949) was the first publication to comprehensively review the work of others (121 citations). That assessment leant credibility to their publication, though it was the exhaustive field campaign on which Cameron et al. (1949) relied. Cameron et al. (1949) applied good scientific method– an appraisal of multiple working hypotheses to solve complex problems – to the study of pegmatites. They gave due credit to those whose similar conclusions preceded them. Jahns (1955) followed the same course, to a point. Nonetheless, his familiarity with or awareness of prior studies was unmatched by any other practioners of his time.

DISCOURSE

The first portion of Jahns (1955, pp. 1027-1058) is an amalgamation of historical development of the field, empirical observations, and interpretive hypotheses. In "*Source and Mode of Emplacement*" (p. 1027-1067), the treatment is more interpretive. Like Cameron et al. (1949), Jahns (1955) mostly tackled the igneous versus metamorphic origins of pegmatites. Here, he was more equivocal than Cameron et al. (1949) and in Jahns (1953a), and he offered this enigmatic conclusion (p. 1067):

"Review of the arguments presented by these and other investigators leaves the reader still groping for satisfactory generalizations as to the quantitative importance of different processes. Magmatic action has been emphasized in the great majority of discussions, but this might mean only that metamorphic pegmatites have received relatively little attention. Further, one is tempted to raise the somewhat irreverent question as to how many arguments have begun, rather than ended, with the notion of a dominantly magmatic or a dominantly metamorphic origin."

Jahns (1955) seemed to suggest that some individuals have been so convinced of a concept that they have stated it ahead of the evidence, much as Jahns (1953a) did (see essay #5 of this series).

Jahns (1955) laid out four principal hypotheses (p. 1070) regarding the internal development of pegmatites after emplacement:

(1) development by metasomatism of pre-existing rock;
(2) development by deposition from "*pegmatitic fluids*" coursing through open fractures;
(3) development by crystallization from a "*pegmatitic magma*" in a closed system;
(4) development by any of the three foregoing mechanisms followed by hydrothermal replacement of prior mineral assemblages.

Here, as in Cameron et al. (1949) the term "*pegmatitic fluids*" appears to carry the connotation of an aqueous solution, whereas "*pegmatitic magma*" implies a silicate liquid. The former has low viscosity and low solute content; the latter has high viscosity and high solute content. The former

can exist stably as a fluid down to the freezing point of aqueous solutions; the latter freezes to glass, a semisolid, at temperatures in the range of 300°-400°C, if it has not already crystallized.

"*Compositions of the Fluids. – Most pegmatites seem to be products of silicate and aluminosilicate systems in which volatile constituents were present, and their range in gross composition differs little from that of ordinary igneous rocks.*" (p. 1071)

Jahns' use of "*fluids*" and "*silicate and aluminosilicate systems*" here eluded the clear meaning that those terms should have had: hydrous alkali aluminosilicate liquid, a.k.a. granitic melt. That said, Jahns (1955) spent several pages discussing the nuanced differences among pegmatites, noting (p. 1076), for example, those that are anomalously rich in fluorine and rare earths (later characterized as the Nb-Y-F family of pegmatites by Černý, 1991) as opposed to those that are notably rich in lithium, boron, and beryllium (Černý's Li-Cs-Ta family). Jahns did not, however, stray far from the conclusion that the compositions of pegmatites, other than those associated with mafic or syenitic plutons, are granitic in composition.

Temperature and Pressure
A distinguishing feature of Jahns (1955) was his effort to review and assess quantitative measures of pegmatite formation, as opposed to the intuitive understanding that had dominated publications to that point. The section on "*Temperature and Pressure*" (p. 1077) began that pursuit as an inquiry of methods. Investigations of mineral stability came into play. He cited Bastin (1911) for a study of the morphological habits of quartz in pegmatites of Maine, which incorrectly associated the long axes of quartz in graphic granite with their *c*-axis direction of growth (it is not: see London, 2008). "*Most specimens of quartz that have been tested* [read: for which the morphology indicates its growth as alpha or beta form] *during recent years evidently were formed at temperatures below 573°C* [read: the alpha-beta inversion temperature at one atmosphere]. He cited Roy at al. (1950) for having "*pointed out that pegmatite spodumene must have formed at temperatures below 500°C, or at slightly higher temperature under elevated pressures, as the high-temperature modification of this mineral* [read: β-spodumene] *is not known in natural occurrences.*"

Jahns' (1955) review of scientific approaches to the temperatures of crystallization within pegmatites included fluid inclusions in minerals:

"*The use of fluid inclusions as geologic thermometers has had its ups and downs since it was first suggested by Henry Sorby about a century ago... Lively discussion of the method has appeared in many recent publications... and needs no discussion here.*" (p. 1079)

Nonetheless, Jahns (1955) went on to cite several results that "*indicate the considerable variation in results obtained by the inclusion method, which at present seems to be most satisfactory for minerals formed at relatively low temperatures.*" (p. 1080)

Jahns (1953a) put the temperatures of crystallization in pegmatites "*almost certainly below 600°C.*" (p. 597), though supporting evidence was not mentioned. By the time of this publication, the early experimental studies on several chemical joins in the hydrous granite system, which Jahns came to rely on for direction (introduced on p.1078), were in progress. This

included the study of the alkali feldspar system $NaAlSi_3O_8$- $KAlSi_3O_8$-H_2O (Bowen and Tuttle, 1950), from which Jahns (1955) observed that the primary feldspars in pegmatites are subsolvus. He also noted that the morphology of quartz was likely that of the low-temperature alpha modification. For these reasons, Jahns' (1955) once again assigned the thermal state in zoned pegmatites to "...*temperatures not greatly above 575°C.*" (p. 1080)

Jahns (1955) had little to say about the pressures attending the crystallization of pegmatites, for the good reason that sound evidence was lacking at the time. He had previously remarked that the pressures should be great enough to prevent the major escape of volatile components (Jahns, 1953a). Jahns started to draw parallels between the experiments at moderate pressures, mostly in the range of 200 MPa (2,000 bars) as useful analogues of the pegmatite-forming environment. The key role of elevated pressure was in relation to the effect of H_2O fluid pressure on the melting and crystallization of silicate systems:

"The effect of water in lowering the liquidus-solidus temperature in silicate melts has been clearly shown by experimental studies..., and is further attested by the crystallization temperatures of pegmatite themselves." (p. 1081)

The use of the term "*water*" is unfortunate in a topic that has had more than its share of vague and variable references to the states of its fluid phases. Jahns (1955) used it when describing H_2O as a component of the melt and as a separate aqueous solution. Later, he acknowledged that "*it is important here to distinguish between magmatic rest-liquids* [read: hydrous melts] *and aqueous solutions derived from them.*" (p. 1082). Thereafter, H_2O as a component of melt or as the dominant component of an aqueous solution took center stage in Jahns' work.

Pegmatite-Forming Fluids
Much of the important text of this monograph lies between pages 1080-1084, as Jahns sorted through the various hypotheses and limited experimental evidence for the properties of hydrous silicate liquids and their evolution through the process of fractional crystallization. For Jahns, the issue largely boiled down to the nature of the transition from melt-only to aqueous-only states. From the earliest studies in the field, the transition from melt to aqueous solution was thought to be continuous, meaning supercritical in state, with no separation of fluid phases over that span (e.g., Fersman, 1931; Lindgren, 1913, 1937). The growing evidence from experimental petrology indicated that a miscibility gap between aqueous solutions and silicate melts likely exists at moderate crustal pressures. That miscibility gap reflects a finite limit on the solubility of H_2O in silicate liquid, such that increasing H_2O content would result in the exsolution of a coexisting aqueous solution. For this, Jahns (1955) cited an early experimental study from Smith (1948) for the existence of a miscibility gap in the system containing H_2O and a putative pegmatite-forming melt at an estimated pressure in the range of 200 MPa (no actual measurement). Smith's (1948) work was as irrelevant as it was innovative: irrelevant because of the bizarre compositions used as proxies for pegmatite-forming melts, innovative for how Smith came to those compositions and deduced the results of the experiments. Jahns (1955) concluded that:

"...pegmatitic fluids can persist as liquids to very low temperatures, that resurgent boiling is to be expected in many pegmatite-forming systems, and that the very real possibility that late-stage fractionation into two immiscible liquids must be considered." (p. 1083)

Unfortunately, Jahns (1955) could not escape the ambiguity of the times (also see my essays on Cameron et al., 1949). Jahns had been precise about the terms "*pegmatitic magma, hydrous magma, and rest-magma*" (p. 1081) in reference to silicate liquid, and he referred to "*aqueous solution*" and "*vapor*" (p. 1084) as something else and separate. His phrase "*pegmatitic fluids… as liquids*" presumably refers to silicate melt. The origins of the term "*resurgent boiling*" are obscure. I have not found a source in Bowen (1928, 1933), nor from on-line searches through physical chemistry, and Jahns (1955) cites no source. The terms resurgent boiling, retrograde boiling, and second boiling are deemed as equivalent. As used by Jahns (1955), "*resurgent boiling*" results from the crystallization of anhydrous minerals from hydrous melt upon cooling. The composition of the melt would either evolve continuously above the critical or consolute point of the system into an aqueous solution, or "boil" when the H_2O content of the melt exceeded its solubility on a solvus between melt and aqueous solution.

The term retrograde boiling is in reference to decreasing temperature, but hydrous melts do not necessarily vesiculate ("boil") upon cooling at elevated pressure. Many hydrous obsidians are not vesicular (though rhyolite domes have a tendency to explode after their emplacement), and the H_2O content dissolved in silicate melts can be readily quenched in the glass in experiments. It is actually crystallization, not cooling, that brings silicate liquids to their "*resurgent boiling*" or second boiling point. The term first boiling is in reference to the exsolution of a volatile component through decompression of a liquid, as for example the vesiculation of CO_2 upon opening carbonated beverages, or ascent of a volatile-bearing magma.

If Jahns' (1955) reference to "*resurgent boiling*" is correctly associated with a miscibility gap in which two fluids – aqueous "*vapor*" or "*solution*" and silicate "*liquid*" – coexist, then the third state of "*two immiscible liquids*" indicates that Jahns (1955) was considering the possibility that a second dense liquid forms, with or without the creation of a third, low-density vapor phase.

Before laying out his own vision of the pegmatite-forming processes, Jahns (1955) cited Bowen (1933) as providing an informed opinion in support of it. Bowen (1928), however, was not an advocate for an important role of a low-density aqueous solution as a principal means of achieving the chemical evolution of a melt or the crystalline assemblage. From "*The Evolution of the Igneous Rocks*", the chapter entitled "*The Importance of Volatile Constituents*" contains Bowen's frequently cited admonition:

"*To many petrologists a volatile is exactly like a Maxwell demon; it does just what one may wish it to do.*" (p. 282)

"*An action frequently appealed to in present-day writings as a cause of differentiation of igneous rocks is gaseous transfer. Most petrologists leave one very much in the dark as to what they mean by the terms.*" (p. 293)

"*…deep bodies have sent dikes into surrounding rocks at all stages of their careers and these dikes are often, at least at their margins, aphanitic or glassy. They have thus crystallized so rapidly that if the liquid were vesicular the aphanite would be vesicular as well. Yet vesicular dikes or dike selvages are exceedingly rare and are apparently always found in association with*

flows which indicate for them a shallow depth… It is very much to be doubted, therefore, that any process which involves vesiculation can be the principal or even an important process in igneous differentiation. But even on the supposition that vesiculation is a common condition in deep-seated magma and that for some reason its evidence is concealed from us, the quantitative adequacy of the process is seriously in doubt." (p. 294)

In "*The Broader Story of Igneous Differentiation, Briefly Told*", Bowen (1933) devoted a substantial portion of the discussion to the pegmatitic stage of igneous evolution because of its presumed relationship to the formation of ore-bearing veins. By this time, and in this case, Bowen was an advocate for the important role of "*second boiling*". His change of heart, if it might be called that, came from early experiments by R.W. Goranson (1931) that determined the limit of solubility of H_2O in a liquid of the composition of the Stone Mountain granite, Georgia (USA) at elevated fluid pressure. The solubility of H_2O in silicate liquid was later reinvestigated by Burnham and Jahns (1962). With Goranson's (1931) initial results, and starting with known or theoretical amounts of H_2O and a particular crystallizing assemblage, petrologists like Bowen could predict at what point in the crystallization of a silicate magma that a separate aqueous phase could be expected to "boil" out of it. This is precisely what Jahns and Burnham (1969) did in their famous article. Bowen acknowledged that boiling was not a necessary consequence of crystallization, but that it was the likely case:

"*within the liquid…'gas bubbles slowly rise and suffer selective (fractional) condensation. Close to the boiling source the liquid will be continually enriched in hyperfusible components that are relatively non-volatile; at points more and more remote the liquid will be increasingly richer in those hyperfusible constituents that are more volatile'…*" Bowen (1933), p. 123.

Thus, Bowen (1933) envisioned the "*gas bubbles*" as a transfer medium, taking volatile constituents where the bubbles formed and returning them to the melt at "*points more and more remote*." Insofar as this is the essence of the model that has long been attributed to Jahns and Burnham (1969), it originated with Bowen (1933). In the same passage, Bowen (1933) added what was meant to be a defining comment regarding that transfer process:

"*In an actual example the vapor formed will move only a very short distance before it encounters a cooler environment where it is partly or wholly condensed, becoming part of the liquid there existing.*"

Bowen (1933) seemed to suggest that a vapor that has exsolved from a location in the melt at high temperature would be resorbed in a nearby domain of the same melt at lower temperature. That would be the case only if that cooler melt is undersaturated in the components of the vapor, which would then necessitate some sort of gradient in the concentration or chemical activity of H_2O in the melt from saturated to undersaturated domains. A cooler domain of the melt is more likely, not less, to be saturated in H_2O as a result of crystallization (second boiling). Bowen also appears to have presupposed a temperature gradient that decreases upward. That might be the case over long distances in large magma bodies, or near the upper contacts of any magma bodies with their host rocks, but not their lower contacts, as in the case of thin, subhorizontal pegmatite dikes. Bowen likely had a mental image of what he tried to convey in words. Whatever Bowen (1933) envisioned, this appears to be the source of Jahns' (1955) repeated reference to

"*condensed vapors.*" Bowen (1933) also introduced the term "*hyperfusible*", later used by Cameron et al. (1949), Jahns (1953a), and this work, to describe constituents that contribute to a low solidus temperature of igneous liquids. Bowen (1933) appears to have had a significant impact on Jahns' (1955) thinking.

Jahns (1955) continued to expound on the problems with nomenclature to describe the physical and chemical state of the pegmatite-forming system:

"*Discussions of pegmatitic fluids have been complicated in a most unfortunate way by differences in the usage of certain terms.*" (p. 1083), which he described as "*the semantic swamp that extends across the field of late-magmatic and post-magmatic processes.*" (p. 1084)

Jahns took exception to the term "*pneumatolytic*", which refers to the transfer of magmatic components through an exsolved aqueous solution, and normally entails metasomatic replacement of surrounding rocks at temperatures close to those of the magmatic liquidus. Greisenization, a widely used and widely misused term, has been commonly associated with a pneumatolytic stage in the evolution of granitic magmas. In the early applications of the terminology, the pneumatolytic stage fell between pegmatitic and hydrothermal stages in the continuous evolution of a silicic magma (e.g., Lindgren, 1913, 1937). By discounting "*pneumatolytic*" as a meaningful description of state, Jahns was left with magmatic "*liquid*", hydrothermal "*aqueous solutions*", and "*pegmatitic fluids*".

In the section on "*Crystallization*" (pp. 1084-1091), Jahns (1955) once again laid out the arguments for the principal conclusions of Cameron et al. (1949) and Jahns (1953a):

- ➢ pegmatites crystallize from their margins to center,
- ➢ once emplaced, pegmatites crystallize in an essentially closed system,
- ➢ the giant crystals and rare mineral assemblages of pegmatites are essentially primary, and not formed by replacement of pre-existing rock.

The section ends with a return to the subject of inclusions within minerals, and quite specifically the occurrence of aqueous inclusions together with crystalline ones. Jahns (1955) reiterated that inclusions are "*difficult to interpret because of uncertainties in recognizing those that represent material trapped during the growth of the host crystals*" (p. 1089), which presupposes that such primary inclusions do in fact exist. He cited verbatim the opinion of F.G. Smith (1953), an early pioneer in fluid inclusion research, that "*most of the liquid inclusions in pegmatite minerals are of secondary (= subsequent) origin and the compositions of such inclusions may bear no simple relation to the composition of the liquid present during the first crystallization.*" (p. 1090)

Jahns (1955) then stated that "*The present writer (328) has reached somewhat similar conclusions on the basis of studies of minerals from several pegmatite districts, and suggests that three general types of primary inclusions may be rather widespread*" (p. 1090)

These are not "*somewhat similar conclusions*", as Smith (1953) found little evidence of a primary origin of fluid inclusions in minerals from pegmatites, and mostly evidence to the contrary. The citation to Jahns (328) is given in the list of references as:

"Jahns, R.H., 1955, The genesis of pegmatites. IV. The role of replacement processes. V. Conditions and processes of crystal growth: manuscripts in preparation." (p. 1118)

along with a third article

"Jahns, R.H. and Campbell, D.D., 1955, The genesis of pegmatites. III. Trends in the distribution of rare elements: manuscript in preparation."

To those not familiar with academic publishing, it is unconscionable in modern times to cite works in references unless these have been submitted, reviewed, and formally accepted for publication by the editor. It is also a duty of editors to see that such citations do not appear as matters of fact if in fact they do not exist. Authors can make reference to "unpublished data" in the body of text, but as such, those "data" carry little weigh until they have been fully presented and, importantly, successfully undergone peer review. Here, Jahns cited three articles in "*The Genesis of Pegmatites*" series that were never published, and may not have been written. I can say that I have encountered no other author of that or any other time who cited unpublished work as entries in a list of references. I asked Dr. Kenneth L. Taylor, Emeritus Professor of the History of Science at the University of Oklahoma with a specialization in geosciences, if the modern standard of citations might have been different in Jahns' time. In his opinion, the current strictures and expectations of citation were likely in force at least from the start of the 20th century, if not earlier. Jahns used the same device in a later article from 1963, which will be reviewed in another essay. By that time, the standard surely was one of adherence to published works only in any list of references. Jahns' methods might have been innocent gestures of good intentions, though no other authors I am aware of used the same ploy. Whatever the intent, they had the effect to mislead readers into thinking that the conclusions stated were not only factual but that those facts would be verified through the publication process, when in Jahns' case, they never were. In this manuscript, all of the most important of Jahns' conclusions rest upon the contents of citation 328, which comes up repeatedly through the text.

Jahns (1955) described the three types "*primary*" inclusions as:

> ➢ *"solid inclusions... probably trapped as solid material.*
> ➢ *solid inclusions (with or without recognizable fluid phases)... probably trapped samples of pegmatite liquid (mother liquor).*
> ➢ *fluid aqueous inclusions... probably trapped samples of a vapor phase in equilibrium with the pegmatite liquid, or possibly samples an immiscible aqueous liquid.*

If and when it can be established that inclusions of categories 2 and 3 are indeed primary, and that they are accurate samples of the fluids present during growth of the crystals, they should prove very useful in deducing the nature and behavior of these fluids during the formation of pegmatites. (p. 1090)

It would not be Jahns, however, that made that case. He did not follow that path, perhaps because the study of inclusions was already fraught with uncertainty in interpretation, and because Frederick G. Smith, a recognized expert, had already pronounced them as mostly of secondary

origin in pegmatite minerals and of no use in deciphering the main stage of pegmatite formation. Jahns' (1955) presentation, however, created the impression that he had found evidence for coexisting silicate and aqueous fluids in the form of primary inclusions in pegmatite minerals, and his model could go on from that point.

Internal Processes
Jahns (1955) began the section "*Differentiation and Segregation of Elements*" (pp. 1091-1095) with a reprise of the systematics of the zoning sequence in pegmatites that was expounded by Cameron et al. (1949), and which was offered as evidence that pegmatites form by igneous rather than metasomatic processes. Jahns (1955) noted for the first time the problem of quartz cores as the last primary unit when "…*sialic rest-magmas should have a bulk composition corresponding at least approximately to the thermal trough in the fusion surface of the alkali-aluminum-silica-water residual system.*" (p. 1093); that is, with the composition of the thermal minimum or eutectic in the hydrous haplogranite system: $NaAlSi_3O_8-KAlSi_3O_8-SiO_2-H_2O$. He proposed that

"In this writer's opinion (328), a quartz core would be the normal end product of crystallization of a pegmatite liquid undergoing resurgent boiling. The alkalis in such a system would be progressively removed in the vapor phase, whereby silica would be progressively enriched in the liquid phase, perhaps to very low temperatures." (p. 1094)

Was Jahns' (1955) proposing that the melt composition evolved toward silica at low temperature as alkalis were removed, or did he mean that the precipitation of quartz was a result of the displacement of the composition of the melt from that of the thermal minimum in the hydrous granite system? The citation, 328, was never published, so we will never know. The difference matters. In the former case, Jahns (1955) cited the study of Tuttle and England (1955) that placed the melting temperature of quartz above ~ 1,100°C at elevated H_2O-pressures. A hydrous melt of the composition of pure quartz would be untenable at the temperatures that Jahns inferred for pegmatite crystallization. In the latter case, Jahns was aware of the evolving experimental results in the hydrous granite system (Bowen and Tuttle, 1951) and the likely "*composition corresponding at least approximately to the thermal trough in the fusion surface of the alkali-aluminum-silica-water residual system.*" The loss of alkalis, or a shift of composition in any direction from the thermal minimum, would drive crystallization as the melt became increasingly undercooled in relation to its liquidus temperature. But, the removal of alkalis alone would not cause a granitic liquid to move into the liquidus field of quartz only, because the melt would become increasingly peraluminous as well.

The component of alumina is the key to understanding the magmatic versus hydrothermal origins of quartz-rich rocks. Alumina is vanishingly soluble in aqueous solutions, especially when chloride is added (Anderson and Burnham, 1983). Because of the extreme insolubility of alumina in hydrothermal solutions, metamorphic petrologists have long balanced mineral reactions to conserve Al, meaning that it is treated as completely immobile. In the case of Jahns' proposed model, alumina would be enriched in the residual melt to a greater extent than silica, and peraluminous minerals would be prevalent in the last-formed core unit. Burnham (1979) entertained the loss of alkalis to aqueous vapor as a means by which subaluminous, what would be termed subduction-related I-type granites, become peraluminous toward the end of their crystallization. Burnham's (1979) model for the evolution of porphyry deposits entailed the

precipitation of quartz in hydrothermal veins, but those veins lack aluminous minerals including micas and feldspars. Burnham and Nekvasil (1986) stated that quartz cores in pegmatites must be hydrothermal, not magmatic, on the grounds that the cores lack aluminous minerals.

Metasomatic Replacement

In "*The Role of Replacement Processess*" (pp. 1096-1098), Jahns (1955) recanted his previous statements that the uniform sequence of zones among thousands of pegmatites argues for their direct crystallization from silicate liquid, and that replacement assemblages generally account for far less than 1 % of the volume of any given pegmatite body. Jahns (1955) stated:

"*In the present writer's opinion, widespread reaction between crystals and rest-liquid must have taken place in many bodies of magmatic pegmatite, and lesser amounts of replacement must also have been accomplished by hydrothermal solutions formed either by direct descent or by immiscible separation from the rest-liquid at very last stages. Neither of these processes, however, appears to offer a satisfactory explanation of the many impressive aggregates of replacement material, which seem much better attributed to the activities of condensed vapors formed by resurgent boiling of rest-liquid... Readily explained in terms of this process are the observed sequences of many replacement minerals, as well as the occurrence of replacement bodies that can be trace sourceward toward the centers of the host pegmatites bodies. More important is the sound explanation it provides for some otherwise troublesome features, such as the temporal overlap in the formation of zones and replacement bodies in many masses of pegmatite, discrepancies between the composition of many interior zones and the composition that would be expected if they had been formed within a system whose liquid phase was yielding solid material only, and the vigorous corrosion and replacement of quartz, potash feldspar, and other minerals simultaneously with crystallization of the same minerals at other points within the same pegmatite bodies.*"

It is not clear how or why Jahns (1955) distinguished "*hydrothermal solutions formed ... by immiscible separation from the rest-liquid*" from "*condensed vapors formed by resurgent boiling of rest-liquid.*" These appear to be one and the same process. The pressure-temperature locus of the liquid-vapor curve and the critical end point of H_2O were already known, and far below the pressures and temperatures inferred for the crystallization of pegmatites. Nor is it clear that there is a problem that needs to be solved by this process, except for Jahns' new claim that replacement phenomena are widespread and that replacement represents a significant stage of pegmatite genesis. This is a radical departure from Cameron et al. (1949), Heinrich (1953), and Jahns (1953a,b), all of whom stressed that the highly systematic sequence of zonation within pegmatites is consistent with direct precipitation from a silicate liquid.

Jahns (1955) then addressed the "*Development of Special Features.*" Giant crystals (p. 1098) were the subject of Jahns (1953a), and here he conveyed his vision for their environment of growth:

"*If the giant crystals are indeed primary in origin, they must have developed from highly fluid melts over narrow ranges of temperature, with growth rates that probably were extremely rapid in terms of geologic time, according to Jahns (328). Many of them seem to have been oriented in response to temperature gradients... Worthy of consideration is the possibility that the formation*

of most giant crystals corresponded to the period during which the pegmatitic liquid was undergoing resurgent boiling."

High fluidity of the melt, narrow ranges of temperature, and rapid growth are all stated with reference to presumed evidence in the unpublished work of Jahns (328). None of it had been determined by experimental or theoretical means when he wrote this manuscript. Jahns used turns of phrase, such as "*must have developed*", to give a sense of factual knowledge, as if all other options had been considered and discarded when weighed against the evidence. Here, just a few pages after introducing the term "*resurgent boiling*" as a process, Jahns made it a *de facto* "*period*" in the evolution of pegmatite-forming melt.

Jahns' (1955) discussion of "*Graphic Granite and Other Intergrowths*" (p. 1099) is surprisingly brief, considering that graphic granite was the original defining texture of pegmatites, it is widespread in pegmatites of all types, and it is the only texture found in pegmatites that is unique to pegmatites (London, 2008). Jahns reviewed the opinions of the time, most of which argued that the intergrowths of quartz in feldspar that define graphic granite are a product of simultaneous crystallization of the two minerals, but Jahns did not weigh in with his own opinion. It came to light during Jahns' life, and within his research group, that graphic granite is in fact the product of crystallization from highly viscous and H_2O-undersaturated silicate liquids (Fenn, 1986).

Jahns' (1955) review of "*Cavities*" (p. 1099-1101) concluded that they must form by more than one process. Some are evidently "*primary features*", while others appear to have developed by solution of pre-existing minerals. They are, nonetheless, rare, and "*occur in less than one percent of all known pegmatites.*" That estimate might be closer to 0.01 percent of all known pegmatites.

A short section on "*aplites*" (pp. 1102-1103) would later become a key element of the Jahns model for pegmatites. At this stage of his career, he reviewed their characteristics, and the interpretations of others:

"*…the numerous occurrences observed by the present writer seem to have certain common characteristics. The do not represent elements of composite intrusion, they form masses that are asymmetrically disposed within the enclosing pegmatite bodies, they are temporally equivalent to one of more zones in these bodies and in general are older than the innermost zones, many of them show unmistakable evidences of replacement, generally of potash feldspar by albite… and many of them show equally impressive evidences of primary crystallization and local movements of crystal-rich liquid. These features are not readily reconciled in deducing any single theory for the origin of these rocks, which are difficult to explain either as ordinary zones or as ordinary repacement* [sic] *bodies.*" (p, 1103)

Ore-Forming Fluids
Jahns (1955) concluded the manuscript with a review of "*Pegmatite Bodies, Quartz Veins, and Ore-Forming Fluids.*" He carried forth the widely held claim that a great many pegmatite bodies grade gradually and continuously into quartz veins that emanate from pegmatites into surrounding rocks [2]:

"It is scarcely surprising that many investigators have regarded pegmatites as intermediate in origin between normal igneous rocks and the quartz of hydrothermal veins. Most of them either have commented in general terms upon gradations between magmas and the solutions that deposited vein quartz, or have discussed, in contrasting degrees of detail, the derivation of pegmatite- and vein-forming fluids from the same parent magma...(p. 1104)

*"Bowen (67) regarded vein- and ore-forming solutions as distillates derived through resurgent boiling of pegmatitic rest-magmas. This mechanism may well account for many of the features that can be observed **within** pegmatite bodies and some larger masses of igneous rocks...but its application to veins and ore deposits is open to question...*

"Some of the argument in relating quartz vein-forming hydrothermal solutions to pegmatites could be resolved by a satisfactory answer to the following question: are the quartz 'veins' that can be traced into bodies of pegmatites the products of dilute hydrothermal solutions or are they essentially magmatic in origin?" (p. 1104)

Jahns (1955) went on to propose eight distinctions between quartz of hydrothermal veins and quartz masses within pegmatites. None of these would likely be compelling to contemporary geoscientists. One, however, speaks of evidence that is so unusual as to be noteworthy:

"The quartz itself, though similar in many respects to quartz of hydrothermal veins, is more likely to be twinned according to the Dauphiné law..., to contain so-called 'growth zones', and to show well-developed rhombohedral cleavage." (p. 1105) [3].

Jahns (1955) did not answer the question posed, but he did remark on the likelihood that quartz masses in pegmatites, and by extension their apophyses into surrounding rock (e.g., Fig. 21, p. 1093), are igneous in origin:

"Despite firm statements to the contrary..., quartz masses of this type might well represent the last part of the magmatic stage of pegmatite formation. No longer can such an origin be dismissed on physico-chemical grounds, as there is increasing experimental evidence to suggest that rocks composed mainly of quartz could be formed from the residual silicate liquids at temperatures low in the range usually suggested for pegmatites." (p. 1105)

As that experimental evidence, Jahns cited a study by Tuttle and Friedman (1948), who investigated the liquidus relations of the system Na_2O-SiO_2-H_2O at elevated but unspecified (unknown) pressures. They observed a field in which quartz coexisted with aqueous solution and a liquid of the approximate composition of 40 wt% SiO_2, 15 wt% Na_2O, and 45 wt% H_2O at temperatures as low as 250°C. This, presumably, is the liquid to which Jahns (1955) referred as the experimental evidence for igneous rocks formed mainly of quartz at low temperature. Jahns' error, which has been made by many others since him, was in tying a synthetic chemical system to the natural one without regard for how one might evolve to the other. In this case, the problem lies in the evolution of an aluminosilicate melt containing ~ 15 wt% Al_2O_3 to an alumina-free melt composed of alkalis and silica. Jahns had already remarked that the final melt ought to be close to that of the thermal minimum in the hydrous granite system. One of the important contributions of experimental petrology to understanding natural melts came from experiments

to which alumina was added to binary metal oxide-silica systems. With the addition of alumina, the synthetic compositions more closely approached the natural ones in composition, and the low-temperature regions of melting and two-liquid immiscibility in alkali-silica systems such as this one disappeared.

Concluding Remarks

"*The Study of Pegmatites*" (1955) served to establish Jahns' authority on the subject, by which his own opinions gained weight. Jahns used this article to introduce his conviction that "*resurgent boiling of rest-liquid*" accounts for many of the troublesome features of pegmatites. In this manuscript, Jahns largely refuted his conclusions of just a few years prior, including Cameron et al. (1949) and Jahns (1953a,b), that pegmatites can be explained by fractional crystallization of a silicate liquid in an essentially closed system, and in which metasomatic replacement and hydrothermal crystallization play only minor roles. He did so mostly in reference to his own observations as purportedly laid out in manuscripts that were never published. No where in this manuscript is there an explanation of how this process works, starting with a hydrous granitic liquid, and ending with zoned pegmatites with quartz cores. These explanations were cited as belonging to reference 328.

It is evident that Jahns (1955) sought to take the study of pegmatites from a largely intuitive, field-based inquiry to a laboratory setting for validation. He weighed two methods – fluid inclusion analysis and experimental petrology – found the former lacking in confidence, and sided with the experimental approach. By 1955, Jahns had teamed up with O. Frank Tuttle and C. Wayne Burnham at Pennsylvania State University (Wright, 1985), who led the way in the experimental study of granite systems.

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Footnotes

[1] My library search over the first four years of my graduate education, 1976-1980, generated 723 hand-written index cards with the complete citations and pertinent notes on articles I had found and read in the library of Arizona State University.

[2] Of the hundreds of pegmatites I've seen in the field, I have ever seen one in which a quartz core extended out of the pegmatite body as a quartz vein. At Sennen Cove, on the southwest tip of Lands End, Cornwall, UK, I did observe in 1990 a porphyritic granite with a tourmaline-rich margin that graded continuously into a quartz-tourmaline rock without diminution of width from dike to vein nor change in the texture of the rock. In a large gallery exposure at Argemela, Portugal, I observed in 1989 a radial pattern of quartz veins that emanated from the top of a zoned petalite-bearing pegmatite, but the veins did not evidently penetrate into the pegmatite.

[3] Pegmatitic quartz is anhedral and interstitial to other minerals. It possesses no distinct forms by which twinning could be ascertained in any way other than detailed x-ray or microscopic examination of very large areas of massive quartz. Growth zoning is exceptionally rare in pegmatitic quartz. Having smashed countless pieces of pegmatitic quartz, I can say I have run into rhombohedral cleavage only once, and rarely, in massive quartz from the Tanco pegmatite, Manitoba.