

## DIES DIEM DOCET: THE DECIPHERMENT OF UGARITIC

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In the field of Ugaritic studies, three scholars are generally credited with playing major roles in the decipherment of Ugaritic: Hans Bauer, Paul Dhorme and Charles Virolleaud. To date, only one scholar, Alan D. Corré, has produced a detailed analysis of the precise roles that each of these scholars played<sup>1</sup>. While Corré's reconstruction of the decipherment process is, in general, commendably sound, it nevertheless does not take all of the relevant data into account, leaves certain facts unexplained (or underexplained) and is methodologically weak in its assigning dates to two important elements of the primary record that involve instances where lecture presentations preceded the published versions of the papers<sup>2</sup>. Thus there is room for improvement on Corré's work. The present article will proceed by laying out in chronological order and considerable detail the pertinent facts, attending more fully to data such as paper presentation dates, article completion dates, and intellectual context. It will also glean relevant data from articles, records and private communications heretofore overlooked as sources for reconstructing the decipherment process. It will then bring all of this information to bear on assessing the above scholars' respective roles, especially the problematic role of Charles Virolleaud.

In March of 1928 a local farmworker plowing near Minet el-Beida dislodged a stone slab that covered a passageway leading to a vaulted tomb. When the Assyriologist Charles Virolleaud, then Director of the French *Service des Antiquités*<sup>3</sup> in Beirut, was informed

<sup>1</sup> A.D. Corré, "Anatomy of a Decipherment", *Wisconsin Academy of Sciences, Arts and Letters* 55, 1966, 11-20. Accounts that are contemporary or nearly contemporary with the decipherment process itself are cited in the course of this article. Note also R. de Langhe, *Les textes de Ras Shamra-Ugarit et leurs rapports avec le milieu de l'Ancien Testament*, vol. 1, Paris 1945, 221-234 and G. Sauer, *Die Sprüche Agurs*, Stuttgart 1963, 127-128. Popular treatments, such as E. Doblhofer's *Le déchiffrement des écritures*, Paris 1959, 209-227, and M. Pope's *The Story of Decipherment*, London 1999 [1975<sup>1</sup>], 117-122), focus primarily on the methods the respective scholars employed. More recent scholarly treatments such as K. Cathcart's "The Decipherment of Ugaritic", in W.G.E. Watson - N. Wyatt (eds.), *Handbook of Ugaritic Studies*, Leiden 1999, 76-80, and A. Caquot, M. Szyner and A. Herdner's discussion in TOU 1, 35-41, do not reconstruct the decipherment process in sufficient detail.

<sup>2</sup> Specific instances of these shortcomings will be documented throughout this article.

<sup>3</sup> Both Syria and Lebanon at this time were under the French Mandate. The *Service des Antiquités* was a department of the French High Commission and controlled policy regarding antiquities, subject to the guidelines of article 14 of The Mandate for Syria and Lebanon, July 24 1922 (S. Longrigg, *Syria and Lebanon under French Mandate*, London 1958, 290, 379-380). Virolleaud had been the director since Oct. 1, 1920 (R. Dussaud, "L'Œuvre scientifique syrienne de M. Charles Virolleaud", *Syria* 33, 1956, 9). A detailed study by M. Gelin (*Le Service des Antiquités en Syrie et au Liban pendant le mandat français, 1919-1946*) is in press. Corré ("Anatomy", 12) noted the involvement at this point of the *Service des Antiquités* but evidently did not think it pertinent to mention that Virolleaud was the director. In my view, Virolleaud's direct involvement in the site at this early juncture is extremely

of the find, he dispatched a colleague, Léon Albanèse, to visit the site. Albanèse reported in the journal *Syria*<sup>4</sup> finding Cypriot pottery in the tomb and, in an additional note to Albanèse's report, René Dussaud, *Syria*'s editor, reported that Virolleaud himself on two subsequent visits to the site had found more Cypriot pottery, some fragments of which exhibited Mycenaean influence<sup>5</sup>. These initial finds prompted Dussaud to speculate that Ras Shamra was a Cypriot outpost<sup>6</sup>. As we shall see, both these initial finds themselves and Dussaud's interpretation of them would have serious implications for Virolleaud's earliest attempts to decipher Ugaritic.

In addition to being the editor of the journal *Syria*, René Dussaud was also a Near Eastern antiquities conservator at the Louvre Museum and a member (as well as acting president in the latter half of 1929 and president in 1930) of the *Académie des Inscriptions et Belles-Lettres* (hereafter the *Académie*). He proposed that Claude Schaeffer lead an archaeological mission to be funded by the *Académie* and the Louvre<sup>7</sup> to excavate Ras Shamra and the Minet el-Beida area<sup>8</sup>. In his summary report of the first campaign, Schaeffer described arriving at Minet el-Beida by camel caravan, guarded by 20 soldiers<sup>9</sup>. Pickaxing in the necropolis area where the tomb had been discovered commenced on

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pertinent to understanding his initial position regarding the language of the alphabetic cuneiform texts, and the fact that he was the director of the *Service* explains why he got first crack at deciphering the texts (see below).

<sup>4</sup> L. Albanèse, "Note sur Ras Shamra", *Syria* 10, 1929, 19.

<sup>5</sup> R. Dussaud, "Note additionnelle," *Syria* 10, 1929, 20-21.

<sup>6</sup> Dussaud, "Note additionnelle", 21. Dussaud repeated this characterization in his "La Lydie et ses voisins" (*Babyloniaca* 11, 1930, 164), in which he also mentioned that a cylinder seal Virolleaud found at the site before formal excavations began had non-Semitic names inscribed on it (166). As Virolleaud at that time was the editor of *Babyloniaca* ("Publications de M. Charles Virolleaud", *Syria* 33, 1956, 1), it is reasonable to assume that he was familiar with this article.

<sup>7</sup> Dussaud, "L'Œuvre", 10. The Oct. 12, 1929 issue of *L'Illustration* (4519, p. 402) notes that the governor of the Alaouite State, in which Ras Shamra was located, also contributed financial support.

<sup>8</sup> On the basis of a letter Dussaud wrote to Franz Cumont on Dec. 7, 1930, C. Bonnet ("Les lettres de René Dussaud à Franz Cumont conservées à l'Academia Belgica de Rome", in M. Dietrich - I. Kottsieper [eds.], «*Und Mose schrieb dieses Leid auf*»: Studien zum Alten Testament und zum Alten Orient, Münster 1998, 116-118) points to an interest on the part of Dussaud in the question of contacts between classical and oriental cultures, which was beginning to be framed at that time by the notion of syncretism. According to J. Friedrich (*Ras Schamra: Ein Überblick über Funde und Forschungen*, Leipzig 1933, 5), the initial Cypriot and Mycenaean pottery finds at Minet el-Beida and Ras Shamra were what prompted Dussaud to secure financial support for excavating there. Schaeffer ("Séance du 8 Août", CRAIBL, 1929, 234) simply notes that the excavation was undertaken on Dussaud's initiative.

<sup>9</sup> C. Schaeffer, "Les fouilles de Minet-el-Beida et de Ras Shamra (campagne du printemps 1929)", *Syria* 10, 1929, 286. The French presence in Syria under the French Mandate frequently provoked political protest and social unrest, and so this general state of affairs is reason enough to explain the presence of an armed guard. More specifically, on February 5, 1929, shortly before the excavation began, Henri Poinso, France's High Commissioner for Syria and Lebanon, had suspended indefinitely the Syrian Constituent Assembly, over certain articles in a constitution the Assembly had drafted the previous summer (P.S. Khoury, *Syria and the French Mandate*, Princeton 1987, 335-345). This proroguing of the Assembly provoked angry speeches, press articles and street demonstrations (Longrigg, *Syria and Lebanon*, 185), thus creating a potentially dangerous work environment for the French excavators.

April 2, 1929, and on May 9, on the earlier instructions of Dussaud<sup>10</sup>, the excavators redirected their attention to the tell. The first cuneiform tablets were discovered on May 14, at sunset, in an area that Schaeffer at that time thought was a palace, and Schaeffer wrote to inform Dussaud of the find that very evening<sup>11</sup>. Two days later the excavators unearthed a hoard of mint condition tools and weapons among which were five axeheads inscribed with cuneiform signs. On May 17 Schaeffer went to Latakia to phone Virolleaud and inform him of the finds. Virolleaud came to the camp at Minet el-Beida the very next day, spent that night in Latakia but then returned unexpectedly to the site the following day and instructed Schaeffer to give him all the inscribed texts<sup>12</sup>. Schaeffer gave Virolleaud those that were in the best condition, and Virolleaud took them back to Beirut.

<sup>10</sup> Schaeffer, "Les fouilles", 293-294. Dussaud had visited the site on April 9-10.

<sup>11</sup> Schaeffer, "La première tablette", *Syria* 33, 1956, 165. Though the article was written in 1956, Schaeffer's recollections here are augmented by references to and quotations from his personal diary of the first campaign. Dussaud relayed the news of the find to the *Académie* on May 24 ("Séance du 24 Mai", CRAIBL, 1929, 153).

<sup>12</sup> Schaeffer, "La première tablette", 167. Thus Corrè ("Anatomy", 13) was in error in portraying Schaeffer as voluntarily entrusting the tablets to Virolleaud. Rather, as Virolleaud was the director of the *Service* and therefore in control of the disposition of archaeological finds, Schaeffer had no choice in the matter. Indeed, Virolleaud's position at the time of these initial finds explains his very involvement in the decipherment process. It is difficult to determine precisely why Virolleaud insisted that Schaeffer surrender the tablets to him on the spot. One could speculate that the prospect of exclusive initial access to so important an inscriptional find would provide more than sufficient motivation, but the situation was doubtlessly more complex. Dussaud, in his role of conservator at the Louvre, had a vested interest in obtaining as many artifacts as possible for the museum's collection, an interest that Virolleaud evidently opposed. In a series of letters Dussaud wrote to Cumont in April of 1924, for example, Dussaud complained that Virolleaud had accused him of exporting artifacts to the Louvre without proper authorization and hence Virolleaud had demanded that Dussaud send the artifacts back. Dussaud viewed Virolleaud as a pencil-pusher (*rond-de-cuir*) who allocated artifacts as he pleased, without concern for the scientific value of keeping groups of related artifacts together (letters of April 16, 24 and 30, conserved at the Belgian Academy in Rome). In a February 20, 1928 letter to Cumont (conserved at the Belgian Academy in Rome) regarding artifacts allocated to the British Museum, Dussaud opined that it seemed that Virolleaud had lost his mind (*«Virolleaud me semble avoir perdu la tête»*). Thus Dussaud and Virolleaud clearly had a history of disagreeing about the allocation of artifacts (cf. C. Bonnet, "La découverte archéologique de la Syro-Phénicie dans les années '20 et '30 d'après quelques témoignages épistolaires", in the forthcoming *Festschrift* for Manfred Dietrich, and Dussaud's letter of November 28, 1929 to Cumont [Bonnet, "Les lettres", 114-116]). Virolleaud's demand of Schaeffer can be viewed, then, as something of a preemptive strike, a move on Virolleaud's part intended to ensure that the inscribed texts would not somehow find their way to the Louvre without his authorization. (I would like to thank Prof. J. Hamesse, Director of the Belgian Academy, and the heirs of F. Cumont, for making Dussaud's letters available to me, and Corinne Bonnet for providing me with copies of the letters.) As is evidenced by P. Bordreuil and D. Pardee's list of museum holdings (*La trouvaille épigraphique de l'Ougarit 1: Concordance*, Paris 1989, 383-392), Virolleaud was ultimately unable to prevent Dussaud from acquiring first campaign epigraphic materials for the Louvre. By October 1, 1929, Virolleaud was no longer director of the *Service des Antiquités*: Dussaud had replaced him with Henri Seyrig (E. Will, "Henri Seyrig", *Syria* 50, 1973, 260). Virolleaud relocated to Paris. Virolleaud's relinquishment of his directorship was announced to the *Académie* on July 10, 1929: the record simply states that he would be returning to France ("Séance du 10 Juillet", CRAIBL, 1929, 204). Virolleaud's move was later presented as resulting from his resignation ("Séance du 30 Août", CRAIBL, 1929, 250), but the overall dynamics of the situation raise the suspicion that his resignation may have been forced.

The following day (May 20), according to a note in Schaeffer's personal diary, Virolleaud wrote him to say that the inscriptions were extremely important because the writing was an alphabetic form of cuneiform theretofore unknown and for the moment indecipherable<sup>13</sup>. Some days later, in Beirut, Schaeffer gave Virolleaud the remainder of the texts and the two agreed that Virolleaud would publish them by year's end<sup>14</sup>.

On September 20, 1929, Virolleaud made a presentation to the *Académie* about the cuneiform inscriptions<sup>15</sup>. He reported that some were written in Babylonian and others, including those on the axeheads, in an unknown language. On the basis of the repertoire of signs (26 by his count), Virolleaud expressed certainty that the writing was alphabetic, not syllabic. He noted the use of word dividers in the texts, but stated that, nevertheless, he could only conjecture about the language and nature of the documents. In the same session, Dussaud commented that Ras Shamra in the second half of the second millennium B.C.E. was inhabited principally by Cypriots and Aegeans<sup>16</sup>.

During the first four months of 1930, the published record indicates that Virolleaud made three presentations that included comments on the Ras Shamra tablets. Reporting to the *Société Asiatique* on February 14, 1930<sup>17</sup>, Virolleaud is once again recorded as saying that this new, alphabetic cuneiform was as yet undeciphered. On the basis of the brevity of the words, Virolleaud conjectured that the vowels were not represented. He suggested that further research on the language of the tablets should probably be oriented towards Cyprus and the Aegean. At a conference held at the Sorbonne on March 22, 1930, Virolleaud gave a paper<sup>18</sup> in which he again referred to the alphabetic cuneiform as unintelligible<sup>19</sup>. He noted with certainty that Cyprus was the origin of the people who inhabited Ras Shamra and buried their dead in the necropolis of Minet el-Beida, though he also pointed out the wide variety of cultural influences in evidence<sup>20</sup>. Regarding the alphabetic writing, he commented that it was of a kind completely different than that of the Phoenicians but founded on the same [i.e. alphabetic and consonantal] principle, and mused about whether there had been numerous, independent attempts to simplify writing on the coast of Syria in the second half of the second millennium or whether there initially had been only one<sup>21</sup>. In an April 9, 1930, lecture that he gave in Berlin<sup>22</sup>, Virolleaud

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<sup>13</sup> Schaeffer, "La première", 167.

<sup>14</sup> Schaeffer, "La première", 168. We know that Dussaud viewed Virolleaud as something of a dawdler (Dec. 27, 1928 letter to Cumont; cf. his Oct. 8, 1932 letter to Cumont, both conserved at the Belgian Academy in Rome) and this may be the reason why Schaeffer got Virolleaud to agree to a publication date at this juncture.

<sup>15</sup> "Séance du 20 Septembre", CRAIBL, 1929, 265-266.

<sup>16</sup> "Séance du 20 Septembre", 266. Dussaud expanded upon his remarks in his "Note additionnelle" (*Syria* 10, 1929, 297-303) to Schaeffer's first campaign report.

<sup>17</sup> JA 216, 1930, 353.

<sup>18</sup> "La Syrie et la Phénicie dans la haute antiquité d'après les fouilles récentes", subsequently published in *Revue générale des Sciences pures et appliquées*, 1930, 495-509.

<sup>19</sup> Virolleaud, "La Syrie", 508.

<sup>20</sup> Virolleaud, "La Syrie", 507-508.

<sup>21</sup> Virolleaud, "La Syrie", 508-509.

began his brief discussion of the Ras Shamra alphabetic texts by saying that the Phoenician alphabet was not the only one in use on the Syrian coast in the latter part of the second millennium. Once again, he referred to the language of the Ras Shamra tablets as undeciphered. Thus it is clear that at least as late as April 9, 1930, Virolleaud did not realize that the alphabetic cuneiform of Ras Shamra expressed a Semitic language<sup>23</sup>.

Though Virolleaud may have honored his agreement with Schaeffer to produce an *editio princeps* of the inscriptions found in 1929 before the end of that year<sup>24</sup>, the 1929 fascicle of *Syria* that contained this article<sup>25</sup> did not appear until April of 1930<sup>26</sup> and was

<sup>22</sup> Virolleaud's abstract of this lecture, "Syrien und Phönizien nach neuesten Ausgrabungen", was published in FF 6 (July 10, 1930), 261-262.

<sup>23</sup> In his 1936 publication *La légende phénicienne de Danel*, Paris 1936, Virolleaud claimed to have deduced that the language of the Ras Shamra alphabetic texts was West Semitic and therefore to have assigned a value equivalent to Hebrew *lamed* to the first sign of tablet 18, to have isolated the word «Baal» and to have guessed that *l* and *b* were being used as prepositions by November of 1929 (69-70). If this is true, then his three presentations in early 1930 must be adjudged to be intentionally misleading. However, I consider it far more likely that Virolleaud's recollections, in 1936, of his role in the decipherment process are inaccurate. For a crystal-clear example, see (below) his description of his October 2, 1930 letter to Bauer. Note also that Dhorme disputed the accuracy of Virolleaud's presentation in *Danel* of the decipherment process (E. Dhorme, review of Virolleaud's *Danel*, *Syria* 18, 1937, 112-113; cf. W. Baumgartner, "Ras Schamra und das Alte Testament", *Theologische Rundschau* 12, 1940, 171 and de Langhe, *Les textes*, 223 n.1). Corré ("Anatomy", 19 n. 35) evidently was unaware of Virolleaud's March 22 and April 9 presentations, as he specified Feb. 14 as the latest recorded date at which Virolleaud was still orienting his decipherment efforts towards Cyprus. Believing Feb. 14 to be the crucial date, Corré speculated ("Anatomy", 19) that Virolleaud may well have achieved partial or almost complete decipherment before anyone else even began work on the texts. Given that Bauer began working on the texts on April 22 (see note 36), Virolleaud's comments on the language on April 9 make Corré's speculation highly unlikely. That Virolleaud was working on projects other than the tablets by early April of 1930 can be gleaned from a presentation he made to the *Académie* on April 5. At that meeting Virolleaud gave a paper concerning what he called the legend of the cedar mountain, a topic which he clearly treated in considerable scholarly detail ("Séance du 5 Avril", CRAIBL, 1930, 63-64).

<sup>24</sup> Schaeffer ("La première", 168) in 1956 recalled that he did, but this article appeared in a volume honouring Virolleaud, and the evidence Schaeffer cites is that Virolleaud's *editio princeps* was published in the last fascicle of *Syria* for 1929, but that fascicle did not appear until April of 1930 (see note 26).

<sup>25</sup> Virolleaud, "Les inscriptions cunéiformes de Ras Shamra", *Syria* 10, 1929, 304-331. While Virolleaud had no doubt communicated much of the substance of this article at the Sept. 20, 1929 meeting of the *Académie* (cf. 304, n. 1), the article evidences progress that Virolleaud had made subsequent to his presentation to the *Académie*. In the *Académie* meeting, Virolleaud had stated that the alphabet had 26 letters (see above) whereas in the article he says 26 or 27 (305). At the *Académie* meeting ("Séance du 20 Septembre", 266) he stated that one could only guess as to the nature of the documents, but in the article (306) he makes a few (admittedly tentative) suggestions on the basis of purely formal observations. Corré ("Anatomy", 13-14) does not distinguish between Virolleaud's Sept. 20 presentation and the published article, and so he incorrectly credits Virolleaud with having reached by Sept. 20 conclusions that he demonstrably had not reached by that date. This methodological flaw in Corré's work is a contributing factor to his positive evaluation of Virolleaud, an evaluation with which I take issue (see below). As Virolleaud gives no information in the article under discussion as to when he finished writing it, I cannot place it precisely. However, given that, at least as late as April 9, 1930, Virolleaud had not discerned that the language of the inscriptions was

therefore, theoretically at least, available for emendation or supplementation into early 1930. In this article Virolleaud once again stated that the undeciphered writing must be alphabetic. The presence of word dividers allowed him to note the brevity of the words, and on this basis he proposed that perhaps the vowels, or at least the short vowels, were not represented<sup>27</sup>. Turning his attention to the axeheads<sup>28</sup>, he pointed out that four of them had the same six-sign inscription, and that this six-sign combination also appeared at the beginning of one of the tablets (number 18) but preceded by a single sign not present on the axeheads. On the basis of this observation, Virolleaud proposed that the six-sign combination was a proper name<sup>29</sup> and that the single sign preceding it on tablet 18 was the equivalent of Akkadian *ana*, «to», marking the addressee of a letter. On the fifth inscribed axehead, Virolleaud noted that the same six-sign combination was preceded by a four-sign word, and proposed that this latter word designated the object upon which it was inscribed. He suggested that searching through all Eastern languages for a four-letter word for «axe» might well provide a sufficient basis for deciphering the language. Finally, Virolleaud discussed various languages and writing systems indigenous to Asia Minor, Cyprus, Crete and the Aegean and their relative likelihood for serving as potential clues for determining the language of the inscriptions<sup>30</sup>. He did not discuss any Semitic language in this context.

With Virolleaud's drawings of the inscriptions and accompanying observations now in the public domain<sup>31</sup>, other interested scholars could begin to try to decipher the inscriptions. Two such persons were Hans Bauer, a professor in Halle, Germany, and Édouard (Paul)<sup>32</sup> Dhorme, Director of the *École Biblique* in Jerusalem. Dhorme, who had been decorated for his cryptological work in World War I<sup>33</sup>, was, in late 1929,

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Semitic, and given that there is no indication to the contrary in the article, it does not prejudice Virolleaud's case to discuss the article at this juncture in my presentation.

<sup>26</sup> Dussaud, *Les découvertes de Ras Shamra (Ugarit) et l'Ancien Testament*, Paris 1941, 64 n. 1. Recall that Dussaud was the editor of *Syria*.

<sup>27</sup> Virolleaud, "Les inscriptions", 305-306.

<sup>28</sup> Virolleaud, "Les inscriptions", 306-307.

<sup>29</sup> Noting that the six-sign combination was divided in two in tablet 18, Virolleaud proposed that it was a compound name, perhaps theophoric ("Les inscriptions", 307). Though the suggestion proved wrong, drawing attention to the division was a helpful observation as there is no word divider in this six-sign combination on the axeheads.

<sup>30</sup> Virolleaud, "Les inscriptions", 307-309.

<sup>31</sup> News of the finds, including photographs of selected inscribed pieces, had been published prior to Virolleaud's *editio princeps* in *l'Illustration* (Number 4519, Oct. 12, 1929), the *Illustrated London News* (Number 4724, Nov. 2, 1929) and the *Leipziger Illustrierte Zeitung* (Number 4427, Jan. 16, 1930), but these publications did not furnish a sufficient basis for systematic work on the inscriptions. For further citations of early publications in the popular press, see Friedrich, *Ras Shamra*, 37.

<sup>32</sup> Paul was his religious name, which he used in publications until 1931. Thereafter he used Édouard.

<sup>33</sup> W.F. Albright, "New Light on Early Canaanite Language and Literature", *BASOR* 46, 1932, 16; Doblhofer, *Le déchiffrement*, 220.

finishing up a manuscript on Semitic languages and writing systems<sup>34</sup>. He had visited the Louvre in late October of that year and seen there an exposition of the finds from Minet el-Beida<sup>35</sup>. Bauer had written a monograph on deciphering the Sinai inscriptions and, with his colleague Pontus Leander, had produced historical grammars of both Hebrew and Aramaic. In short, both of these men were accomplished Semitic linguists and epigraphers.

Bauer received Virolleaud's *editio princeps* on April 22, 1930<sup>36</sup>. As both he<sup>37</sup> and others<sup>38</sup> have described in detail his approach to deciphering the texts, suffice it to say that by April 27 he believed he had essentially accomplished decipherment of the language, and the following day informed Dussaud of his success<sup>39</sup>. While Bauer does not specify exactly what he communicated to Dussaud that day and on at least one subsequent occasion<sup>40</sup>, Dussaud confirms that, by May 18, he was convinced that Bauer had been able to identify some 20 letters<sup>41</sup>, and at the May 23, 1930, meeting of the

<sup>34</sup> The *Avant-propos* to Dhorme's *Langues et écritures sémitiques*, Paris 1930, is dated Dec. 1, 1929, in Jerusalem.

<sup>35</sup> P. Dhorme, "Trouvailles sensationnelles en Syrie", RB 39, 1930, 152-153. Dhorme reserved comment on the writing system until Virolleaud had published the texts.

<sup>36</sup> H. Bauer, *Entzifferung der Keilschrifttafeln von Ras Shamra*, Halle 1930, 3 n. 1.

<sup>37</sup> Bauer, "Die Entzifferung des Keilschriftalphabets von Ras Shamra", FF 24, Aug. 20, 1930, 306-307; *Entzifferung*, 3-10. Proceeding from the hypothesis that the language was West Semitic, Bauer's method was essentially to identify the signs that he thought were prefixes, suffixes and single-sign words and then compare these results to an inventory of common west Semitic prefixes, suffixes and single (consonantal) grapheme words. Then, taking into account how frequently the respective signs appeared in the texts as well as speculating as to the relative frequency of consonants functioning as prefixes, suffixes and single-sign words in West Semitic, and noting both the inclusion and exclusion of the signs and of the West Semitic prefix, suffix, and single-grapheme words in relation to his three categories, Bauer arrived at two possible values for each of four signs. Accepting Virolleaud's proposal that the first sign of tablet 18 could be the equivalent of Akkadian *ana*, «to», Bauer identified it as *l*. Then, combining his results thus far with a search for common West Semitic words and divine names, and following up on Virolleaud's suggestion to look for a word meaning «axe», Bauer proceeded to assign values to signs.

<sup>38</sup> Egs. Doblhofer, *Le déchiffrement*, 214-217; Pope, *The Story of Decipherment*, 119-120.

<sup>39</sup> Bauer, *Entzifferung*, 3 n.1. The apparent speed with which Bauer accomplished an initial decipherment is nothing short of stunning. Bauer acknowledged that the method he developed in his earlier work on the proto-Sinaitic inscriptions (*Zur Entzifferung der neuentdeckten Sinaischrift und zur Entstehung des semitischen Alphabets*, Halle 1918, 13-14) provided him with the means to so quickly decipher the Ras Shamra alphabet (*Entzifferung*, 3). Bauer was thus able to prepare his strategy in advance of the publication of Virolleaud's *editio princeps* as the inscriptions' employment of word dividers was part of the published record ("Séance du 20 Septembre", CRAIBL, 1929, 266; *Leipziger Illustrierte Zeitung*, Jan. 16, 1930, 73).

<sup>40</sup> «Am 28. [April] machte ich [Bauer] Herrn Dussaud die erste Mitteilung, der seinerseits am 23. Mai der Académie des Inscriptions darüber berichtete» (Bauer, *Entzifferung*, 3 n. 1).

<sup>41</sup> Dussaud, "Déchiffrement par M. Hans Bauer des textes de Ras Shamra", *Syria* 11, 1930, 201. Dussaud's announcement includes specific words that he believed Bauer had identified.

*Académie* Dussaud announced that Bauer had succeeded in deciphering the language<sup>42</sup>. Dussaud also reported that Bauer's results were verified by the fact that, following Virolleaud's suggestion to look for a four-consonant word for «axe», Bauer's system yielded the term *grzn*<sup>43</sup>, which has a Hebrew cognate meaning «axe». Dussaud's reportage is consistent with Bauer's statement that on May 15 he sent off a short preliminary report to *Vossische Zeitung*, a Berlin newspaper<sup>44</sup>. Bauer's article appeared in the June 4, 1930, issue (128), and included examples of words that he believed he had correctly deciphered<sup>45</sup>, one of which was *grzn*. As is understandable in the context of a popular publication, Bauer did not cite tablet and line numbers for his readings. He also reported and illustrated by example his finding that, inexplicably but doubtlessly, the Ras Shamra cuneiform alphabet had two signs for (the equivalent of Hebrew) *aleph*. He claimed to have successfully deciphered 20 signs, but the article includes neither a sign list with proposed consonantal values nor a list of the 20 values that he believed he had discerned. The specific words he cites employ 14 consonants.

<sup>42</sup> "Séance du 23 Mai", CRAIBL, 1930, 130-131. Dussaud clarified his announcement by saying that, while difficulties remained, Bauer's work was decisive for identifying the language as a dialect of Phoenician.

<sup>43</sup> Dussaud, "Déchiffrement", 201.

<sup>44</sup> "Das Alphabet von Ras Schamra: Die Entzifferung einer neuen Keilschrift" (Bauer, *Entzifferung*, 3 n. 1). Bauer excerpts relevant portions of the article in his *Das Alphabet von Ras Schamra: seine Entzifferung und seine Gestalt*, Halle 1932, 43-45.

<sup>45</sup> Bauer's *Vossische Zeitung* article includes words not cited by Dussaud in his "Déchiffrement" (above). In a later work (*Les découvertes*, 64 and n. 2), Dussaud credited Bauer with having identified a list of words substantially different from those he cited in "Déchiffrement" and added that he (Dussaud) communicated the *Les découvertes* list to the May 23, 1930 meeting of the *Académie*. The *Les découvertes* list includes four words not present in "Déchiffrement" that did appear in *Vossische Zeitung*. The CRAIBL report of the May 23 meeting of the *Académie* (1930, 130-131) contains no specific words, but this is not surprising as CRAIBL reports are essentially minutes of the meetings. The report does, however, record Dussaud saying that Bauer had correctly identified 20 signs. It seems clear, then, that in both "Déchiffrement" and *Les découvertes* Dussaud was selecting examples from the results that Bauer had sent him. The disparity between the examples listed in the two works can be accounted for by what Bauer's mistakes would eventually be determined to be. Dussaud's sample in "Déchiffrement", written before Bauer's mistakes had been identified, includes words that Bauer clearly got wrong, while his list of examples in *Les découvertes*, written well after Bauer's mistakes had been recognized, are all words that Bauer essentially (though not all precisely) got right. That said, it appears that both Dussaud and Bauer, in retrospect, were, by what they selected to report about Bauer's initial findings, minimizing the extent to which Bauer had made mistakes. Dussaud's *Les découvertes* list is exactly the same, and in exactly the same order, as the list Bauer gives in his 1932 publication *Das Alphabet von Ras Schamra* (42e). The lists also share the additional feature of correcting the transliteration of the one sign in this sample of words that Bauer didn't initially get exactly right (š replaced by ṣ̌ [=l], Bauer's sign 16 in the 1930 publication *Entzifferung*, 12). On the same page (64) that his list appears, Dussaud cites (n. 1) the relevant page (42) of Bauer's *Das Alphabet*, so it is clear that Dussaud was aware of Bauer's list and was reproducing it. Bauer's selection of words in *Das Alphabet* was obviously motivated by wanting to show himself in the best possible light, and Dussaud, by reproducing Bauer's *Das Alphabet* list, was tacitly justifying his May 23 report to the *Académie* regarding Bauer's success. For an earlier tack taken by Dussaud to bring his May 23 remarks into line with Bauer's limited success, see his review of Friedrich's *Ras Schamra*, *Syria* 15, 1934, 205).

Meanwhile, in Jerusalem, Paul Dhorme was also working on deciphering the Ras Shamra cuneiform alphabet. Following up on Virolleaud's suggestion that the first sign of tablet 18 could be the equivalent of Akkadian *ana*, «to», Dhorme began by speculating that this sign was the «Phoenician *lamed*»<sup>46</sup>. This led him to identify the word *b<sup>l</sup>* at the beginning of each line of tablet 14 and to identify the word *mlk*, but he had erred in his assignation of *n* and *t* and thus had been thrown off course until, in mid-June, W.F. Albright showed him Bauer's *Vossische Zeitung* article<sup>47</sup>. As we have seen, Bauer's *Vossische Zeitung* article did not cite tablet and line numbers for his proposed readings but it *did* specify inferentially the four signs that Bauer was reading *grzn* on one of the inscribed axeheads, and *n* was one of the two letters that had led Dhorme astray. Accepting Bauer's (fortunately correct) identification of the sign for *n*, Dhorme was able to correctly read *l rb khnm*, «to the chief of priests», at the beginning of tablet 18<sup>48</sup>. Given the combination of 1) Bauer's general non-specificity about the signs lying behind his readings, 2) the fact that Bauer had led Dhorme to a correct reading, 3) the fact that, from Dhorme's point of view, he and Bauer had both found words such as *b<sup>l</sup>* and *khnm* and 4) Dhorme's knowledge of Dussaud's announcement to the *Académie* that Bauer had deciphered 20 signs<sup>49</sup>, Dhorme assumed, prior to having seen Bauer's proposed alphabet, that he and Bauer were in substantial agreement<sup>50</sup>. (In fact, as Dhorme would soon discover, his and Bauer's alphabets differed considerably.). By August 15, 1930,<sup>51</sup> Dhorme had produced a list of 29 signs with corresponding consonantal values to all but four, of which 18 values would prove to be correct.<sup>52</sup>

<sup>46</sup> P. Dhorme, "Un nouvel alphabet sémitique", RB 39, 1930, 572-573.

<sup>47</sup> Dhorme, "Un nouvel", 573. See also Albright, "New Light", 16. Corré ("Anatomy", 15) notes that Bauer's article was published in the supplement (*das Unterhaltungsblatt*) to the June 4 issue.

<sup>48</sup> Dhorme, "Un nouvel", 573. Thus Virolleaud had not been precisely correct when he had speculated that the beginning of tablet 18 contained a compound proper name (see note 29).

<sup>49</sup> Dhorme, "Un nouvel", 573.

<sup>50</sup> Dhorme ("Un nouvel", 573) simply says that he based his conviction on «*les éléments contenus dans l'article [i.e. *Vossische Zeitung*] cité ci-dessus*». I have specified what seem to me to be the more obvious elements and conjectured that Dhorme was also influenced by Dussaud's report to the *Académie*.

<sup>51</sup> Dhorme, "Un nouvel", 577.

<sup>52</sup> Dhorme, "Un nouvel", 574. Both here and in subsequent tallies I count «*alephs*» as correct even when they are not accompanied by the correct vowel value. Two values that I have counted as incorrect require further comment. The sign to which Dhorme assigned the value *h* would prove to be *h*. Dhorme did not ascribe the value *h* to any sign, and the Ugaritic words he cited (576) to support his value *h* have Arabic cognates with *h*. Dhorme was certainly a good enough Semiticist to know the import of the Arabic cognate evidence, so perhaps he understood the value correctly even though he transcribed the sign as *h* (and see note 55). The value *ʔ* is present in the section of Dhorme's list where he provided corresponding signs, but there is no corresponding sign, just a large and a small dot. Corré ("Anatomy", 17 and n. 25) speculated that the lack of a discernible sign for *ʔ* was probably a transcription error and therefore credited Dhorme with being the first to correctly identify *ʔ*. In subsequent publications, however, Dhorme does not take credit for having determined the sign for *ʔ*. Rather, he credits Bauer (e.g. Dhorme, "Le déchiffrement des tablettes de Ras Shamra", JPOS 11, 1931, 4).

On August 20, 1930, a short article by Bauer appeared in *Forschungen und Fortschritte*<sup>53</sup>. The main focus of the article is Bauer's description of his process of decipherment, but the article also contains references to specific tablets and lines as well as drawings of selected sign groups and Bauer's transliteration and translation of them. Thanks to Dussaud, Dhorme was alerted to this article shortly after he had finished correcting the proofs to the *RB* article he had completed on August 15 and, as a result of reading Bauer's article, added a postscript, dated September 14, 1930, to his own article<sup>54</sup>. Now knowing more about Bauer's alphabet, but still not having seen it in its entirety, Dhorme stated in this postscript that he and Bauer differed on the signs representing the (frequent and important) letters *k*, *m* and *ʒ*<sup>55</sup>. Dhorme then sent Bauer a proof copy of his article, which Bauer received at the end of September<sup>56</sup>. At that time, Bauer had finished writing his *Entzifferung der Keilschriftafeln von Ras Schamra*<sup>57</sup>, had submitted it to the publisher, and was awaiting its imminent appearance. This monograph contained Bauer's first publication of his entire alphabet and, as he received Dhorme's alphabet too late to revise his own<sup>58</sup>, we can for the first time see exactly how far Bauer had progressed. Of the 25 signs to which Bauer assigned values, 14 are correct<sup>59</sup>. Though Bauer could not incorporate Dhorme's results into his monograph he did append a *wichtiger Nachtrag* to the publication, in which he acknowledged receiving Dhorme's article and in essence expressed his realization that he had made some mistakes. He did

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<sup>53</sup> "Die Entzifferung", FF 24, 1930, 306-307.

<sup>54</sup> Dhorme, "Un nouvel", 577.

<sup>55</sup> Dhorme, "Un nouvel", 577. Dhorme read Bauer's *k* as *m* and Bauer's *m* as *ʒ*. Dhorme was correct about both. Bauer's FF article included a sign that he (correctly) transliterated as *h*, and this is the same sign (see note 52) that Dhorme transliterated as *h*. The fact that Dhorme did *not* state that he and Bauer differed on the value of this sign is additional evidence that Dhorme understood the sign correctly even though he transliterated it as *h*.

<sup>56</sup> Bauer, *Das Alphabet*, 45i.

<sup>57</sup> The monograph's dedication is dated August 4, 1930.

<sup>58</sup> See the *wichtiger Nachtrag* appended to *Entzifferung*.

<sup>59</sup> Bauer, *Entzifferung*, 13-14. The signs he transliterated as  $\bar{g}$  and  $\bar{w}$  respectively are correct, as the lines over the letters simply indicate that Bauer thought there was more than one sign for *g* and *w* respectively. In his 1932 *Das Alphabet* (6-9), Bauer discussed how he accounted for the mistakes he had made. It is evident from his discussion that Bauer was defending the integrity of his method in spite of his mistakes. Bauer attributed his errors primarily to «missing» word dividers that led him to misidentify the sign for *ʒ* as a suffix, which misidentification eventuated in further errors. Though the «missing» word dividers certainly contributed significantly to his errors, they were not the only cause. For example, Bauer also erred in considering one sign as a variant of another (*Entzifferung*, 3-4), and the relative probability component of his method led him, for example, at an early stage of his reasoning process, to discard *k* as a potential value for the sign that turned out in fact to be *k* (*Entzifferung*, 4). It is also inaccurate to claim, as some have done (e.g. O. Eissfeldt, "Hans Bauer", AFO 11, 1936/7, 405; H. Wehr, "Hans Bauer", ZDMG 91, 1937, 177), that Bauer's method was purely mathematical. Bauer did not arrive at the value *l* for the first sign of tablet 18 by mathematically based deduction, nor did he arrive at *grzn* for the four signs on one of the axeheads in that manner.

not have time to examine their respective differences systematically, as *Entzifferung* appeared either at the end of September<sup>60</sup> or in early October<sup>61</sup>.

It is on October 1, 1930, that Virolleaud once again enters the written record. The tablets unearthed in the 1930 campaign had made their way to Paris<sup>62</sup> in August of 1930<sup>63</sup>. Unlike the tablets from the 1929 campaign, these new tablets included long, narrative texts. On August 20 texts of a fragmentary nature from the 1930 campaign had been put at Virolleaud's disposal, and on September 20, after a careful cleaning, the larger, narrative texts had come into his hands<sup>64</sup>. On October 1 Virolleaud wrote a letter to Dussaud in his capacity as President of the *Académie*, and the text of the letter was entered into the record of the October 3 *Académie* meeting<sup>65</sup>. In this letter, Virolleaud announced that he had succeeded in deciphering the Ras Shamra alphabet. He stated that, on the basis of the 1929 texts, he had been able to identify some number of words that appeared to be Semitic such as «*melek, ba'l* [sic], *ben, bet* et *shelosh.*»<sup>66</sup>. He noted that Bauer had obtained similar results, then advanced the opinion that the 1929 texts had remained on the whole unintelligible because they were few in number and very fragmentary. It was thanks to the 1930 texts that serious difficulties had been resolved such that he (Virolleaud) could now confirm that he, and not Bauer, had correctly deciphered the important signs *k*, *m* and *s*<sup>67</sup>. He then states that he had also corrected *p* to *š* and *q* to *p* and had identified the sign for *ṭ*. With these remarks about specific letters, Virolleaud informs us that he was familiar with Bauer's work prior to October 1. That he was aware of Bauer's entire alphabet, and not just those values Bauer had published in *FF*, can be inferred from his comments about *p*, *q*, and *ṭ*. Bauer had not identified *p* and *q*

<sup>60</sup> Dhorme, "Le déchiffrement", 3-4; "Première traduction des textes phéniciens de Ras Shamra", RB 40, 1931, 32; Dussaud, "La mythologie phénicienne d'après les tablettes de Ras Shamra", RHR 104, 1931, 355 n.1.

<sup>61</sup> Bauer, *Das Alphabet*, 47.

<sup>62</sup> Recall that Virolleaud had been living and working in Paris since relinquishing his directorship of the *Service des Antiquités*.

<sup>63</sup> Friedrich, *Ras Shamra*, 21.

<sup>64</sup> Virolleaud, "Le déchiffrement des tablettes alphabétiques de Ras-Shamra", *Syria* 12, 1931, 15-16.

<sup>65</sup> "Séance du 3 Octobre", CRAIBL, 1930, 276-277.

<sup>66</sup> "Séance du 3 Octobre", 276. Virolleaud gives no indication of precisely when he had isolated these words. As we have seen, at least as late as April 9, 1930, there is no unambiguous evidence (see note 23) that Virolleaud thought the language of the tablets was Semitic. The earliest evidence that Virolleaud was of this opinion is a remark made by Dussaud to this effect in the May 23, 1930 *Académie* meeting ("Séance du 23 Mai", 131; cf. Dussaud's slightly later "Déchiffrement", 200). Presuming that Dussaud would have informed Virolleaud about his contact with Bauer, and given that Bauer had been in contact with Dussaud since late April, Virolleaud would have had to change his mind sometime in mid- to late April if he reached the conclusion that the language was Semitic independently of Bauer. Given that Dussaud credits Bauer with being the first to have the idea that the language was west Semitic (*Les découvertes*, 64), I consider it unlikely that Virolleaud reached this conclusion prior to and independent of Bauer.

<sup>67</sup> "Séance du 3 Octobre", 277. Virolleaud, like Dhorme, was right about *m*, and he was right about assigning the value *k* to one of the two signs that Bauer had read as *w*. Virolleaud erred in assigning the value *s* to the sign Bauer read as *m*; rather, as Dhorme had concluded, the correct value was *š*.

in his *FF* article, but in *Entzifferung* he proposed incorrect signs for both<sup>68</sup>. Moreover, *t* was one of the very few letters *not* represented in his *Entzifferung* list, so Virolleaud's comment that he had identified it, without classifying it as a correction, presumes that he knew that Bauer had not proposed a sign for *t*. Thus, prior to October 1, Virolleaud either had a copy of *Entzifferung* or knew Bauer's alphabet by some other means.

In his October 1 letter, Virolleaud goes on to claim that he had definitively identified 26 out of 28 signs<sup>69</sup>, but the letter does not include a list of signs with corresponding values. From his comparison of his results with Bauer's it is clear that Virolleaud had made at least two errors: Bauer's *m* would prove to be *š* (Virolleaud thought it was *s*) and his *p* would prove to be *s* (Virolleaud thought it was *š*). The latter error additionally tells us that Virolleaud was still assigning some value other than *š* to the axehead sign that proved to be *š*<sup>70</sup>. On the basis of the demonstrable errors, then, the most signs that Virolleaud could have had right by October 1 was 23, with one additional sign nearly right (i.e. his *s* should have been *š*). Virolleaud's letter closed by offering to make a presentation to the *Académie* delineating his method and communicating his principal results.

Also on October 1, 1930, after having had the benefit of consulting Dhorme's *RB* 39 article in proofs, Bauer communicated to Dussaud his transcription of the first six lines of tablet 12<sup>71</sup>. On October 2 Virolleaud wrote Bauer a letter that he described in his 1936 publication *La légende phénicienne de Danel* as communicating to Bauer the complete results of his research<sup>72</sup>. In response to this claim Bauer published and commented upon Virolleaud's letter<sup>73</sup>, and reading the letter makes it abundantly clear that Virolleaud in

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<sup>68</sup> Bauer, *Entzifferung*, 14.

<sup>69</sup> "Séance du 3 Octobre", 277.

<sup>70</sup> See note 85. In "Le déchiffrement" (15), Virolleaud claimed to have determined prior to Sept. 20, 1930, that Bauer's value *z* for the axehead sign in question was incorrect. However, the October 1 letter indicates that Virolleaud had not determined the correct value, as he assigned the value *š* to Bauer's *p*, not to Bauer's *z*.

<sup>71</sup> Bauer, "Zum Alphabet von Ras Schamra", *OLZ* 33, 1930, 1062 n. 2. Perhaps Dussaud had not received this communication by October 3 as there is no reference to it in the record of the October 3 *Académie* meeting.

<sup>72</sup> Virolleaud, *Danel*, 71 n. 4.

<sup>73</sup> Bauer, "Zur Entzifferung der Keilschrift von Ras Schamra", *OLZ* 40, 1937, 81-83. As Virolleaud did not subsequently dispute the text of the letter, I am assuming it is authentic. Also, Bauer had previously had occasion (*Das Alphabet*, 41-56) to reproduce documents related to the process of decipherment and, keeping in mind that he was doing this before the era of xerox machines, he was essentially faithful to his originals. In *Das Alphabet* he sometimes deleted sections of documents without noting that he was doing so (e.g. he did not indicate that he was not reproducing the beginning of his *Vossische Zeitung* article [43]) and was susceptible to haplography (see [49] the second sentence of his reproduction of his *OLZ* 1930 article, haplography triggered by «P. Dhorme» in sentences two and three of the original), but these flaws are understandable given the arduousness of handcopying and I have detected in them no intention of skewing the record. I can note one case where Bauer seems to make a word substitution (rather than a copying error) that marginally enhances his portrayal of his results (regarding his sign 16 [50], parenthetic «*meist*» replacing «*gewöhnlich*» of the original). It is in his selection of documents and, in at least one possible case (see note 45), his choice of which results to convey, that one needs to exercise caution about his presentation of past events.

*Danel* grossly misrepresented its contents<sup>74</sup>. The letter begins with Virolleaud belatedly thanking Bauer for sending him an unspecified publication and apologizing for his delay in responding on the grounds that he wanted to read Bauer's work carefully but he had been absorbed by studying the texts of the 1930 campaign. Following a very brief description of the 1930 texts, he says that these documents have allowed him to correct errors made by both Bauer and himself. He closes by informing Bauer of his upcoming presentation to the *Académie*<sup>75</sup> and of his letter to the same body which will be read at their October 3 meeting, and states that this letter gives notice of his results to date. As is evident, then, Virolleaud's October 2 letter to Bauer contains no specifics whatsoever concerning the alphabet *per se* and hence, contrary to Virolleaud's assertion, could not have been of any use to Bauer in his ongoing efforts to identify signs. Thus Virolleaud's claim that Bauer adopted his results and incorporated them into his «October 5 alphabet» (see below) without giving appropriate credit to Virolleaud<sup>76</sup> is completely groundless.

Meanwhile, during the first few days of October, Bauer was continuing his efforts, still based entirely on the texts unearthed in 1929. After having written his *wichtiger Nachtrag*, Bauer proceeded to a more systematic consideration of the values Dhorme had proposed and, on October 3, wrote a letter to Dhorme acknowledging where, in his estimation, Dhorme had been correct<sup>77</sup>. Also in this letter Bauer informed Dhorme that he had arrived at a new determination of the signs representing *t*, *ḥ* and *ṣ*, and had consequently had to change his reading of the word for «axe» from *grzn* to *ḥrṣn*<sup>78</sup>. Two days after writing Dhorme, Bauer produced his so-called «alphabet of October 5»<sup>79</sup>. Of

<sup>74</sup> See note 23. Oddly, Carré ("Anatomy", 19) does not inform his readers that Bauer's "Zur Entzifferung" included the actual text of the letter. He also downplays Virolleaud's clear lack of veracity.

<sup>75</sup> Virolleaud's letter of October 2 specifies to Bauer that he will be presenting his results to the *Académie* on October 17, whereas the CRAIBL records indicate that he made his presentation on October 24. In a letter dated October 30, 1930, Cumont writes that Virolleaud had made a presentation on October 17 (Bonnet, "La découverte archéologique", forthcoming). Thus it seems that Virolleaud had been assured of an Oct. 17 presentation date to the *Académie*, that he was alerting colleagues to that date but, for some reason, his presentation was postponed.

<sup>76</sup> Virolleaud, *Danel*, 71-72 n. 4.

<sup>77</sup> Dhorme, "Première traduction", 32.

<sup>78</sup> Dhorme, "Première traduction", 32. Bauer was right about all three values. To support his new reading *ḥrṣn*, Bauer cited as cognates Akkadian *ḥaṣinnu* and Aramaic *ḥaṣṣina*. Elsewhere, Dhorme ("Le déchiffrement", 4) accounts for the *r* in *ḥrṣn*, as compared to the absence of *r* in the proposed cognates, by dissimilation. Now, the reader will recall that proposing *grzn*, «axe», as the correct reading for four signs on one of the axeheads played a central role in Bauer's early efforts, and accepting this reading provided Dhorme with a way out of his impasse regarding *n* and *t*. Thus it is supremely ironic that these four signs, so crucial in the process of decipherment, may very well not be a word for «axe» at all. In a recent article, P. Bordreuil has proposed a different interpretation of these four signs ("Le premier mot de l'herminette inscrite découverte à Ras Shamra en 1929: outil ou personnage?", in *Und Mose schrieb*, 127-132). Building on earlier observations by R. De Langhe and C. Gordon, Bordreuil assembles a strong case for reading the four signs *not* as a word for «axe» but rather as a personal name cognate with Phoenician *ḥrṣ*, «gold». Thus, if Bordreuil is right, an incorrect hypothesis, by sheer coincidence, played a critical role in correctly deciphering Ugaritic.

<sup>79</sup> This is the alphabet Bauer published in "Zum Alphabet" (1062-3), where he also published his transliteration of the first 6 lines of tablet 12. (For Bauer's transliteration and translation of tablet 12

the 26 signs for which he proposed values, only one (which he himself questioned) was entirely wrong, 3 were nearly correct<sup>80</sup> and 22 were correct. Expressed in terms of correct values rather than correct identification of signs, Bauer's October 5 alphabet had 24 correct values and 1 nearly correct. He credited Dhorme as the source of establishing 5 values that he had not discerned on his own<sup>81</sup>.

On October 24, 1930, Virolleaud made his presentation to the *Académie*. At this juncture, Virolleaud had had the opportunity since at least early October to consult both Bauer's *Entzifferung* and Dhorme's "Un nouvel alphabet sémitique"<sup>82</sup>. The record of the October 24 meeting indicates that, as promised, Virolleaud described the method he employed in deciphering the texts but, as is understandable for this genre of recording, no specifics are mentioned<sup>83</sup>. The record makes some mention of Virolleaud's results, but not with respect to his identification of signs *per se*<sup>84</sup>. It is clear, however, that Virolleaud's presentation essentially accords with his "Le déchiffrement des tablettes alphabétiques de Ras-Shamra", as he himself states<sup>85</sup>. In "Le déchiffrement", Virolleaud

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in its entirety, see his "Ein kanaanäisches Alphabet in Keilschrift", ZDMG 84, 1930, 251-254). Presumably to authenticate October 5 as the date by which he had arrived at this alphabet, Bauer noted (1062 n. 2) that he had communicated this alphabet to Dussaud on October 5. (Dhorme ["Première traduction", 32-33] noted that Bauer had also sent him his revised alphabet on October 5.). While Bauer's "Zum Alphabet" did not appear in print until December of 1930 (Friedrich, *Ras Shamra*, 21), the fact that Dussaud received Bauer's revised alphabet some time shortly after October 5 means that, theoretically, Bauer's revisions were available to Virolleaud approximately two weeks prior to his October 24 presentation to the *Académie*.

- <sup>80</sup> In "Zum Alphabet" (1062) Bauer listed his proposed values alongside sign numbers, but did not draw the signs themselves. In *Das Alphabet* (50) Bauer did provide the respective signs for each of the sign numbers of "Zum Alphabet", so it is easier to work with his October 5 alphabet in the *Das Alphabet* format. I include in my category of nearly correct 1) sign 4, which he correctly identifies as *q* but to which he also incorrectly ascribes the value  $\dot{g}$ , by which transcription he represents what is now more commonly transcribed as *g* (see *Entzifferung*, 9 line 5), 2) sign 16, which he incorrectly transcribes as *s*, but (if I understand rightly his parenthetical transcription) which he almost correctly notes as usually (see note 73) representing Protosemitic \**t* and 3) sign 21, which he correctly transcribes as *z* but incorrectly gives the additional value *z*.
- <sup>81</sup> Bauer, "Zum Alphabet", 1062. Regarding the correct identification of *p*, Bauer credited Dussaud as well as Dhorme.
- <sup>82</sup> Virolleaud, "Le déchiffrement", 16. Virolleaud is (in my reading) intentionally ambiguous here as to whether he is saying the two works in question appeared at the beginning or toward the end of October. We know (see notes 60 and 61) that *Entzifferung* was released either in late September or early October, and Dhorme's "Un nouvel" was published in the October 1 fascicle of RB, though Dussaud recalls (*Les découvertes*, 64 n. 7) that this October fascicle appeared in September. Thus Virolleaud, in theory, had had ample time to consult these works before making his presentation and, also in theory, had had access to Bauer's October 5 alphabet through Dussaud. Virolleaud in *Danel* (72) noted that Dhorme had been working independently of him, but does not say that he had been working independently of Dhorme. Re the October fascicle of RB appearing in September, note that Dhorme, as director of the *École Biblique* at this time, was also the editor of RB (A. Guillaumont, "Édouard Dhorme", RHR 169, 1966, 126) and therefore was in an excellent position to speed up publication of the fascicle.
- <sup>83</sup> "Séance du 24 Octobre", CRAIBL, 1930, 301.
- <sup>84</sup> "Séance du 24 Octobre", 302.
- <sup>85</sup> Virolleaud, "Le déchiffrement", 16. This statement does not apply to the first section of the article, and Virolleaud qualified the statement by noting that he had made some slight additions to his

referred to his earlier deduction (in *Syria* 10) that the first sign of tablet 18 should be the equivalent of the preposition «to», but in contrast to this earlier publication he now says that, given Ras Shamra's location in northern Phoenicia, he had hypothesized that this sign corresponded to the preposition *ʾ*<sup>86</sup>. He then described how identifying *l* had led him to some other, common Semitic words, but that being able to read a few words here and there was insufficient to establish definitively the nature of the language. The 1930 texts were necessary to accomplish complete decipherment which, apart from one letter out of 28, Virolleaud stated that he had succeeded in doing<sup>87</sup>. In the text of the article itself, however, Virolleaud identified not 27 signs but 26, of which 2 are entirely wrong, 2 are nearly correct and 22 are correct<sup>88</sup>. Given that it is impossible to ascertain precisely how Virolleaud might have benefitted from the previously completed work of Bauer and Dhome and where his results were the product of purely independent research, the most meaningful observation I can make is that Virolleaud at this point was the only one to have discerned *z* and a third «*aleph*».

Indeed it is mystifying why Virolleaud did not made public until October of 1930 any of the progress he claimed he had been making. This must have seemed mysterious to his contemporaries as well, as his opening paragraphs in “Le déchiffrement”<sup>89</sup> are an answer to the implicit question of why he did not make his results public sooner. He says that he had been on the verge of publishing in mid-May his findings to date when Bauer announced<sup>90</sup> that he had essentially accomplished decipherment of the texts. But because Bauer's book took a long time to appear, he continued making progress on the 1929 texts until Schaeffer informed him of the important 1930 finds<sup>91</sup>. Virolleaud does not specify when Schaeffer told him about the 1930 texts, but according to Schaeffer's second campaign report, the alphabetic cuneiform texts were found between the beginning and the middle of May<sup>92</sup>. Thanks to Dussaud, the *Académie* knew, as of their May 23, 1930

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presentation. In the first section of the article, Virolleaud claims that, sometime after Aug. 20 and prior to September 20, 1930, he had ascertained that Bauer was wrong about *g* and *z*. This claim is inconsistent with his October 1 letter to the *Académie*, as at that juncture Virolleaud did not aver that Bauer's *g* and *z* were incorrect. Regarding *z*, see note 70.

<sup>86</sup> Virolleaud, “Le déchiffrement”, 16. Virolleaud gives no indication of precisely when he reached this conclusion. As we have seen, the earliest indication that Virolleaud thought that the language of the tablets was Semitic is Dussaud's comment to that effect in CRAIBL (“Séance du 23 Mai”, 1930, 131).

<sup>87</sup> Virolleaud, “Le déchiffrement”, 17-18. Note that his October 1 letter says 26, not 27.

<sup>88</sup> Corré (“Anatomy”, 18) identifies the missing letter as *w*, and guesses that Virolleaud had probably identified it correctly. Nearly correct are the value *š* for the sign for *l* and the value *s* for the sign for *š* (and note his discussion).

<sup>89</sup> These paragraphs were not part of Virolleaud's Oct. 24 presentation to the *Académie* (Virolleaud, “Le déchiffrement”, 16) and may have been composed as late as June, 1931 (see note 102).

<sup>90</sup> Actually, as we have seen, Dussaud made the announcement, and it is to Dussaud's announcement that Virolleaud refers in his footnote (“Le déchiffrement”, 15 n. 2). Bauer's initial announcement to Dussaud had taken place on April 28 (see note 40).

<sup>91</sup> Virolleaud, “Le déchiffrement”, 15.

<sup>92</sup> Schaeffer, “Les fouilles de Minet-el-Beida et de Ras Shamra, deuxième campagne (printemps 1930)”, *Syria* 12, 1931, 1, 4, 8.

meeting, that Schaeffer had unearthed more cuneiform texts<sup>93</sup> and so Virolleaud must have known about them on or about this date. Subsequently, he also must have known that Bauer was busy collating the 1929 texts in the Louvre<sup>94</sup> and hence would not be publishing a book on the topic until he had finished collating. Thus Virolleaud's explanation for not publishing earlier does not accord well with information available elsewhere in the contemporary record. Virolleaud's explanation implies that a long time elapsed between mid-May and when he learned about the 1930 texts, which is a distortion of the facts and therefore not a credible explanation.

Virolleaud's account in "Le déchiffrement" of what he had deciphered on the basis of the 1929 texts is puzzling for yet another reason. Recall that Virolleaud had suggested in his *editio princeps* that finding a four-consonant word for «axe» could well lead to the decipherment of the language. Yet Virolleaud nowhere in "Le déchiffrement" (or in any other publication) gives any indication that he had followed up on what, in his own estimation, was a potentially crucial lead. Pursuing this proposal had led Bauer in very short order to Hebrew *grzn* and, in spite of the fact that *grzn* proved not to be an entirely correct reading, this initial identification facilitated substantial progress for both Bauer and Dhorme. So why did Hebrew *grzn* evade Virolleaud and find no place in his description of his process of decipherment? I submit that *grzn* evaded Virolleaud because Virolleaud, prior to learning about Bauer's work through Dussaud, was not looking towards the Semitic languages for a cognate. This submission is consistent with the fact that Virolleaud's presentations and publications through the first four months of 1930 contain no indication whatsoever that he thought the language of the tablets was Semitic. Once Virolleaud began to investigate the possibility that the language was Semitic, he wanted to distance himself as much as possible from Bauer's (and later, Dhorme's) work. This is evident from his vagueness regarding specifically when he arrived at his various conclusions<sup>95</sup>, as well as his reticence to discuss whether and to what degree he was dependent on either Bauer's or Dhorme's work<sup>96</sup>. That Bauer had proposed *grzn* as the appropriate cognate was public knowledge as of the publication of Dussaud's announcement of Bauer's success in *Syria* 11 and, as we have seen, *grzn* was the only word traceable to specific signs in Bauer's June 4, 1930 *Vossische Zeitung* piece. To maintain distance from Bauer's work, Virolleaud would either have had to claim that he

<sup>93</sup> "Séance du 23 Mai", 131. This is the same meeting in which Dussaud announced that Virolleaud thought that the language of the tablets was Semitic, and so it is clear that the two scholars had been in contact prior to May 23.

<sup>94</sup> Bauer in *Entzifferung* (VII-VIII and 7) refers to his trip to Paris to collate the texts. This must have taken place before Aug. 4, 1930, as that is the dedication date of *Entzifferung* (cf. Dhorme, "Un nouvel", 576). Dhorme was under the impression that Bauer was working with both Dussaud and Virolleaud while at the Louvre ("Un nouvel", 576) but if this had been the case, Bauer surely would have mentioned it, and he did not.

<sup>95</sup> The exception, as we have seen, is the time line he proposes in *Danel*, but as we have also seen, several of Virolleaud's recollections in *Danel* are demonstrably inaccurate.

<sup>96</sup> Virolleaud's reticence is also noted by Friedrich (*Ras Shamra*, 21). Recall also (see note 94) that Dhorme thought that Virolleaud would be working with Bauer and Dussaud when Bauer went to the Louvre to collate the texts. If Virolleaud had declined an invitation to work with Bauer, his non-participation could be construed as another effort to distance himself from Bauer's work.

reached the same conclusion prior to and independently of Bauer (which would render his self-proclaimed decision not to publish in mid-May virtually inexplicable), or ignore *grzn* entirely in descriptions of his process of decipherment. Virolleaud apparently chose the latter option.

Following the October 24 *Académie* meeting, the French press reported Virolleaud's success at decipherment as if, to that point, no other researchers had made any progress whatsoever in deciphering the Ras Shamra alphabet<sup>97</sup>. Dhorme was understandably upset when he learned of these press releases, as he had known nothing of Virolleaud's work until he read the releases and he knew the press' characterization of Virolleaud's work to be untrue<sup>98</sup>. Choosing not to wait until the publication of the 1930 texts, Dhorme continued working on the 1929 texts and, by December 8, 1930, had produced a revised alphabet. With appropriate credit to Bauer, Dhorme proposed values for 26 signs, 24 of which were correct<sup>99</sup>. As Virolleaud later acknowledged, Dhorme had arrived at these revised results completely independently of Virolleaud<sup>100</sup>. Indeed, the (prefaced and slightly supplemented) text of Virolleaud's October 24 presentation to the *Académie* did not appear in *Syria*<sup>101</sup> until July 1931<sup>102</sup>, and the first publication of his complete alphabet<sup>103</sup> did not appear until January 1932<sup>104</sup>. Of the 29 signs for which Virolleaud proposed values in this latter publication, 2 are wrong, 3 are nearly correct<sup>105</sup> and 24 are correct. He listed but did not identify a 30th sign which, later in the article, he speculated (incorrectly) might be a third *h*<sup>106</sup>.

Thus, though a few problems remained outstanding, by January of 1932 the decipherment of Ugaritic was essentially complete and the results published. How, then, should we evaluate the respective contributions of Bauer, Dhorme and Virolleaud?

<sup>97</sup> Dhorme, "Première traduction", 33. Dhorme does not specify the newspapers in question. Corré ("Anatomy", 18 n. 30) cites a portion of an article that appeared in *Le Figaro* on October 25.

<sup>98</sup> Dhorme, "Première traduction", 33 and n. 2. One of these errant French newspaper articles was apparently translated into German and then appeared, on *September 17, 1931* [!!] in the *Hallischen Nachrichten*, a newspaper published in the very city where Bauer taught. For the text of the article, see Bauer (*Das Alphabet*, 54-56). O. Eissfeldt's rejoinder to this article, which was published in the same newspaper on September 19, can be found in his *Ras Shamra und Sanchunjon*, Halle 1939, 1-4.

<sup>99</sup> Dhorme, "Première traduction", 32-33. I am assuming that the dot under the value *p* was inadvertent, as Dhorme had correctly identified the sign for *p* previously, in his RB 39 sign list. In addition to incorrectly identifying 2 signs, Dhorme also incorrectly attributed a second value (*z*) to the sign for *d*.

<sup>100</sup> Virolleaud, *Danel*, 72.

<sup>101</sup> Virolleaud, "Le déchiffrement", *Syria* 12, 1931, 15-23.

<sup>102</sup> Friedrich, *Ras Shamra*, 21. Virolleaud had earlier (December 12, 1930) made a presentation to the *Société Asiatique* (JA 218, 1931, 171) but the report of the presentation contains no information not present in "Le déchiffrement". We can learn, however, that as of December 12, 1930, Virolleaud still thought there were 28 and not 30 signs.

<sup>103</sup> Virolleaud, "Un poème phénicien de Ras-Shamra", *Syria* 12, 1931, 194.

<sup>104</sup> Friedrich, *Ras Shamra*, 21.

<sup>105</sup> Virolleaud incorrectly equated the sign now transcribed *š* with Hebrew *samek*, and misidentified *š* as *s* and *ʃ* as *ʃ* (but see his n. 1).

<sup>106</sup> Virolleaud, "Un poème", 203.

The distinction of accomplishing the initial stage of decipherment rightfully belongs to Hans Bauer. In tandem with the method he developed for assigning values to affixes and single grapheme words, Bauer capitalized on the purely formal observations Virolleaud had made in his *editio princeps* and thereby independently discerned numerous sign values correctly. Bauer's work also provided Dhorme with a way out of his early impasse regarding the letters *n* and *t*. Bauer was evidently scrupulous about his dating of relevant events and about acknowledging his eventual indebtedness to Dhorme. The fact that he initially *had* made significant errors led him, I surmise, to overestimate the integrity of his method and overlook its flaws when he presented his explanation of how he had made those errors. In any event, for reasons already elucidated, I cannot agree with Bauer and those of his contemporaries who maintained the correctness of his method in spite of the errors in his initial results. It is correct to say, however, contrary to what Virolleaud claimed in *Danel*, that Virolleaud made no contribution to Bauer's revised alphabet of October 5, 1930. By this date, and solely on the basis of the 1929 texts, Bauer, with due credit to Dhorme, had correctly or almost correctly discerned the consonantal values of 25 signs.

Though Paul Dhorme did not solve his impasse prior to seeing Bauer's *Vossische Zeitung* article, the only specific information about sign values that Dhorme possibly could have gleaned from it was Bauer's reading of *grzn* for four signs on one of the axeheads. As two of these values proved to be wrong, Bauer's contribution to Dhorme was minimal, numerically at least. Indebted to Bauer only for *r* and *n*, Dhorme succeeded in puzzling out 18 or 19 correct sign values by August 15, 1930. As soon as he recognized that his and Bauer's alphabets differed more dramatically than he had originally surmised, he shared his (as yet unpublished) results with Bauer. This act initiated an exceedingly fruitful scholarly cooperation that was of benefit not only to both men personally but also to the decipherment process itself, as it facilitated the rapid elimination of errors. Thus by December 8, 1930, Dhorme had correctly discerned the values for 24 signs, based solely on the 1929 texts. Like Bauer, Dhorme was conscientiously forthcoming about providing relevant dates and giving Bauer due credit. Given the clarity of the record in these two respects, it is possible to say with virtual certainty that Bauer and Dhorme achieved their combined results with no contribution from Virolleaud beyond the formal observations he had made in his *editio princeps* and without any recourse whatsoever to the texts unearthed in the 1930 campaign.

This leaves us with assessing the role of Virolleaud. Viewed without attention to context, it seems in retrospect almost incredible that Virolleaud could have speculated that a consonantly and alphabetically written language discovered on the coast of Syria would be anything other than a West Semitic language. However, Virolleaud's close ties at that time to the excavation itself and the early pottery finds, as well as his close (albeit professionally problematic) ties to Dussaud and to the intellectual enterprise of pondering connections between preclassical and oriental cultures, explain why Virolleaud initially overlooked the obvious. At least until April 9, 1930, Virolleaud evidently was of the opinion that the language of the alphabetic texts was non-Semitic. Indeed, and though proper caution needs to be exercised when making an observation from silence, there is no record of Virolleaud recognizing the language as Semitic until Dussaud's May 23,

1930 report to that effect to the *Académie*<sup>107</sup>. At this juncture Dussaud had been in contact with Bauer since late April and so, in order to state with absolute certainty that Virolleaud recognized the language as Semitic independently of Bauer, one would have to postulate that Virolleaud reached this conclusion in the three week window between April 9 and the end of the month. Given that Dussaud, who was in close contact with both Bauer and Virolleaud, credited Bauer with first recognizing that the language was West Semitic, it is highly unlikely that Virolleaud reached this conclusion independently. Unlike Bauer and Dhorme, Virolleaud is positively and consistently opaque both about specifically when he reached his respective conclusions as well as the relationship of his work to Dhorme's and Bauer's. We can deduce from his October 1, 1930, letter to the *Académie* that Virolleaud knew Bauer's unrevised alphabet by this date, but cannot determine the degree to which Bauer's work had contributed to Virolleaud's to this point. Additionally, Virolleaud had access to Dhorme's first publication (and, possibly, to Bauer's revised alphabet, through Dussaud) weeks in advance of his October 24 presentation to the *Académie*, but again, we cannot ascertain to what degree these works may have contributed to Virolleaud's findings due to Virolleaud's lack of disclosure. The time line and comments Virolleaud provides in his 1936 *La légende phénicienne de Danel* cannot serve to clarify matters because, as we have seen, several of his recollections in that publication are simply not trustworthy. Given this consistent *pattern* of non-specificity, ambiguity and inconsistency, I can only conclude that Virolleaud had something to hide. This conclusion is strengthened by noting that Virolleaud, according to his own testimony, *chose* to keep his (alleged) results private. From mid-May through September of 1930 he chose not to publish at all<sup>108</sup>, and on October 1, when he finally

<sup>107</sup> Bonnet ("Les lettres", 124 n. 36) has noted that, among the four letters conserved at the Belgian Academy that Virolleaud wrote to Cumont, only the one written on June 8, 1931 contains information pertinent to the Ras Shamra texts and so this letter is not primary evidence for the period under discussion.

<sup>108</sup> Corré ("Anatomy", 18) acknowledges this point when he states that Virolleaud «seemingly ... was unwilling to publish» until he could confirm his findings via examination of the 1930 texts. Corré takes a much more positive view of Virolleaud's unwillingness to publish. He thinks it «entirely possible that Virolleaud had achieved a partial, or perhaps almost complete, decipherment before the others ever started» ("Anatomy", 19) but, as we have seen, Corré was unaware of Virolleaud's presentations of March 22 and April 9, 1930. Corré also erred in attributing the entire substance of Virolleaud's "Le déchiffrement" to October 24, which led him to state that «Virolleaud's exposition of October 24, 1930, shows such detailed understanding of the *contents* of the tablets that it is clear that the decipherment was far behind him» ("Anatomy", 19-20). However, careful comparison of the CRAIBL record of October 24 with "Le déchiffrement" indicates that it is precisely on the topic of the contents of the tablets that the two documents diverge, which clearly indicates that Virolleaud had revised the text of his October 24 presentation regarding contents. As we have seen, "Le déchiffrement" did not appear until July, 1931, giving Virolleaud a substantial amount of time to appreciate the contents of the tablets more fully. In further support of Virolleaud, Corré states that Virolleaud «himself testifies that he was just about ready to publish his decipherment *when Bauer communicated his finding to Dussaud* ("Anatomy", 20, emphasis mine). If accurate, this would bolster Corré's case for Virolleaud having discerned that the language was Semitic independently of Bauer. However, as we have seen, Bauer first communicated his results to Dussaud on April 28. The testimony to which Corré refers is Virolleaud's statement ("Le déchiffrement", 15) that he was on the verge of publishing his results when, *in mid-May*, Bauer [*sic*, see note 90] announced his results. Thus again, Corré clearly errs, and recognizing the error once again undermines Corré's favourable assessment of Virolleaud. Finally, Corré credits Virolleaud with «scholarly altruism» on the grounds

did put something in writing, he chose not to include a list of signs with corresponding values. Even when he revised and published his October 24 presentation to the *Académie* he did not include a sign list. Thus the fact that Virolleaud's alphabet was the last of the three to appear in print was not due simply to happenstance. Rather, Virolleaud evidently wanted it to be that way.

Why would Virolleaud have purposely delayed committing his results to writing? I submit that the answer lies in a claim that echoes throughout his publications on the topic of the decipherment. Virolleaud could not plausibly have claimed to have accomplished the first decipherment, so he attempted to redefine what counted as success. As we have seen, Virolleaud claimed that complete decipherment was not possible on the basis of the 1929 texts alone, a claim that he repeated frequently<sup>109</sup>. He used this claim to distinguish his accomplishments from those of Bauer and Dhorme and to maintain that complete decipherment was the right and proper goal<sup>110</sup>. I suggest that Virolleaud put off committing his results to writing precisely so that he could claim to be the first to have accomplished complete, as opposed to initial and partial, decipherment. Not committing his results to writing gave him additional time to verify those results. As the record shows, however, unfortunately for Virolleaud, his repeated claim that decipherment was not possible on the basis of the 1929 texts alone is demonstrably unfounded<sup>111</sup>. Working solely with the 1929 texts, Bauer and Dhorme had correctly or almost correctly identified 25 signs. With the added benefit of the 1930 texts, the *most* that Virolleaud could have correctly or nearly correctly identified by October 24 was 25 signs (i.e. 24 explicitly identified, plus giving him the benefit of the doubt for the missing *w*). These tallies also belie Virolleaud's attempt to claim the distinction of complete decipherment. Thus, by both Virolleaud's criteria as well as my own, I must conclude that Virolleaud's role in the decipherment of Ugaritic has been generally overrated.

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that Virolleaud «could easily have delayed publication of the [1929] tablets until he was sure of a decipherment, or despaired of achieving one» (“Anatomy”, 20). But, as we have seen, Schaeffer obtained a commitment from Virolleaud to publish the texts by the end of 1929 in exchange for handing over to him the complete 1929 corpus. This raises a doubt about whether Virolleaud indeed could *easily* have delayed publication for very long. As we have seen, publication of the 1929 fascicle of *Syria* that contained Virolleaud's *editio princeps* was delayed until April, 1930, though whether this was due to footdragging on Virolleaud's part is nowhere explicit in the record. What can be said is that the article appeared four months *later* than the date to which Virolleaud had committed and so, whatever the reason, publication was in fact delayed. By early April Virolleaud had had access to the corpus for ten months and had not even discerned that the language was Semitic, so it is possible that Virolleaud *had* in fact despaired of achieving decipherment before making his drawings and transcriptions available to other scholars.

<sup>109</sup> “Séance du 3 Octobre”, CRAIBL, 1930, 277; Virolleaud, “Le déchiffrement”, 17-8; December 12, 1930 report to the *Société Asiatique*, published in JA 218, 1931, 171; Virolleaud, “La littérature phénicienne retrouvée”, *Revue archéologique*, 1931, 297; *Danel*, 71-72 n. 4.

<sup>110</sup> JA 218, 1931, 171; Virolleaud, “Le déchiffrement”, 18; *Danel*, 71-72 n. 4.

<sup>111</sup> So also Dhorme, review of *Danel*, *Syria* 18, 1937, 112-113.

ABSTRACT

Three scholars are generally credited with major roles in the decipherment of Ugaritic: Hans Bauer, Paul Dhorme and Charles Virolleaud. This article presents a comprehensive account of the precise roles that each of these scholars played, superseding previous treatments by incorporating information heretofore overlooked as well as by attending more fully to data such as paper presentation dates, article completion dates, private communications and intellectual context. This more comprehensive examination offers an explanation as to why Virolleaud initially overlooked the possibility, obvious in retrospect, that the alphabetic cuneiform of Ras Shamra conveyed a Northwest Semitic language. The record further indicates that, collaboratively, and solely on the basis of the 1929 texts, Bauer and Dhorme deciphered as many or more of the Ugaritic signs than Virolleaud deciphered with the added advantage of access to the texts unearthed in 1930. Thus Virolleaud's oft-repeated assertion that the 1929 texts provided an insufficient basis for decipherment, as well as his claim (in October of 1930) to have accomplished virtually complete decipherment, are groundless.