A Glance at the History of Linguistics
With particular regard to the historical study of phonology

Holger Pedersen (1867–1953)
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Volume 7

Holger Pedersen

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WITH PARTICULAR REGARD
TO THE HISTORICAL STUDY OF PHONOLOGY

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Almost ten years have passed since I approached Dr. Caroline C. Henriksen of Copenhagen to consider undertaking an English translation of Holger Pedersen's 1916 study, *Et Blik på Sprogvidenskabens Historie*, a smaller and, as the subtitle reveals, a much more restricted account of the study of language in the 19th century. It was my view that this earlier text could shed interesting light on Pedersen's later synthesis, which, in its 1931 translation, had become so popular, particularly in North America, and that his account of the development of historical phonology in the earlier study would be more easy to follow, prior to the 'Saussurean Revolution' in linguistics at least.

Subsequent to this change in the disciplinary matrix the material available in historical-comparative Indo-European linguistics became studied with perhaps an increasing intensity, but surely by a gradually decreasing number of students. As the result of a misunderstanding of Saussure's true intentions (which were largely misrepresented by the editors of the *Cours*), the idea had gained widespread currency that synchronic linguistics could not only be dealt with quite separately from diachronic linguistics, but that the latter was little more than an accessory to the former which could easily be dispensed with.* We may recall that Bloomfield's *Language* of 1933 followed the model provided by the *Cours* in separating these two 'points de vue', even to the extent that the historical portion of his book contains no single cross reference to anything mentioned in the preceding descriptive section, indeed as if there were two sciences of language entirely divorced from each other and as if one such field could operate satisfactorily without reference to the other.

We are now painfully aware of the damage that this misre-

* Comparison between the *Cours* as edited by Charles Bally and Albert Sechehaye and the critical edition prepared by Rudolf Engler (Wiesbaden 1967-68, 1974) reveals that each time the 'vulgata' text speaks of an incommensurability between the synchronic and the diachronic viewpoint, Saussure had merely spoken of a (methodologically important) difference between the two in his Geneva lectures.
presentation of Saussure's teaching has inflicted on the study of language. This overemphasis on a kind of general linguistics, soon to be identified almost exclusively with linguistic theory, and, in various quarters, as the only truly scientific pursuit within the discipline, has tended to lead, under various followers of Noam Chomsky, to a linguistics void of language. As a result, Pedersen's, in his own view 'popular', account of the development of research in historical Indo-European phonology, not least his various 'digressions' (represented by smaller type in the text), has become much more difficult to follow for students of linguistics today than it was in Pedersen's own day. To this we can add the fact that few students today have received training in the classical languages, something which, at least in Europe prior to World War II, formed a regular part of grammar school education. However, lack of a general historical interest, especially in North America, has no doubt had the most unfortunate effect on diachronic linguistics.

Happily, there are signs that there has been a reawakening of interest in historical linguistics in recent years and a growing respect for a more widespread data-orientation in the study of language. Thus, it seems that the delay in publication of the present book may not have been so regrettable after all as the times for an appreciation of a survey of the present kind appear to be better now than they were perhaps ten years ago.

Since texts written in languages other than English have become less easily accessible to many modern linguists, I am grateful to Dr Caroline C. Henriksen for her translation of the Danish text. Personally, I owe her an expression of thanks for her collaboration and for providing me with important supporting material, including drafts of Pedersen's bibliography — originally compiled by Peter A. Henriksen — of the back matter to the present volume, copies of the original texts of Pedersen's 1899 and 1916 works as well as a picture and a signature of Pedersen. Last but not least, I am grateful to her for having read an earlier version of my introduction and for having approved of the organization of the volume as a whole.

Finally, I would like to thank various people at Georgetown University, in particular certain members of its School of Languages and Linguistics and the personnel at the J. M. Lauinger Memorial Library, for assistance received in the research for the present edition. For making my sojourn so pleasant and profitable, I owe special thanks to Kurt R. Jankowsky for his friendship and his ubiquitous and generous support.

Washington, D.C., May 1983

K. K.
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HOLGER PEDERSEN
A SKETCH OF HIS LIFE AND WORK

Holger Pedersen was born on 7 April 1867 as the son of a school teacher at Gjelballe, near Lunderskov, Kolding province, Jutland. After completion of grammar school, with distinction, he entered the University of Copenhagen where he took courses in Greek, Latin, and Danish.1 His teachers were Karl Verner (1846-96), Vilhelm Thomsen (1842-1927), Hermann Möller (1850-1923), and Ludwig Wimmer (1839-1920); one could hardly imagine a better slate of professors in the fields of Classics, Slavic, and Germanic. Following completion of his studies with the highest marks in 1890, Pedersen started his career as an academic, sending off his first papers to the most distinguished journals in historical-comparative linguistics of the period at the age of

twenty-four. These include Adalbert Bezzenberger's *Beiträge zur Kunde der indogermanischen Sprachen*, Karl Brugmann and Wilhelm Streitberg's *Indogermanische Forschungen*, and Adalbert Kuhn's *Zeitschrift für vergleichende Sprachforschung*. The years 1892-96 constituted Pedersen's Wanderjahre. His first station was the Mecca of Indo-European philology, the University of Leipzig, where he studied comparative linguistics with Brugmann (1849-1919), Slavic and Lithuanian with August Leskien (1840-1916) as well as with the much lesser known Robert Scholvin (b.1850), Sanskrit and Celtic with Ernst Windisch (1843-1918), Indo-Iranian with Bruno Lindner (1853-1930), and attended lectures by Eduard Sievers (1850-1936) in the field of Germanic. (Pedersen's important paper, "Das indogermanische s im Slavischen" (Pedersen 1895b), which impressed Brugmann so much that he recommended its publication in *Indogermanische Forschungen*, is dated "Leipzig, d. 14. februar 1893".)

Pedersen's collaboration with Brugmann went so far that he undertook a trip to Korfu with him from March to August 1893 to study Albanian in loco. (Only in 1891 had the Neogrammarians Gustav Meyer [1850-1900] published his *Etymologisches Wörterbuch der albanesischen Sprache* [Strassburg: Trübner].) This field trip resulted in a number of publications. On the recommendation of Brugmann and Leskien, a 200-page edition of Albanian texts with a glossary appeared in the series issued by the Royal Saxon Academy of Sciences in Leipzig (Pedersen 1895a), and articles on Albanian etymology and phonology were published in Bezzenberger's and in Kuhn's Journals (Pedersen 1894a, 1895c), with others following several years later (e.g., Pedersen 1897c), including a German translation of the Albanian texts (Pedersen 1898b). Pedersen continued to publish in this field for many years after, several items appearing in Albanian translation in the journal and series published by the Mekhitarists in Vienna (Pedersen 1904a, e; 1907a, c; 1911) and others in German linguistics periodicals and various additional scholarly outlets (e.g., Pedersen 1900c, d; 1905b, 1906b, 1914, 1924c).

But Pedersen had no intention of becoming a mere specialist of Albanian, and for the Winter semester of 1893/94 we find him taking courses with Johannes Schmidt (1843-1901) and probably others, such as the In-
do-Iranian philologist Karl Friedrich Geldner (1853-1929), at the University of Berlin. (Pedersen 1895c, for instance, is dated "Berlin, den 7. februar 1894"). According to Hjelmslev (1973:32), Pedersen profited greatly from Schmidt's teachings, but when he goes on to state that Pedersen learned nothing from Georg von der Gabelentz (1840-1893), this cannot only reflect on Pedersen's interest in Chinese and general linguistics, which no doubt was scant at best, but it must also be noted that Gabelentz had died in December 1893 and therefore could not be frequented by Pedersen any more.

Pedersen's publication record indicates that he had moved to the University of Greifswald by Fall 1894 — compare the date indicated at the end of Pedersen (1897c): "Greifswald, den 19. december 1894". There, he studied Sanskrit and particularly Celtic with Heinrich Zimmer (1851-1910), a scholar who must have impressed him profoundly, since he continued to work in Celtic studies from that time onwards for most of his long life, in particular until about 1930, by which time several other branches of Indo-European were receiving his attention. Pedersen spent the summer and fall of 1895 on the Aran Islands, west of Galway, Ireland, studying a particularly conservative variety of Gaelic. As it happens, this West-Irish dialect had been studied by Franz Nikolaus Finck (1867-1910) in the previous year; but before Finck's two-volume grammar and dictionary left the press, Pedersen had been able to communicate to him a number of errors and omissions he had noted in the 1896 publication of the Wörterbuch. They were added as 47 pages of "Nachträge" to the two-volume publication, Die Araner Mundart: Ein Beitrag zur Erforschung des Westirischen (Marburg: Elwert, 1899). The first result of Pedersen's field work however was a short paper on modern Irish incantations (Pedersen 1896). But, most importantly, it led to his doctoral dissertation submitted to the University of Copenhagen in 1896, and published subsequently as a monograph (Pedersen 1897a) and a

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2 Although the first papers appeared with an 1893 imprint, several of them were submitted as early as 1891, as may be gathered from the places and dates given by him at the end of his contributions; e.g., his second paper (Pedersen 1893b) is dated "Kopenhagen, 25. mai 1891".

3 Many years later, in 1928, a number of these Albanian folk-tales were translated, together with five others collected by Auguste Dozon (1822
130-page contribution to Kuhn's Zeitschrift (Pedersen 1897a), which he dedicated to his former teacher Zimmer. By that time, Pedersen had become firmly established in the scholarly field.

In November 1896 Karl Verner had died, and Pedersen, who defended his thesis early in 1897, with Thomsen and Otto Jespersen (1860-1943) as jury members, was wondering already then whether he should not give more attention to Slavic, the subject taught by Verner, though it was not before the turn of the century that Pedersen published on the subject (e.g., Pedersen 1902b, 1904c,d; 1905c, 1909b, etc.). In 1897, Pedersen became a lecturer in Celtic, adding Slavic to his subjects of instruction in the following year; by 1900, Pedersen was a reader in comparative grammar at the University of Copenhagen. When, in 1902, he received the offer of a professorship at the University of Basel, he declined the offer but managed to persuade the authorities at his own university to establish an extra-ordinary professorship for him in the following year. He had to wait ten years to receive the chair held by Vilhelm Thomsen who retired late in 1912 at the age of seventy.

Before ascending the last rung of the academic ladder, however, Pedersen had received yet another offer of a professorship abroad: this time, in 1908, from the University of Strassburg, since the Franco-Prussian war a German university. But Pedersen had no inclination to be stationed outside his home country, though in matters of scholarship he was by no means a nationalist. In 1909 and 1913, Pedersen published his magnum opus, a 1400-page Comparative Grammar of Celtic, written in German with the imprint of Vandenhoeck & Ruprecht in Göttingen, where

to 1891), and published by Paul Pennimore Cooper in a volume entitled Tricks of Women and other Albanian Tales (New York: W. Morrow & Co., 1928), xvi, 204 pp.

4 Whenever there is a divergence in matters of dates between Sommerfelt (1966) and Hjelmslev (1973) — see end of footnote 1 for details —, I have followed the latter as he was more closely associated with Pedersen, and for a longer period of time, succeeding to his chair in 1927.

5 There is no hint in his work that he held particularly strong anti-German sentiments, as is frequently expressed by Danish scholars especially after World War II. Cf. Cahen's affirmation of 1916 that Pedersen "n'est pas un germanophage" (see note 14 below, p.153).
it was reissued as late as 1976. In 1937, *A Concise Celtic Grammar* appeared with the same publisher, a project on which he had collaborated with the Welsh scholar Henry Lewis (1889-1968). By then, Pedersen had become a very broad Indo-Europeanist, who had distinguished himself in the areas of Celtic, Armenian, Slavic — he published a Russian grammar and textbook in 1916 — and Baltic, in addition to Greek and Latin, languages with which he was familiar from childhood. Pedersen also addressed general questions concerning the Indo-European proto-language and Germanic. From around 1930 onwards and especially following his retirement in 1937, he devoted more attention to those languages that only in our century had been identified as Indo-European, Hittite and Tocharian (e.g., Pedersen 1933b, 1934a, 1935a, 1938, etc., and 1941, 1943b, 1944, etc., respectively). During this period, Pedersen dealt with questions of method in Indo-European linguistics more frequently than in his earlier career. Thus he discussed the relationship between Hittite and the other Indo-European languages in a 227-page study (Pedersen 1938), investigated the relationship between Baltic and Slavic (Pedersen 1943c), followed by a similar work concerning Tocharian (1941), Lycian and Hittite (1945a), and the question of the common Indo-European and pre-Indo-European stops (1951a), to mention just a few. However, there is only one small paper in which the question of General Linguistics is broached, in a contribution to the festschrift in honour of the first decipherer of Hittite, the Czech scholar Bedřich Hrozny (1879-1952), whose first efforts Pedersen had not found to be particularly praiseworthy in his 1916 survey (see pages 25-26 of the present volume). Here Pedersen (1949c) asks the question of whether a general linguistics on empirical grounds is at all possible. His response, given more than thirty years after the publication of the Cours de linguistique générale (Lausanne & Paris: Payot, 1916; 4th ed., 1949), is very curious indeed, since for Pedersen such a general linguistics would have to consist of two parts, a phonology and a morphology — with syntax being, as in August Schleicher's argument 100 years earlier, a

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6 In 1954, a Russian translation was published (Pedersen 1954).
more doubtful domain of linguistic (in the sense of 'sprachwissenschaftlich') analysis. In Pedersen's view, General Linguistics is thus not what Hermann Paul would have called a 'Prinzipienwissenschaft', a field mapping out the methodological and epistemological framework of scientific research, and of course nothing comparable to what we have learned from Saussure's teachings.

Pedersen's biographers agree that he was and remained a comparative-historical philologist, though, as his scholarly work suggests, a widely read and thoroughly trained one in almost every branch of the Indo-European language family. But they also agree, with regret, that he showed little, if any, interest in general linguistic theory. This personal bias is particularly obvious in his historiographic work, which will be the subject of the remainder of this introduction. But, before continuing, let us conclude the narrative concerning Pedersen's biography. According to Hjelmslev, Pedersen always enjoyed a 'santé de fer', in fact to the extent that he was somewhat insensitive to the physical frailty of others, including Karl Verner, on whom he wrote an obituary (Pedersen 1897b). Following his travels to Germany, Greece, and Ireland during the 1890s, he rarely left Denmark for the remainder of his life, which was devoted to teaching and research. He died in 1953 at the age of eighty-six.

No doubt Pedersen is best known among modern students of language, especially in North America, for his *Linguistic Science in the 19th Century*, which first appeared in Danish in 1924 (and in Swedish translation in the same year), and which was translated into English by the professor of Germanic languages at Northwestern University, John Webster Spargo (1896-1956) and published in 1931. It was reissued in 1962 under the pretentious and misleading title "The Discovery of Language", and has seen a number of subsequent reprintings, having become something like a textbook to be acquired by every student in the field.7

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7 Pedersen also spent a few months in 1894 at the University of Moscow, but he was not particularly impressed by the teachings of Filipp Fedorovič Fortunatov (1848-1914), the head of the 'Moscow School'.

8 For reviews of the 1962 reprint, see Robert Austerlitz in *Word* 19.126 to 128 (1963), and Neville E. Collinge in *Foundations of Language* 1.
Regarding this book, Alf Sommerfelt (1966:285) noted the following:

C'est en réalité une histoire des études comparatives et historiques, non seulement des langues indo-européennes, mais aussi d'autres grandes familles. La linguistique générale, [...] n'y entre pas. Il est significatif que, par exemple, Humboldt ou Hermann Paul ne sont mentionnées qu'à l'occasion de problèmes historiques. Le Versuch einer Theorie phonetischer Alternationen [Strassburg: K. J. Trübner] de [Jan] Baudouin de Courtenay [(1845-1929)], paru en 1895, n'y figure pas.

This observation is very true indeed, and is a clear reflection of Pedersen's post-neogrammian bias. To him, linguistics is an historical discipline, and comparative grammar is the only true science of language. Wilhelm von Humboldt (1767-1835) is mentioned only twice in passing, in connection with Basque (Pedersen 1924a:124) and with having proved, in his famous Über die Kawi-Sprache (published posthumously in 1836-39), the kinship between Indonesian and Polynesian (p.130). Other scholars associated with the Humboldtian tradition in linguistics are at best mentioned in connection with historical work, e.g., Finck in conjunction with the study of Gypsy dialects (p.17), and, at worst, totally ignored, even though they might have produced significant contributions to linguistics that ought to have been referred to in Pedersen's book; compare, for instance, Heymann Steinthal's (1823-99) Die Mande-Neger-Sprachen psychologisch und phonetisch betrachtet (Berlin: F. Dümmler, 1867), dealing with African languages, or Georg von der Gabelentz's (1840-93) Chinesische Grammatik of 1881 (Leipzig: T.0. Weigel), which was reissued as late as 1953 (Berlin: Deutscher Verlag der Wissenschaften), to mention only two distinguished scholars. Of course, their work in general linguistics is not referred to either, but this neglect does not only pertain to the post-Humboldtian generalists, but also to 'main-streamers' of the period, in particular Hermann Paul's (1846-1921) Prinzipien der Sprachgeschichte (Halle/S.: M. Niemeyer, 1880), the acknowledged 'bible of the junggrammatiker school' (Malmberg), which had its fifth edition in 1920, four years before the appearance of Pedersen's Sprogvidenskaben i det nittende aarhundrede! Indeed, the


list of omissions in the area of general theory and methodology of linguistic research could be extended almost ad infinitum. But this should not prevent us from acknowledging the book's merits,\(^{10}\) which are great indeed, considering the fact that the author has attempted a survey of the study of language in the 19th and early 20th century, which exceeds the confines of earlier accounts, such as Theodor Benfey's (1809-1881) 837-page *Geschichte der Sprachwissenschaft* of 1869 or Berthold Delbrück's (1842-1922) *Einleitung in das Sprachstudium* of 1880 (6th rev. ed., Leipzig: Breitkopf & Härtel, 1919),\(^{11}\) both very influential works which, curiously enough, Pedersen never mentioned in his own historical surveys, neither the one of 1899, the one published in the present volume (Pedersen 1916a), nor his final synthesis of 1924.

Thus, apart from the sections devoted to the study of Indo-European languages, Pedersen's 1924 book contains a 40-page chapter on "The Study of non-Indo-European Families of Languages" (Pedersen 1931:99-140) as well as one devoted to "Inscriptions and Archaeological Discoveries: The study of the history of writing" (141-239), accounts not found in the books by Benfey, Delbrück or Vilhelm Thomsen, with whose *Sprogvidenskabens Historie* (Copenhagen: Gad, 1902) Pedersen certainly intended to rival, his disclaimer in the 1916 sketch (Pedersen 1916a:10, note) notwithstanding.

But Pedersen's 1924 study was by no means his first historical account of linguistics in the 19th century; as a matter of fact, in 1899, when he was 32 and a "Privatdocent i slaviske Sprog ved Københavns Universitet" (as the title page indicates), Pedersen published the first such attempt. His 64-page "Sprogvidenskaben" constitutes what French-
men may call a vulgarisation (with no pejorative ring about the term) of the establishment of comparative-historical grammar, by Bopp and Rask (Grimm is passed over in silence!), at the beginning of the past century. For 19th-century scholars (and indeed for many scholars of today) linguistics as a science commenced with the work of these linguists (cf. Pedersen 1899:10-12). As many historians before and after him, Pedersen begins the narrative by relating the discovery of Sanskrit and the other Aryan languages in the second half of the 18th century by western scholars and gives a description of the various members of this Indo-Iranian branch of Indo-European (12-16). Next, he treats Armenian (16-17) and Albanian (17-21), followed by a survey of the Slavic group of languages (21-27), with the Baltic group being treated as a separate, though more closely affiliated branch (27-29). Comparatively much space is devoted to Greek, which for a variety of reasons, including its development of the first full-fledged alphabetical writing system, had played an important role in comparative historical linguistics (29-36). There are also treatments of the following branches of the Indo-European language family: Italic (36-40), Celtic (40-47), and Germanic (47-57), the latter receiving, in view of the readership, by far the most extensive analysis. It is also quite understandable that Danish scholars from Rask to Thomsen and Jespersen, including Verner, Niels Ludvig Westergaard (1815-78), a Sanskrit scholar, and Ludvig Wimmer (1839-1920), a specialist on Germanic languages and a runologist of distinction, figure prominently in Pedersen's account. The concluding passages of his 1899 sketch make brief mention of various non-Indo-European languages and language groups: Basque, Etruscan, Finno-Ugric, Turkish (on which he expatiates more fully, 58-62), and various Caucasian languages, all unrelated to the preceding ones. The concluding paragraph (63-64) is devoted to questions of method in historical-comparative research, a subject to which Pedersen allots more ample space in his subsequent works on the history of linguistics (as well as in individual papers throughout his entire career, where minute analyses of phonological or morphological phenomena are interdispersed with methodological observations).
In his *Sprogvidenskaben* of 1899, we may discover *in nuce* Pedersen's two later studies in the history of linguistics, not only with regard to the subjects covered, but also with respect to Pedersen's foci of interest, personal preferences, and his critical and sober stand on matters of method in scientific research in historical linguistics. I believe I need not dwell on this last subject, as it will become abundantly clear in the 1916 sketch, the English translation of which is presented in this volume.

Hjelmslev (1973:36-37) noted the following about Pedersen's 1916 *Et blik på sprogvidenskabens historie:*

> En novembre 1916, il écrivit, dans le programme de l'université [de Copenhague], une étude [...] qui reflète d'une façon très caractéristique sa personnalité d'auteur. À la fin de cet ouvrage, Holger Pedersen formulait un programme de travail qui inspira la jeune génération des linguistes et orienta sa propre conception dans les années ultérieures: "Il faut établir un système d'ensemble des changements phonétiques, un relevé du grand nombre possible de changements phonétiques constatés, en y adjoignant un essai d'explication phonétique." [Cf. page 83 in the present volume.]

Hjelmslev does not indicate in what sense his historical survey reflects Pedersen's personality; nor does he seem to understand the nature of Pedersen's program. Hjelmslev was most probably thinking of his collaboration with other linguists of the younger generation during the 1930s and 1940s, notably with Hans-Jørgen Ædall (1907-1957), on phonological questions which eventually led to his own glossematic theory of language. In my view, Pedersen and Hjelmslev are worlds apart; while Pedersen remained a follower of the Junggrammatiker in matters of his research interests, his methodology, and indeed his philosophy of science, Hjelmslev showed an early interest in Saussurean general linguistic theory, in which historical linguistics is but one domain and not the centre of attention. Pedersen, as we have noted earlier, has no appreciation for a linguistic approach which is deductive rather than inductive, and he doubts the feasibility and indeed the value of a general linguistics.

In other words, if Hjelmslev's observation is to stand, namely, that Pedersen's 1916 essay is characteristic of his personality as an
author, we must discover it in other traits of his scholarship. In his first note on page 9 of his Glance at the History of Linguistics (page 85 in the present volume), Pedersen describes his study in the following terms, distinguishing it from Vilhelm Thomsen's much broader survey of 1902 (cf. bibliography, p. 95):

I have centered my attention solely upon one specific aspect of the history of linguistics: the development of notions concerning linguistic kinship and the history of [historical] phonology, and I have placed special emphasis on underlining the causality in this development.

If we take a look at the table of contents (p.vii), we obtain the impression that Pedersen's account follows the traditional manner of presenting the history of comparative-historical linguistics from its beginnings in the work of Leibniz, Hiob Ludolf, and others in the 18th century to the success story of the Junggrammatiker in the last quarter of the 19th century, with whose work he associates himself. However, upon closer scrutiny, we will notice a number of 'digressions' (identified as such by Pedersen himself and set off optically by smaller print in the text), which appear to suggest particular areas of interest and concern on Pedersen's part: The first such digression (1916a:11-13; this volume: 6-8) pertains to a comparison between Classical Greek and Sanskrit morphology and the relative opacity of the former; the second excursus (1916a:16-19 = this volume: 11-15), sketching the lack of a true historical perspective (and a lack of scientific method) in the linguistic debates from the ancient times to the early 19th century is followed by yet another, still much more extensive digression (1916a:19-34 = this volume: 15-27), in which Pedersen delves into a discussion of the various languages surrounding ancient Greece and inhabiting ancient Italy, the use of the alphabet in these early periods, also by those for whose languages it was not designed, with critical review of the recent literature on these subjects, etc. As a matter of fact, we might find in at least a number of these 'digressions' Pedersen's research interests and methodological concerns clearly reflected, which may be summarized as circumspection in matters of historical-cultural evidence and soberness in phonological-
morphological analysis. At the same time, we may note another characteristic which accompanies the reader throughout the book: caution, i.e., avoidance of hasty conclusions (cf. his criticism of Kretschmer in this regard, p.21) or what he terms carelessness with data (cf. p. 23), and skepticism towards newly proposed hypotheses (e.g., the question as to whether or not Hittite was an Indo-European language; cf. pp.24-26).

I will leave it to the reader to identify other traits in Pedersen's argument, though perhaps one other feature may be mentioned here because it still appears to be an issue of debate along national, if not nationalistic, lines: The question concerning the discovery — and the true discoverer — of the Germanic Consonant Shift (and the High German Consonant Shift), which since Max Müller's (1823-1900) Lectures on the Science of Language (London 1861ff.) has come to be referred to as 'Grimm's Law'.\(^{12}\) In his 1916 sketch, Pedersen makes a forceful argument in favour of Rask's priority (52-58 = this volume: 51-59), which culminates in the claim that the 'law' should be identified with Rask's name, not Grimm's (p.59). Pedersen did not reiterate this claim in his 1924 synthesis, in which he did in fact incorporate most of his 1916 material — compare chapter 7 of his Linguistic Science in the 19th Century, "The Methods of Comparative Linguistics" (240-310) with the present text, but he reverted to his strong views in favour of his compatriot in his introduction to Hjelmslev's edition of Rask's Selected Works (cf. Pedersen 1932:xlvii). Again, I leave it to the informed reader to decide to what extent Pedersen's claim is justified. To be sure, Grimm not only profited from Rask's insights, but also duly acknowledged the lead he had received from Rask (cf. p. 44 in the present volume), and he might well have objected to Max Müller's appellation had he seen it. (Grimm died in 1863, three years before the German translation of Müller's Lectures began appearing.) The fact that Grimm noted on page 590 of the second revised edition of

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\(^{12}\) Cf. the discussion as presented in Paul Diderichsen's (1905-1964) masterly study, Rasmus Rask und die grammatische Tradition, transl. into German by Monika Wesemann (Munich: Fink, 1976), esp. pp.133ff.
his *Deutsche Grammatik* (1822) that the 'lautverschiebung' does not take place in all instances (listing a number of exceptions) is taken by Pedersen (p.58 in the present volume) as a sign of the inferiority of his observations in comparison with Rask's made several years earlier:

This explicit pronouncement [of Grimm, Pedersen argues,] concerning the unpredictability of the development is significantly worse than Rask's tacit assumption of certain exceptions; silence at least does not exclude the notion of specific conditioning, even if this idea hardly occupied Rask to any particular extent.

We may wonder to what extent 'presentism' was involvement in Pedersen's judgement, especially when we realize how many decades it took to resolve the question of the so-called exceptions to 'Grimm's Law', and that Verner's discovery of 1875 (cf. this volume, pp.70-71) constituted the final, and most important, clef de voûte in the entire edifice. (Verner's findings, we may recall, had been preceded by Grassmann's discovery in 1863, the year of Grimm's death, concerning the dissimilation of aspirates in consecutive syllables.)

Perhaps still one further point in Pedersen's account deserves comment. We have noted earlier that Pedersen maintained, throughout his entire career, a view of linguistic science which had been developed during the last decades of the 19th century, by the Neogrammarians as well as by their erstwhile opponents such as Johannes Schmidt, Adalbert Bezzenberger, Hermann Collitz, and others. His historical treatment of the development of linguistic science in the 19th century attests to this. However, he was more generous than his teachers at Leipzig and their historical mouthpiece Berthold Delbrück in acknowledging, in line with what Schmidt, Collitz, and others had been trying to show during the 1880s, the importance of August Schleicher's (1821-68) work in this development. Pedersen does criticize Schleicher for inconsistencies and for a wrong turn here and there in his phonological analysis, but he also stresses (p.64) that it was Schleicher who brought system into the study of Indo-European phonology and who used clear rules in the reconstruction of proto-forms. Citing from the second edition of the first volume of Schleicher's *Compendium*
(1866), in which he sets out the reasons for his procedure of placing the reconstructed form side by side with the available forms in the historically attested languages, Pedersen gives his full approval of Schleicher's approach, concluding with the following statement (p.64):

This entire method and likewise most of the factual material contained in Schleicher's phonology, which makes up the first volume of his Compendium, impress us as being extremely modern. This assessment deserves emphasis since the neogrammarians pro-domo accounts beginning with Delbrück's Einleitung of 1880 have impressed on subsequent generations of linguists and historians of linguistics that Schleicher was but the last of the representatives of the school of Bopp, and that the mid-1870s witnessed a revolution in the study of language.\(^\text{13}\)

As indicated in the Preface above (p. vi), I believe that the present sketch of the development of the historical study of phonology from the earliest tâtonnements through the establishment of linguistics as an independent field of scientific study to the first decade of this century will receive more attention during the 1980s than it would have received some ten years earlier. We appear to be witnessing a pendulum swing in the direction of a dedicated data-orientation and a move toward history in general, in matters concerning both the evolution of the discipline and the actual study of language. One may wonder, however, whether Antoine Meillet's (1866-1936) view, expressed in his review of the Danish original, will be shared by modern linguists reading the English translation: "D'un bout à l'autre, l'exposé de M. Pedersen, très clair et suggestif, mérite d'être lu par tous les linguistes."\(^\text{14}\)


\(^{14}\) See Revue critique d'Histoire et de Littérature (Paris, 21 April 1917), p. 252. - Cf. also the review by Maurice Cahen (1884-1926) in Bulletin de la Société de Linguistique de Paris 20.157-59 (1916), in which the reviewer rejects the suggestion that P. was Germanophobic.
Sigla employed in this list:


NTF = Nordisk Tidsskrift for Filologi. Copenhagen 1861-1922. [Note that references are to the journal's 3rd series, 1891-1911.]


1893d. "Lat. servus und servare". Ibid., 298-302. [The preceding paper and this one are dated 'Corfù d. 28. Mai 1893' and '29. Mai', respectively.]

* Edited & revised by Konrad Koerner. — Note that the present listing does not include reviews, newspaper articles, or contributions to collective reference works. For a fuller bibliography, see the "Bibliographie des publications", compiled by Hans Hendriksen, in Mélanges linguistiques offerts à M. Holger Pedersen à l'occasion de son soixante-dixième anniversaire 7 avril 1937 (Aarhus: Universitetsforlaget, 1937), pp.ix-xxvii. Post-1937 publications have been gleaned from the UNESCO-sponsored Linguistic Bibliography (Utrecht & Antwerp: Spectrum, 1948ff.) and other bibliographical research tools.
1895a. Albanische Texte mit Glossar. (= Abhandlungen der Königlichen Sächsischen Akademie der Wissenschaften, 15:3.) Leipzig: S. Hirzel, 207 pp. [For a German translation of these texts, see 1898b (infra).]
1895b. "Das indogermanische s im Slavischen". IF 5.33-87.
1897a. Aspirationen i Irsk: En sproghistorisk Undersøgelse. Første Del med Tillæg: Theser til den indoeuropæiske Sproghistorie. Doktor-Abhandlung, Univ. of Copenhagen. [Printed, Leipzig: Spirgatis, 1897], vi, 200 pp. [For second part, in German, see 1899b (infra).]
1897c. "Das albanische neutrum". KZ 34.283-91.
1899b. "Die aspiration im Irischen. Zweiter theil". KZ 35.315-444. [First part (= Pedersen's doctoral dissertation) is 1897a (supra). - Dedicated to Heinrich Zimmer (1851-1910).]
1900b. "Wie viel laute gab es im Indogermanischen?". KZ 36.70-110.
1903/04c. "Den bøhmiske Udtale". Ibid., p.48. [Supplement to 1902/03 (supra).]

1904b. "Zu den lykischen inschriften". KZ 37.189-207.
1904e. "Npast mē hayerēn lezowi patmowt'ean". (= Azayin matenadaran, 47.) Vienna: Mkhitarian Dbaran, VI, 87 pp. [= Armenian version of 1905b (infra).]

1905b. "Zur armenischen Sprachgeschichte". KZ 38.194-240. [For Armenian version, see 1904e (supra).]
1907c. "Hayeren ew drac'i lezunere". (= Azgayin Matenadaran, 52.) Vienna: Mkhitarian Dbaran, 9, 264 pp. [= Revised Armenian transl. of 1906b (supra) by Thomas Ketikean.]


1924a. Sprogvidenskaben u det nittende århundrede: Metoder og resultater. (= Det Nittende århundrede skildret af nordiske Videnskabsmand, 15.) Copenhagen: Gyldendalske Boghandel (Nordisk Forlag, 311 pp.)


1927d. "Den nyere sprogvidenskab og dens nærmeste fremtidsmål". *Tilskueren* 1927.365-73. [For information on this monthly, see item 1906c (supra).]


1931. *Linguistic Science in the Nineteenth Century*. Transl. from the Danish, with revisions (authorized by the author) and indices, by John Webster Spargo [(1896-1956)]. Cambridge, Mass.: Harvard Univ. Press, vi, 360 pp. [Transl. of 1924a (supra); reprinted in 1962 (see infra).]


1952b. "Das lykische Wort trqqas". IF 61.81-85.


Holger Pedersen's Nachlaß has been deposited at the Royal Library of Copenhagen under the catalogue number Ny. kgl. Saml. 2718 fol., and is comprised of the following packets. (Information on the detailed contents of the respective packets can be found in the Registrant over Holger Pedersens efterladte papirer under that cat. no.)

A. General Linguistics (1 packet, 2 file boxes)
B. Indo-European (1 packet)
C. Albanian (3 packets)
D. Armenian
E. Greek
F. Italic
G. Romance
H. Balto-Slavic
I. Lithuanian
K. Slavic (2 packets)
L. Celtic (7 packets)
M. Germanic
N. Danish Orthography
O. Tocharian (3 packets)
P. Hittite (10 packets)
Q. Lycian (1 packet)
R. Other Indo-European Languages (1 packet)
S. Non-Indo-European Languages (1 packet)
T. Biographies of Scholars
U. Articles for Salmonsens Lexicon
V. Biographical & Miscellaneous Items (1 packet)
X. Letters to Holger Pedersen (4 packets)
Y. Letters from Holger Pedersen (1 packet)
Z. Albanian Letters to Holger Pedersen
E. Armenian letters to Holger Pedersen
Ø. Louis Hjelmslev's Papers concerning Holger Pedersen's Estate (1 packet)

XXXI
ET BLIK PÅ
SPROGVIDENSKABENS
HISTORIE

MED SÆRLIGT HENSYN TIL

DET HISTORISKE STUDIUM
AV SPROGETS LYD

AV

HOLGER PEDERSEN

KØBENHAVN
TRYKT I UNIVERSITETSBOGTRYKKERIET (J. H. SCHULTZ A/S)
1916

[XXXII]
A GLANCE AT THE HISTORY OF LINGUISTICS
WITH PARTICULAR REGARD
TO THE HISTORICAL STUDY OF PHONOLOGY

1. INTRODUCTION

The significance of the study of phonology for linguistic science seems never to have been questioned outside the circle of linguists themselves.

At least everyone wishing to criticize the activities of linguists (or presumed linguists) has unfailingly chosen precisely the treatment of phonology as the target for his attacks. Merely the subject matter of this sarcasm seems gradually to have changed.

Initially, accusations were aimed at arbitrariness. Voltaire's pronouncement that etymology is a science in which vowels count for nothing and consonants only for very little made a tremendous hit, primarily, of course, due to its witty formulation, but also because it partly struck home, even though what it struck were for the most part things which no historian could possibly include in any stage in the development of the science of etymology. Comparable to Voltaire's statement is Blicher's familiar Lushøj-etymology. For linguists, however, the latter no doubt lost its satirical impact by being placed in the mouth of Counsellor Urold "the antiquarian" as he is holding an urn unearthed from the barrow:

"Ladies and Gentlemen!" he began. "You are viewing the earthly remains of the Danish King Hadding and his Queen Hartgrepe ... Ladies and Gentlemen! This claim is not ut-
terly unfounded; I shall prove it: The peasants call this barrow Luushøj. This designation derives not from a certain common insect, but from the archaic word Lius, the same as Lys, which was also pronounced Lis - The English, descendents of the Jutes, were not yet able to pronounce the vowel y [8] - the consonants L and R were easily confused, and thus we obtain Rishøj. This is an abbreviated pronunciation of Ripshøj; Ripshøj likewise of Gripshøj. This derives from Grepshøj; Grepshøj from Grepeshøj and Grepeshøj in turn from Hartgrepeshøj. Now it is self-evident: Hartgrepeshøj — Grepeshøj — Grepeshøj — Gripshøj — Rishøj — Lishøj — Lyshøj — Liushøj — Luushøj — there you have it!" [Blicher 1836: V, 104].

The desire to play this tune, the desire to accuse linguists of making arbitrary assumptions concerning sound changes probably still exists; but additional melodies are also beginning to be heard. In 1867 G. Stephens struggles desperately for the right to assume every possible phonetic form in his runic interpretations and speaks out forcefully against the linguists' rigid demands concerning phonetics and sound laws:

"The book-creator mobilizes a whole battery of 'phonetic notation'-symbols for the dialect with which he is dealing. He possesses half a dozen A's, as many E's and I's etc., all the way through the vowels, diphthongs and consonants. The more pedantic and tedious he is, the more he boasts. His A¹, A², A³, A⁴, A⁵, A⁶ form an impressive parade, though there are surely few who could learn to pronounce accordingly ... And equipped with these he proceeds to establish 'nationality'. The village with pronunciation A¹ is Danish, A² Frisian, A³ Saxon, A⁴ English, A⁵ Swedish, A⁶ High German ... Indeed, nowadays there are many who need only to sniff at a vowel in order to be able to inform you quite precisely of everything concerning populations and
INTRODUCTION

dialects and nations thousands of years ago" (Aarbøger for nordisk Oldkyndighed og Historie 1867, 178-79).

This is another key; here the layman does not complain of arbitrariness but actually demands that there be room for arbitrariness.

In Germany Laut-schieber is the nick name for linguists who concern themselves with linguistic comparison. This designation stems, of course, from the serious term Lautverschiebung, employed by Grimm to describe the sound law first discovered by Rask, [9] the first important regularity in phonetic development recognized by linguists. This, however, does not mean that the word Lautschieber, from the point of view of an outsider, need refer to an exaggerated concern for sound laws; it can also be taken in the spirit of Voltaire. Or perhaps it is fortunate precisely because it is subject to both interpretations, because everyone can understand it as he chooses.

With regard to the study of phonology, it is thus not entirely without justification that linguists hear a call of "Hic Rhodus, hic salta!". If one is to go beyond the observation of immediately given linguistic forms and engage in etymology or in historical and comparative linguistics, solid concepts concerning phonetic development are mandatory prerequisites for sure-footed wandering. To be sure, this is not the sole requirement; in etymological, historical and comparative linguistic studies, as in any other linguistic endeavor, all aspects of language must be kept in view. But the study of phonology is most easily dealt with. It will thus be of no little interest to glance at the development of linguistics to date with particular regard to the role which the study of phonology has played at various stages, to attempt an assessment of its significance today and to suggest some of the tasks which must be
tackled in the immediate future if we are to proceed beyond our present state of research.¹ [10]
2. Pre-19th-Century Etymology

Etymological speculations are much older than historical and comparative linguistics. Thus, it is in connection with these speculations that the notion of sound change first comes to play a role. This means that the notion of sound change comes into being in the most unfortunate way imaginable. Because etymology without the reins of historical and comparative linguistics can develop into nothing but child's play. Perhaps it can refrain from developing, which is preferable. But if it develops, it will unavoidably wander astray. To be sure, there are a number of words in every language which are etymologically transparent, otherwise no one would ever come upon the idea of making etymologies; and within etymologically transparent linguistic data phonological alternations will also invariably be present. But as to how these alternations arose and concerning how language develops at all, such untrained reasoning obtains no real notion through observation of a single language in isolation; with naive assurance the most difficult tasks are undertaken, the most impossible results obtained, and all with procedures which have absolutely nothing to do with linguistic reality.

There is hardly a nation which has refrained from this type of etymology. Not even the [Ancient] Indians escaped it. But they were in possession of a counterbalance. There is always a counterbalance to ravings where a truly exact science exists; and the unsurpassed clarity of the ancient Indic language enabled the Indians to carry out an analysis of their linguistic forms which was so penetrating that they became the teachers of modern European linguists. They had founded a true science; and this science, because of its practical aims, remained in constant contact with mother earth. A grammarian like Pāṇini, whose goal was to provide
a system of formalized rules into which the entire linguistic complexity of the language could be incorporated and out of which this same abundance of forms could be unfolded, had no room for vague etymological fantasies.

Worse off were the Greeks, whose grammar was originally an outgrowth of philosophy, that is to say of that abstract desire for knowledge and inquiry which is indifferent to practical application. They were also at a disadvantage because their language was less transparent and clear, which meant that grammar, in the form it had to assume among the Greeks, could never, not even when coupled with an up and coming philology, acquire the same practical nature as that of the Indians.

For we cannot for a single moment doubt the fact that Greek is far less clear in its structure than Sanskrit. To be sure, the most recent developments in linguistics have demonstrated that Greek, with respect to originality, often surpasses Sanskrit. But the sound changes which took place in Greek (primarily the loss of intervocalic \( w, j \) and \( s \)) tend to greatly obscure the original nature of its morphology, whereas the Indic sound changes have not had this effect. The following pair of inflectional patterns provides an example:

\[
\begin{align*}
\text{Skr. nom. } & \text{á}zug-s \ 'horse' & \text{Gk. nom. } & \text{έ}n\text{nea-s} \\
\text{voc. } & \text{á}zug & \text{voc. } & \text{έ}n\text{ea} \\
\text{gen. } & \text{á}zug-sy\text{a} & \text{gen. } & \text{έ}n\text{neou}
\end{align*}
\]

Both of these inflectional patterns go back to one and the same Indo-European paradigm, which can be written as follows (the symbol \( k' \) represents a palatalized \( k \)):

\[
\begin{align*}
\text{nom. } & \text{*ek'wo-s} \\
\text{voc. } & \text{*ek'we} \\
\text{gen. } & \text{*ek'wo-sjo}
\end{align*}
\]

To be quite precise, however, we should rather write these as:
nom. *äk'wa-s
voc. *äk'wa
gen. *äk'wa-sjå

For it is evident from the phonological development in the individual Indo-European languages that Indo-European e and o must have been very open sounds. For e one should not have in mind a sound like the Danish e in sveske, but rather a sound like æ in præst, like the English a in man or like the Copenhagen a in gas. And for Indo-European o one should not have in mind a sound like the Danish o in ost, but rather a sound at least as open as that in godt.

From the point of view of Indo-European, the above inflectional forms are to be analyzed as follows: The nominative ended in -s, the genitive in -sjo, the vocative in nothing; the stem ended in -o, which alternated with -e. In general these vowels frequently alternated with each other: Latin tegō, toga; Greek ΠΈΜΠΩ, ΠΟΜΠΙΩ.

It is now quite clear that the correct analysis can be arrived at as easily through consideration of the existing Indic forms as from the Indo-European paradigm set up on the basis of a careful comparison of all existing Indo-European languages. To be sure, a sound change affecting the inflectional pattern has taken place in Indic: the two vowel colorings e and o (ä and å) have merged. But this has increased rather than decreased the transparency of the forms. In the inflectional pattern both vowels have the same etymological origin, and consequently the merger merely exempted the Indians from making an abstraktion of which they perhaps were capable, but which nevertheless would have complicated their analysis.

On the other hand, it would be virtually impossible to arrive at the correct analysis through direct observation of the Greek inflectional pattern. The fate of the s-sound [13] and its further consequences would have made it almost impossible to analyze the genitive form. Originally intervocalic s had become h, just as s had become h initially preceding a vowel: ἡπτά, cf. Latin septem, Sanskrit saptá etc. Thus a genitive ending -hj-o had to emerge. But -hj- developed further into a long j, which in the Greek orthography had to appear as an i, which forms a diphthong with the preceding vowel:

And neither was this form, which is preserved in Homer, the final stage in the development. In unstressed (proclitic) pronominal forms (in the article), the long j was shortened and finally lost completely, resulting in a genitive ending
which gradually made its way from the proclitic position into independently stressed words. We have no direct evidence of this new ending, and it can hardly have survived for any length of time. The two short (and, according to Ancient Greek pronunciation, closed o-sounds merged into one long, likewise closed o-sound for which the Greek orthography had no special symbol. In the majority of the Greek dialects, for example in Ionic-Attic, ω could not be employed since this symbol designated an open o-sound. Thus o was used, and later ou. The pronunciation gradually moved from closed o to u, evidenced for example by the Romans' representation of Greek words. The final stage in the development thus became:

\[ \text{o} \rightarrow \text{u} \]

The correct etymological analysis of the resulting inflectional pattern was thus quite obscured; however, the analysis \( \text{ιππ-ος, ιππ-ε, ιππου} \) and the interpretation of o-ς, e, -u as inflectional endings had to appear self-evident to the Greeks; but with this the route toward the scientific understanding of the inflectional system, which the Indians were able to attain, was clearly blocked.

This single, but in reality typical, example must serve here as documentation for the suggestion hinted at above, that the difference between Indic and Greek linguistics is in part dependent upon the different natures of the two languages.

At any rate, in their observations on morphology the Greeks achieved nothing which deserves the name scientific, and there was therefore no counterbalance for their uncontrolled etymologizing. And such a counterbalance was not and could not be created, neither by the Romans nor by the Europeans in the Middle Ages or in the first centuries thereafter. Vilhelm Thomsen, in his presentation of the history of linguistics published in the Invitation to Copenhagen University's Annual Celebration on 17 April 1902, [14] provides examples of the art of etymology among the Greeks, Romans and more recent Europeans: pp. 9-10, 22-25, 33. Here we also find examples of the treatment of phonology in these periods. I will not repeat any of this; it will suffice to say that the arbitrariness and absurdities far outshadow Blicher's
Lushöj-etymology. And even though the worst excesses could presumably never have gained credence among more critical minds, still there was so much attached to them that historical and comparative linguistics, when it finally came into being, initially had to bear a heavy burden of inherited misconceptions. Regarding the notion of sound changes, it did not meet with a blank slate; the task it received was not simply to build from the ground upward; first it had to work its way out of a labyrinth of misconceptions.
3. THE BEGINNINGS OF COMPARATIVE LINGUISTICS

When, then, did historical and comparative linguistics begin? How did it originate, and who founded it?

The answer must be: it arose as soon as the external prerequisites for its emergence were present. Many forces contributed toward its formation, and only with great reservation can one individual scholar be called its founder.

It is hardly necessary to stress the fact that the prerequisites for comparative linguistics were totally absent in pagan antiquity. In general there was no interest in the languages of foreign nations beyond that necessitated by practical contacts. The Greeks' view of everyone who was not their fellow countrymen as Бάρβαροι is typical. Furthermore, language was not studied for its own sake, nor with a view to general literary or aesthetic considerations; it was studied with a view to the interpretation of the national literary works composed in it (as with the Greeks) or for the purpose of preserving the nation's religious writings from any [15] form of corruption (as with the Indians). The latter point of view could even less than the former give rise to an interest in foreign languages. As a rare exception one nation could look up to another as its teacher and study its language and literature (as the Romans with respect to the Greeks), or a dialect long extinct as a national language could continue to be cultivated as a written language while a much younger descendant of it could be heard on the tongues of the nation's inhabitants (as was the case in India), or several closely related dialects could be employed along side of each other as written languages (as in Greece). But this was not enough to produce a true comparative linguistics. There were too few elements to compare for the comparison to be truly profitable, and comparison was simply not undertaken. Notions of linguistic
relationships were thus quite distorted. It was quite natural to see the most original language either in one's own spoken idiom or in the tribe's, nation's or period's language whose culture was recognized as superior. Accordingly the Romans (Varro) viewed Latin as a corrupt variant of Greek, particularly of the Aeolian dialect; and they cannot be greatly condemned for this misconception. They lacked the broader perspective which could have shown them that the similarities they had observed between these two languages extended over a far greater geographic area.

This narrow linguistic perspective was, however, not entirely a result of the barbaric reasoning; the external reasons surely carry equal weight. From the time of Herodotus the active minds of the Greeks were also interested in the traditions and history of the barbarians, and they would surely have had nothing against making observations concerning the language of the barbarians as well. A slight indication of this is found in Plato's remarks on the similarity between Greek and Phrygian: δρα τούτο τό άδειον τό πώρον μή τι βαρβαρικάν ἤ. τούτο γάρ οὔτε βάδιον προσάραξα έστιν έλληνική φωνή, φανεροί τ’ είσθεν οὔτως αόρτο καλόντες Φρύγες σμικρόν τι παρακλινόντες. καί τό γε θόμο καὶ τάς κύνας καὶ ἄλλα πολλά (Kratylos 410 A) "observe whether this word πώρ is of barbaric origin or not; because on the one hand it is not [16] easy to connect it with Greek (i.e., with another Greek word), and on the other it is well known that the Phrygians term it (fire) thus, with a slight deviation; and (this they do also with) water (τό άδειον) and the dogs (τάς κύνας) and many other things". And in the same dialogue 425 E Plato says: είσθε δὲ ἡμῶν ἄρχαιότεροι βάρβαροι "the barbarians are older (more original) than we are".

It is quite clear that we are dealing here with true linguistic observations, whereas in remarks concerning the
relationship between two barbarian languages (for example in the words of Eudoksos concerning the Armenians: καὶ τῇ φωνῇ τολλα φρυγξεσσαν "also in their language they show many Phrygian features") we need not see anything more than renderings of native opinions from one quarter or another (and possibly opinions which are as far-fetched as the insistence on a specific relationship between Aeolic and Latin or the indestructable cock-and-bull story about Jutlandic and English, which St. St. Blicher also hints at in the passage mentioned above, or the fables of the Irish concerning their origin in Spain, or other similar hogwash).

And the observations made by Plato were obviously quite correct; here he has obviously touched on material which in itself could easily lead to something. Because the Phrygians were actually an Indo-European nation and spoke a language having ancient characteristics (we know it from inscriptions of which we at least understand with certainty a concluding formulaic curse, also known in Greek form, against those who would offend the grave). The similarity between this language and Greek must have been quite evident, more evident than the similarity between Greek and Latin. And the Greek words mentioned by Plato (τὸρ, ὅνωρ, κύνον) are precisely ancient Indo-European words which recur in nearly all Indo-European languages. Plato no doubt had his hands on the correct Phrygian cognates.

Plato expresses himself somewhat awkwardly; a professional stringency is lacking (τὸρ means 'fire' and at the same time 'the word ἰπτ' etc.). His line of thinking is also distorted on several points. Plato suspects that τὸρ is a loan, because it cannot be etymologized with aid from Greek; this reasoning is quite incorrect; the true core of a language's vocabulary, the most common words which have been a part of the language for the longest period of time are precisely those which as a rule cannot be etymologized. He finds his suspicion confirmed by the word's presence in Phrygian; apparently he is unable to envision any contacts between two different languages other than borrowing. And when he, with the remark ἐκλείμεν ἀρχαίτερον βάρβαρον, appears to go beyond the question of the individual word and to relate the one language as a whole to the other, it is obvious that for him it is a question of which language is the source of the other.

This notion of an existing contemporary language as the source of other (or all other) languages we know in its most drastic form from the story in Herodotus (II 2) of an experiment reportedly undertaken by the Egyptian King Psammetikhos. The formulation of the problem to be solved is expressed in Herodotus' introductory remarks: Οἶ δὲ ἄλγηστοι, πρὸς πάν Ἡ Ψαμμήττυχος σφένου βασιλέως, ἐνέμεισα οἱ πρῶτοις γενέσθαι τάντων ἄνδρῶν. ἔπελθε δὲ Ψαμμήττυχος βασιλεύσας Ἀθῆναις
"The Egyptians thought, before Psammetikhos became their king, that they themselves were the first of all peoples, but after Psammetikhos came to the throne and set out to discover who had been the first, from that time on they thought that the Phrygians had existed earlier than they, but they themselves (earlier) than (all) the remaining". Next comes the story of how Psammetikhos delivers two newborn infants into the hands of a shepherd with instructions to raise them out with the herds in an uninhabited shed, to bring the goats to them at certain times and to satisfy them with their milk, and to undertake whatever else was necessary in order to care for them, all without ever uttering a single word in their presence. The king wanted to know what the children would say first once they had progressed beyond their meaningless squalls. After the shepherd had raised the children for two years in this way, one day when he entered the shed both children imploringly stretched out their hands toward him and cried ἕκος. At first the shepherd did nothing, but when this word was repeated again and again upon his subsequent visits, he reported it to the king. The king had the children brought before him, and after he too had heard the word himself he asked around in search of the language in which ἕκος meant something; he then learned that in Phrygian it meant 'bread'. And in drawing conclusions from this, the Egyptians admitted that the Phrygians were older than they themselves (τοὺς Φρύγας προστερέουσι εἶναι ἑωτᾶν).

What is interesting about this story is not the foolish and cruel experiment itself. In any event it was cruel with respect to the two children; it was even more cruel according to another version, to be sure rejected by Herodotus, in which the care of the children was not left to a shepherd, but to some women whose tongues the king had had cut out of their mouths. It was likewise foolish in its entire execution, not least according to the more gruesome version; it is very shallow phonetics which believes that the ability to produce sounds is lost with the loss of the tongue; precisely something similar to ἕκος could be produced without the aid of the tongue; the tongueless person would of course be able to articulate both with his lips and with the root of the tongue and [18] could make air whistle through his teeth. The shepherd's oath of silence would obviously have been more scientifically reassuring than the vivisection of the unfortunate women. Likewise the investigation of the meaning of the utterance ἕκος is foolish. Did the king really seriously believe that there were no other languages in the world than those with which his interpreters or perhaps the leaders of his foreign mercenaries were acquainted? But all this is
immaterial in the present connection; it is also close to being irrelevant whether or not the experiment actually ever took place. What is interesting for us is the way of thinking out of which the experiment or the account of it arose; the naive conviction that the oldest and most original language is still in existence. Such a view, of course, leaves no room for any notion of linguistic development; it can only accommodate a notion of linguistic corruption.

The same notion that one contemporary language can be the source of another causes the Roman grammarians to derive Latin from Greek. And this non-historical viewpoint is maintained up into more modern times, giving rise to the numerous attempts to prove that first one, then another language was the most ancient language, the language of Adam. The naivety is really just as great whether one is concerned with one contemporary language or another or with Hebrew. As an anachronism we can still run across this type of thinking in the 19th century among authors whose field lies on the periphery of the mainstream of linguistics. Chakhchakhian, for example, in the preface to his *Dizionario armeno-italiano* (Venezia 1837), still finds occasion to remind us that "l'antichità dell' idioama armeno, che vanta comune la propria coll' origine umana" and to support this pronouncement with the following footnote: "Il P. Luca Ingigi [Ghowkas Inchichean] tanto encomiato dagli europei conoscitori delle molte sue opere, e particolarmente dai giornali letterari francesi, nella sua celebre opera *L'Armenia antica* Cap. 18. dimostra con robusti argomenti, essere la lingua armena la lingua stessa di Noe, e conseguentemente di Adamo altresì". Clearly both Chakhchakhian and his somewhat older compatriot Inchichean are behind the times with these statements. Nevertheless, we can hardly help but take note of the fact that comparative linguistics itself, in its infancy, had to struggle to free itself from the remains of this old unhistorical point of view. This is still evident in the wording of the topic for the prize competition which gave rise to Rask's *Under-sögelse om det gamle Nordiske eller Islandske Sprogs Oprin-delse* [1814, published 1818]:

"To conduct a historically critical investigation, illustrated with appropriate examples, of the source from which the ancient Scandinavian language can with the greatest certainty be derived".

Of course, it is less excusable in more recent times than in the past to be prey to a naive unhistorical view. [Later] scholars [19] had a much longer period to look back upon than scholars of the past, and they should perhaps have been able to see that Latin's transition into the Romance
languages is a development, not a chance corruption. But we must not judge them too harshly. Linguistic development, which is really never at rest, but progresses from decade to decade and from year to year, can only with great difficulty be observed directly, and only with the greatest difficulty can the infinitely small shifts in pronunciation be observed as they occur; and it is thus excusable that for such a long time there was no clear understanding of the notion of linguistic development.

But as long as this understanding was absent, clearly no scientific concept regarding the notion of sound change could be expected.

On the whole, most of the errors in Plato's view of language have been maintained right up to the beginning of the 19th century.

The above Plato passages show that the barbarian languages could actually quite easily have attracted the interest of classical antiquity. However, since they did not, external obstacles must have been present. And neither is it difficult to discover these obstacles. In the first place, it was extremely difficult to learn these foreign languages. There was probably, as a rule, no other way than through personal contact with native speakers of the language, and this contact was not furthered by antiquity's anti-foreign social structure (unless we wish to refer to the bought slaves of foreign nationality). In contrast the study of these languages by reading their literature was excluded as a rule. And this primarily because in most cases a literature was simply non-existent.

Greece was surrounded by a number of related peoples, and most of them knew and used writing; but this does not necessarily mean that they possessed a literature.

We can mention the following neighbors who were Indo-Europeans and thus related to the Greeks: most of the peoples in Italy; the peoples of the Balkan peninsula north of the Greeks; a portion of the inhabitants of Asia Minor.

Where Italy is concerned we can be sure that the Etruscans were not Indo-Europeans. But the following were Indo-Europeans:
first of all the tribes belonging to the two well-known linguistic groups, the Celtic (the Gauls in northern Italy) and the Italic (the Latins, the Umbrians and the numerous Oscan–Sabellian tribes); but other tribes not belonging to these two linguistic groups can [20] also with certainty be labeled as Indo-Europeans (the Lepontians, the Venetians, the Messapians).

The Lepontians have left inscriptions in a North-Etruscan alphabet, some in the area west of Lake Maggiore near Ornavasso; Kretschmer has interpreted the urn inscription

\[ \text{latumarui saxutaipe vinom nasom} \]

as

Latumari Sapsutaæque vinum Naxium,

but with no doubt justifiable reservation with respect to the translation of the final word. If the interpretation is otherwise correct, then it is clear that the language is Indo-European; but it is also clear that it is not Celtic; if the two names connected by \( pe \) (\( = \) Latin \( que \), Greek \( τε \), Sanskrit \( चा \)) are in the dative (which they presumably are), it is more likely that the language is Italic; but here we are most probably faced with a vestige of a distinct language group; and since the Lepontians were most probably a Ligurian tribe, it is likely that these inscriptions can be assumed to be examples of the Ligurians' language.

The numerous Venetian inscriptions belong to a distinct Indo-European language group. The assumption which has been put forth of a connection between Venetian and Messapian can be strongly questioned; if there is no connection, then the Messapians (the bootheel peninsula) represent a third Indo-European relic language group.

To what extent these various tribes inhabiting Italy possessed a literature is, of course, difficult to say. The flourishing Latin literature arouses favorable suppositions, and the existence of an Oscan literature has also been suggested; but even this is uncertain; among the Romans we find no indication of a true Oscan literature. The Atellanian plays are designated as \( Oscti ludi \), but that these popular, perhaps half improvised plays with their fixed comic figures referred to a written literature among the Oscans we have a right to doubt.

The Gauls had no written literature, if we may draw conclusions from what Caesar (\( De bello Gallico \) VI 14) reports concerning their kinsmen in Transalpine Gaul; here higher education was in the hands of the Druids, around whom a flock of youths gathered or were sent by parents and relatives. "Magnum ibi numerum versuum ediscere dicuntur. Itaque annos
nonnulli vicenos in disciplina permanent. Neque fas esse existimant ea litteris mandare, cum in reliquis fere rebus, publicis privatisque rationibus Graecis litteris utantur". 

("It is said that there they learn a large number of verses by heart. For this reason some of them remain in school twenty years. But they do not consider it permissible to commit it into writing, even though they use the Greek letters in most other things, in public and private business matters"). [21] The substance of the Druids' instruction (= the unwritten literature of the Gauls) was many-sided; first and foremost was the doctrine of the soul's immortality and of metempsychosis; but in addition many other things were taught: "Multa praeterea de sideribus atque eorum motu, de mundi ac terrarum magnitudine, de rerum natura, de deorum immortalium vi ac potestate disputant et juventuti tradunt". 

("In addition they lecture in detail about the stars and their movements, about the size of the universe and of the earth, about the order of the universe, about the nature and power of the immortal gods, and they teach this to the youth"). We observe here that to deny a people written literature is not the same as denying it an intellectual life.

We may perhaps also infer from the situation with regard to the alphabet that no written literature existed among the Gauls. The Gauls had no distinct alphabet but employed that of their neighbors. In most ancient times the Gauls in Northern Italy used the North Etruscan alphabet. This alphabet lacked symbols for b, d, g for which the Etruscans had no use; but in Gallic b, d, g were quite common sounds, precisely distinguished from p, t, k, and it is not very likely that the Gauls would have been content with having no way to express such an important phonetic distinction if they had used writing to a greater extent. There are, of course, numerous examples which show that other peoples have been content with unusual gaps in their phonetic representation, and we can also mention examples of the same deficiency as with the Gauls; but at any rate, as far as the two most similar instances are concerned, the Cyprian system of syllabic writing and the younger Nordic runes, we can safely maintain that they contain nothing which could endanger our reasoning with respect to Gallic. The Cyprian system of syllabic writing made do with one set of symbols for all three classes of stops, thus distinguishing neither between b, t, φ nor between δ, τ, θ nor between γ, ∅, χ; this paucity stems no doubt from the fact that their writing system was adopted from a non-Greek people with a simpler phonetic system; to remedy this deficiency in the phonetic representation would, in a system of syllabic writing, have required the addition of a complete series of new symbols; it is not as easy to supplement a syllabic system as it is to supplement an alphabetic system; therefore any comparison with Gallic can be
dispensed with. The case of the younger Nordic runes is quite unique; here the paucity of the alphabet is not due to any foreign influence. The runes first arose out of a basis in the Latin alphabet, and the older runes have different symbols for b, d, g and p, t, k; but the Nordic inhabitants subsequently gave up this distinction of their own accord. This is extremely unusual; changes in pronunciation can perhaps, as Wimmer has contended, have played a certain role. In the earliest times b, d, g were possibly open sounds in all positions (like the Danish g and d in sag, sad; and similarly b was a v pronounced with upper [22] and lower lips); later they were closed sounds in word initial position. This may have played a role in the development of the alphabet; but this does not imply that the new sounds did not lend themselves to representation by means of the old symbols. In this respect there was no obstacle. On the contrary. In the history of the runes there is a remarkable connection between the names of the runes and their significance. The old j-rune, called *jæra, later came to signify a, because the name had become dr in accordance with the general phonetic development. The old a-rune, called *ansaR, gradually came to signify q and o according to the regular phonetic development of the name to *qnsuR and *øes. Finally, after R had merged with r, the old rune for z > R came to signify y because its name was yr. These are quite unique processes which clearly could only take place in an age when little was written down, when the characters and their names were known, but when no truly fixed orthographic tradition existed, so that every time a word was to be written the spelling analysis had to be undertaken more or less from scratch and the spelling conventions had to be reestablished more or less afresh; an age in which the notion of the individual characters and their names was fixed in a way quite different from an overall conception of the written word. From the above illustrations it is quite clear that the transition from open b, d, g to closed b, d, g, which occurred word initially and thus precisely in the names of the runes, could not cause the old characters to seem inadequate as representations for the new sounds. It is more likely that they seemed inadequate as representations for the old sounds which were retained within the word. It might have appeared undesirable to represent ð with the symbol for d and preferable to write ø with the same symbol as p; it might have been felt undesirable to represent the bilabial v with the symbol for b so that preference was given to writing v with the same symbol as j:

\[ \text{HARP} \] urarp = warð 'became'
\[ \text{HIF} \] lifa = liva 'to live'. 
Thus the same symbols came to be used for both voiced and voiceless spirants, and this could have promoted the same practice with respect to the stops. However, there was nothing to necessitate the latter; it is difficult to find any other motivation than a certain mental tendency, a certain laziness. It is also characteristic that of the six old symbols for the stops, the three were retained which were the easiest to write \((b, t, k)\) and the three most difficult were abandoned \((p, d, g)\); however, there may have been another reason for giving up the symbol for \(p\): the sound \(p\) was rare in Nordic, and particularly rare word initially. The complete development from the older to the younger runes was clearly only possible under specific social conditions where very little was written, primarily fixed formulas, and where there was no requirement that what was written could easily \([23]\) be read by just anyone (knowledge of runes was a special art, a privilege). Later, of course, the runes were used to record literature (the Scanian law), but only after the alphabet had once again been supplemented.

The comparison with the younger Nordic runes is thus in no way contradictory to the view that the Gauls would hardly have been content with writing in an alphabet which could not distinguish voiced and voiceless stops if the alphabet had been used to record literature. But of course this argument leads only to a certain degree of probability, not to certainty.

The Lepontians and the Venetians also wrote in the North Etruscan alphabet and thus lacked symbols for \(b, d, g\). But since we only know the languages of these tribes from the inscriptions, we can know nothing about how the paucity of the alphabet corresponded to the actual phonological system of the language. However, for every Indo-European language there is great probability that the sounds \(b, d, g\) were present; and thus the alphabet speaks unfavorably where the possible existence of a literature is concerned.

The language of the Messapians surely played no significant role, compressed as it was between Oscan and the Greek of Southern Italy. The poet Ennius, whose home was either among the Messapians or at least among their closest neighbors and who said of himself that he had three souls because he could speak Latin, Oscan and Greek, makes no mention of Messapian.

The Greeks' northern neighbors on the Balkan peninsula, the Macedonians, the Illyrians and the Thracians were no great heroes of the pen. The Macedonians knew no other culture than the Greek; the Illyrians left no inscriptions, and their probable descendants, the Albanians, did not begin creating a literature of their own until the close of the 19th century; the Thracians were so disinclined toward writ-
ing that to date only one Thracian inscription is known (discovered in Ezerovo near Filippopel in 1912).

In Asia Minor things were worst with precisely those peoples whose kinship with the Greeks is most certain, the Phrygians and the Armenians. To be sure, the Phrygians could write; but they were so receptive to everything Greek that they can hardly have possessed much of their own to offset this. It is unlikely that the Armenians were able to write their language before the 5th century A.D. when Mesrop (with great difficulty, if we can rely on Moses of Chorinthia) created an alphabet for them. Prior to this the Armenians, when they had something to say, must have used the language and writing of their various rulers and finally of their Christian teachers (the Greeks and the Syrians). They seem to have been content with this for several thousands of years; if their native name Hay (y = j) actually meant 'Hatians', which it well may according to the sound laws, then they must have been the subjects of the Hittites and thus have observed the art of writing being practiced by others no later than the period of Hittite rule.

There has been much disagreement as to the types of languages spoken by the remaining inhabitants of Asia Minor. In 1896, quite prior to the discoveries which presumably in the near future will enable us to resolve these questions, this whole problem was considered in detail by P. Kretschmer in his significant book Einleitung in die Geschichte der griechischen Sprache. Kretschmer maintained then that with the exception of the Phrygian-Armenian group all the peoples of Asia Minor could be considered as making up "ein Volkstum sui generis" which was neither Indo-European nor Semitic; peoples from this language family of Asia Minor were said to have inhabited Greece prior to the Hellenians, and Kretschmer is favorably inclined toward the assumption that also Etruscan on the one hand and the Caucasian languages on the other belonged to the same language family.

Speculations concerning the pre-Hellenic population in Greece I will leave to others, although I do not reject the possibility that our ever increasing knowledge of the languages of Asia Minor may be able to provide us with certain solid clues for dealing with this question. For many reasons it is by no means far-fetched to suppose that tribes related to the Etruscans, and perhaps also to the unique language family now spoken only in the Caucasus, inhabited Asia Minor during the golden age of the Greeks. Judging from Herodotus' remarks that the Etruscans stem from Lydia, one could well imagine that such a non-Indo-European people could also have lived in precisely this area. Let us hope that the newly discovered inscriptions from Lydia will soon enable us to answer this question with a definite yes or no.
But even if the answer turns out to be yes, this still says nothing conclusive about the major portion of the population of Asia Minor. Kretschmer was much too hasty in pronouncing the verdict: "non-Indo-European". He passed much too lightly over our major source of knowledge at that time, the Lycian inscriptions, which in the very years to follow became the object of a penetrating study, one which brought to light a number of characteristics bearing extreme resemblance to Indo-European. And in addition, his argumentation includes several rather questionable points.

He relies, for example, upon a work by Karolidis on presumed relics of the ancient language of Cappadocia in the [25] present Greek dialects situated in certain out-of-the-way corners of this area at the foot of or actually within the Antitauros mountains (a score or more villages, among others Malakupja and Farasa). Kretschmer quotes the numerals lingir '6', tatli, tutli '7', matli, mutli '8', danjar, tsankar '9', which are admittedly well-suited for making an impression upon the unsuspecting reader; for where else in the world do such numerals exist? His argument is all the more liable to impress, since it must be taken on faith by most; Kretschmer does not provide us with the exact reference to Karolidis; in fact he has not even provided a true account. And not just anyone will be able to check his reference to this work which appeared in Smyrna in 1885; it belongs to Κουσέζου καὶ Βιβλιοθήκη εὐαγγελικῆς σχολῆς and bears the following title: "Α Comparative Glossary of Greek-Cappadocian Words. The Greek Dialect Spoken in Cappadocia and the Relics Preserved in it of the Ancient Cappadocian Language. By P. Karolidis, dr. phil. Awarded by the Greek Philological Society in Constantinople". The author of this work, who incidentally employs his presumed results to support the claim that the ancient Cappadocian language was Indo-European, is totally ignorant of the fundamentals of methodology for comparative linguistics and dialectology. His authority on Indo-European is Bopp (if only he knew how to use Bopp!); he believes that a dialect can be dealt with through a mere concern with its most obvious peculiarities, unusual words, etc., with no presentation of the dialect as a whole, of its sound laws and of the development of its inflectional system; all historical common-sense fails him as soon as he begins to deal with linguistic phenomena. Although he correctly assumes that the Greek language in Cappadocia stems from the period subsequent to Alexander the Great, he has no reservations about crediting the dialect with the retention of extremely ancient forms extending far beyond anything we know from any ancient Greek.
dialect (the pronoun ἥ in Faraša, for example in ἥ με πολ- 

κες = τς με ἔποςησος is viewed as the ancient pronoun from 

which Greek ἥ is derived; it is beneath 

his dignity to consider everyday modern Greek πο, written 

πο, neuter of ancient Greek πος). On page 116 of this 

work we find a section of the numerals. The introductory 

sentence reads: Τα άρθρα των Καππαδοκικών διάλεκτων ὀνό­ 

ματα εἶλον "Ελληνικά ("the numerals in the Cappadocian-Greek 

dialects are Greek"; but we are left in the dark as to their 

pronunciation). But ή άρα χωρική Φαρασική διάλεκτος 

(the primitive dialect in Faraša) has "preserved" an "indefi­ 

nite numeral" (i.e., an indefinite article), pronounced ἰ, 

but prevocally ὰν, for example ἰ νούς = [26] άνθρωπος 

τις, ἰ ἀδεψτ 'a brother'. Karolidis assures us that this is 

no "corrupt" form of ἰ, since they also say ἰ ἐναι = ἰ, 

τις. Karolidis finds it likely that this ἰ, ὰν is a relic 

of the ancient Cappadocian language and to be compared with 

Sanskrit sa- in sa-kṣat 'one time'. Thereupon Karolidis con­ 

tinues: 

Προς τούτον ἡ Φαρασική, ὥς καὶ ἡ τῶν Μαλακσιότων 

diálektos διεξήρξαν ἐν τῇ γλώσσῃ τῶν παιδῶν, ἐν τοῖς παι­ 

γνώσις, ἀρχαία ὄνομα ὀνομάτικα ἀπό τοῦ ἐν μέχρι ἰ ἔκ, ἐν ὰν 

ὁνοματικά ἰς ὀνοματικά ὀνο­ 

ματικά τῆς ἀρχαίας Καππαδοκικῆς γλώσσης ("In addition, the di­ 

alect in Faraša, as well as that in Malakupja, has preserved, 

in the language of the children, in certain games, a series 

of numerals from 1 to 10 in which remnants of the numerals in 

the ancient Cappadocian language can to a certain extent be 

detected"). The series for Faraša and Malakupja is indicated 

as follows:

<table>
<thead>
<tr>
<th>Farasa</th>
<th>Malakupja</th>
</tr>
</thead>
<tbody>
<tr>
<td>ἐνα</td>
<td>ἐνα</td>
</tr>
<tr>
<td>δις'ν (δίς'ν)</td>
<td>δίς'ν</td>
</tr>
<tr>
<td>τρις'ν (τρις'ν)</td>
<td>τρις'ν</td>
</tr>
<tr>
<td>κόνκαρ,</td>
<td>κόνκαρ</td>
</tr>
<tr>
<td>φένκαρ,</td>
<td>φένκαρ</td>
</tr>
<tr>
<td>τάτλη,</td>
<td>τάτλη</td>
</tr>
<tr>
<td>μάτλη,</td>
<td>μάτλη</td>
</tr>
<tr>
<td>λύγιρ,</td>
<td>λύγιρ</td>
</tr>
<tr>
<td>ἀδύγιρα,</td>
<td>ἀδύγιρα</td>
</tr>
<tr>
<td>δέκα</td>
<td>δέκα, δσσα</td>
</tr>
</tbody>
</table>

Karolidis adds a commentary, aimed primarily at inter­ 

preting these numerals etymologically. Concerning the word
c'άνκαρ we read: Το Φαρασιώτ. c'άνκαρ, ἑπερ ἀντιστοιχεῖς πρὸς τὸ δέ, ἄλως ἄλως τοῦ τοῦτο τοῦ τοῦτο τοῦ τοῦτος τοῦτος τοῦτος τοῦτος τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τὸ τ//------------------------------------------------------------------------------------------------
Tell-el-Amarna a great many clay tablets in Assyrian cuneiform containing correspondence between the Egyptian kings Amenophis III and Amenophis IV and various princes in the Near East were discovered. The letters were written in Assyrian, with the exception of three which were written in a hitherto unknown language. Thanks to the peculiarities of the Assyrian script (first and foremost the use of word-characters, which naturally allow for a certain access to the contents of a text even if its language is unknown) and to the fixed phrases of the epistolary style, Assyriologists were able to determine that one of the letters was from the king in Mitanni (on both banks of the Euphrates bordering on Asia Minor). This letter does not concern us in the present context; the language has been connected with the Caucasian languages and with the non-Indo-European population element in Asia Minor; but this is for the moment no more than a totally unproved hypothesis, resting on little or nothing more than the sheer geographic probability. The two remaining letters refer to the country of Arzawa, which already at that time was believed to be connected with the Hittite (the Hattian) culture; the Norwegian J. A. Knudtzon, who published the two letters in 1902, was inclined to seek the country in Eastern Cilicia or Southern Cappadocia. Right from the start these Arzawa letters evoked the same controversy as the Lycian inscriptions had done previously; there were those who considered the language as Indo-European (for example Knudtzon) while others violently denied this. However, it was actually quite clear (as with Lycian) that to deny the connection with Indo-European was absolutely impossible; there could only be doubt as to the type and degree of the relationship. Meanwhile, on an expedition led by Ernest Chantre 1893-94, several clay tablets with cuneiform script in the same language as that of the two Arzawa letters were found in the area of ancient Cappadocia near Bogaz-Köy; in 1905 Hugo Winckler (d. 1913), professor in Berlin, conducted an expedition to the same area and found additional tablets of the same type; the investigation was continued in 1906-07, and now thousands of such tablets were unearthed. These tablets provided sure evidence that the ruins near Bogaz-Köy were the remains of the ancient Hittite capital. And with this the Hittites emerged from the dimness which linguistic monuments discovered previously had been unable to illuminate, "written" (carved and cut) as they were in what is most probably to be viewed as the Hittites' own national writing system: the strange hieroglyph inscriptions from Syria, Cilicia, Cappadocia, those with which the Assyriologist Peter Jensen had grappled. A number of Jensen's readings may be correct (although he was mistaken in his notions concerning the linguistic position of the Hittites), but the results as a whole amounted to little or nothing. The tablets from Bogaz-Köy, on the other hand, were
relatively easy to interpret because they were in Assyrian cuneiform. Unfortunately, as yet the publication and interpretation of the cuneiform tablets and the application of the linguistic results has hardly begun. Most significant to date is Friedrich Delitzsch's publication in 1914 of several tablets containing fragments of word-lists in three languages: Sumerian (the pre-Semitic language in Babylonia), "Accadian" (as Delitzsch terms it; = Babylonian-Assyrian) [30] and Hittite. In addition Harri Holma has conducted a penetrating linguistic analysis of these word-lists (Études sur les vocabulaires sumériens-accadiens-hittites de Delitzsch, Helsingfors 1916; the introduction to this study includes mention of additional and readily accessible publications of Hittite texts). It is safe to say that the Arzawa letters are still our best source of knowledge of the Hittite language. Bedřich Hrozný, professor of Semitic languages at the University of Vienna and librarian at the university library, has circulated a pre-publication announcement of a forthcoming work: Die Sprache der Hethiter, ihr Bau und ihre Zugehörigkeit zum indogermanischen Sprachstamm, 1. Teil: Einleitung, Nomen und Pronomen [Hrozný 1916-17]; in addition, he has published "Die Lösung des hethitischen Problems, ein vorläufiger Bericht" in Mitteilungen der Deutschen Orient-Gesellschaft zu Berlin (Nr. 56, December 1915; summary by K. Wulff in Nordisk tidskrift for filologi, 4. series, V, pp. 81-88). This is the second time in the course of a few years that German science has been impudent enough to provide its scholarly colleagues with a collection of interpretation results prior to the publication of the texts upon which these are based. Although this procedure can be pardoned to a certain extent in view of the extreme difficulties connected with the publication of the texts, we are still left with the unpleasant impression that a concern with being the first and with securing a monopoly for as long as possible is rated higher than a concern for furthering science. Since we can be certain from Knudtzon's work that there is a relationship between Hittite and Indo-European, it is probable that some of Hrozný's results, perhaps even a great many of them, are correct; but he has a rare talent indeed, already evident in the remarks accompanying the pre-publication announcement, for arousing suspicion concerning his own qualifications as a linguist. And even if this suspicion should prove to be unfounded - let us hope so - it is still quite obvious that in no scientific investigation whatever can undocumented information be utilized. For the moment it is the duty of every linguist to be familiar with Hrozný's results without ever basing the slightest detail upon them.

The golden age of the Hittites had ceased before the Greeks' culture began to flourish. It was so decisively finished that no recollection of it is to be found in Greek lit-
erature. But their language surely did not cease to exist. In his Einleitung, [31] pp. 289-400, Kretschmer has attempted to show that his Asia Minor language family extended at least as far as Caria, Lycia, Pamphylia, Cilicia, Pisidia, Lycaonia, Cataonia and Cappadocia; and it seems to me that his argumentation for the linguistic unity in all of these areas is convincing. But this in turn means that dialects of Hittite were spoken in all of these regions. Consequently one of these Hittite dialects is Lycian. We may at least assume this until extremely weighty evidence to the contrary has been presented.

But we cannot expect the similarity between the ancient Hittite language from the middle of the 2nd century B.C. and the language of the Lycian inscriptions from the 5th and 4th centuries B.C. to be immediately obvious. First of all, we understand very little of Lycian; we are just barely able to interpret the common burial inscriptions, but become stumped when faced with a text of another type; and we understand even less of the ancient Hittite language (Hrozny's wisdom is, as noted, of no use). Moreover, the Lycian texts we can read and the ancient Hittite texts of which we can read portions are of such differing contents that the chance of encountering parallel linguistic data is reduced even further. Secondly, there is, of course, a time-span of 1000 years between the two stages of the language, and in addition it is possible, perhaps even probable, that there were dialectal differences between Lycian and Cappadocian already around 1500 B.C. And finally, the different writing systems can also contribute toward obscuring the similarities. The Lycians employed the Greek alphabet, which they had adapted to their own language through the addition of a number of new characters, so that the writing system corresponded rather precisely to the sounds of their language. The ancient Hittites, however, employed the Assyrian cuneiform without further adaptation, and it is at least very doubtful that this writing system could express the Hittite sounds and groups of sounds with sufficient precision.

Thus we cannot expect to be immediately struck by the similarity between the two languages; but it is no exaggeration to maintain that our present knowledge of ancient Hittite and Lycian in no way contradicts the above idea of a relationship between them. The relation between the ancient Hittite nominatives in  söyled accusatives in -n and the Lycian nominatives in a vowel and accusatives in a nasalized vowel looks exactly like the relation between an older and a younger language. Nor do I find it particularly prohibitive to assume an identity between the ancient Hittite relative and indefinite pronoun, nom. ku-îš and acc. ku-in, and the Lycian relative pronoun tí (indefinite tí-ke); that not only a plain k, but also a k pronounced with lip-rounding, in fact even a kw,
can be influenced by a following $i$ and develop into a $i'$ or into a palatalized $t'$ (perhaps to end up as a pure $t$) is clearly known to us from various Indo-European languages, for instance from Greek (where, for example, $*kh'is$, originally the interrogative pronoun, has developed to $t(u)s$). I shall refrain from going into further detail here; the above remarks [32] should be sufficient to illustrate both that a comparison between ancient Hittite and Lycian is possible and that the difference between the two stages of the language is quite significant, separated as they are by a millennium.

The Lycians are thus the descendants of an extremely ancient civilization in Asia Minor, of a people whose culture no doubt also had an influence upon the Greeks. Whether or not this people had a [33] literature cannot be dismissed as rapidly as was the case with the northern peoples of Asia Minor. There are even certain circumstances which point in the direction of an affirmative answer. I am primarily thinking of the great inscription from Xanthus, which is carved on four sides of a stele; it contains, as we know, a Greek epi-
gram of 12 lines and a [34] historical inscription in Lycian concerning events from the year 412 (Thucydides VIII 28); but in addition to this Lycian inscription, composed in the same form of the language which occurs in nearly all the Lycian burial inscriptions, the stele bears an inscription in another dialect (Lycian 2), which deviates from the normal dialect (Lycian 1) in several ways, among others in having, in a num-
ber of instances, $s$ corresponding to $h$ in Lycian 1 (and also with respect to other phonetic peculiarities). Lycian 2 oc-
curs only on the Xanthus-stele and on a sarcophagus from Antiphellus, and in both cases the text is divided into sec-
tions of equal length by means of a mark having the form $)$ (on the Xanthus-stele the sections are of about 45 syllables, on the sarcophagus they are somewhat shorter). Arkwright has assumed, and surely quite correctly, that this is a metrical division and that Lycian 2 is a special poetic language.

This agrees well with the fact that we cannot locate Lycian 2 geographically in relation to Lycian 1, whereas, on the other hand, there are a number of reasons which lead us to the as-
sumption that Lycian 2 possesses more ancient characteristics than Lycian 1 (where $s$ and $h$ correspond, $s$ is the most ancient; a change from $s$ to $h$ has been demonstrated for a number of languages, a change from $h$ to $s$ doubtfully anywhere). Thus, Lycian 2 was obviously an older dialect which still held its own in poetic usage at a time when a younger form of the language had become a common spoken language in Lycia and had asserted itself as a written language for prose and matters of a practical nature. Such circumstances seem almost of necessity to require the existence of a written literature.
The final result of these rather lengthy considerations is this: even if some of the neighboring peoples related to the Greeks had a written literature, most of them apparently did not. But now I shall distress my readers with the pronouncement that the entire investigation concerning this subject is quite superfluous. Because even if there had been an abundance of written literature everywhere, the Greeks would have been unable to benefit from it. In antiquity book production was so toilsome that there is no way it would have been able to extend beyond that which addressed itself to a relatively common and broad interest, to the educated public in its entirety or at least to great portions thereof. And the study of foreign languages without reference to practical interests will always be an undertaking for specialists due to the amount of material required by it. The best proof for the statement that antiquity's book market would have been unable to accommodate such specialist interests lies in the fact that the scanty material which did exist for such studies has been lost without leaving a trace. We know that Ovid, during his exile in Tomis on the Black Sea, wrote a poem in honor of Emperor Augustus in the language of the area, Getic; we can bemoan the loss of this poem, but it is easy to see how it had to be lost; who is to have preserved it and copied it if the Getes themselves did not? The Punic passages which Plautus incorporated into his comedy Poenulus were only copied because the entire comedy was preserved and copied; but in what state have they been transmitted to us? so incomprehended and distorted that not even the most learned expert in the Semitic languages can interpret them with certainty. If we had no other knowledge of Punic and the remaining Semitic languages these passages could not teach us the slightest detail.
Nor was there any flourishing of linguistic science when the new spirit of Christianity replaced antiquity's way of thinking and created literature among numerous peoples who had previously been without. The literature created in this way is of inestimable significance for linguistics today, but in its own age it did not and could not have any linguistic import. In the same way as Christianity, Buddhism too brought honor to the barbarian languages and provided us with linguistic monuments for which we are exceedingly grateful today. From the point of view of Indo-European linguistics it is natural in this connection to think first of all of the literary relics unearthed in the most recent investigations in Chinese-Turkestan. These are in that strange Indo-European language spoken in the area around Kuča and Karašar which agrees in particular with Italic and Celtic and which, rightly or not, goes under the name of Tocharian. But in its own day Buddhism, just as little as Christianity, produced no science of linguistics. [36]

The many new peoples who became the bearers of the European culture after the close of antiquity each had their own linguistic horizon which was as narrow as that of the Greeks and Romans. And so it was necessarily to remain until the invention of printing. First with the art of printing was linguistic comparison made possible. And although it seems as if things progress slowly even after the invention of printing, we cannot deny that there is progress.

Thus, when Joseph Justus Scaliger (1540-1609) in his little treatise (printed in Jos. Just. Scaligeri Opuscula varia antehae non edita, Paris 1610, pp. 119ff.) divides the languages of Europe into four major classes (Latina matrix, Graeca, Teutonica, Sclauonica, also termed the deus-, Θεός-, gott- and bog-languages) and seven minor classes (Albanian,
Turkish, Hungarian, Finnish together with Lappish, Irish, Breton, Basque), he illustrates a perspective which no ancient author could have possessed. But he adduces no linguistic data as proof for the correctness of his classification, and it is quite evident that his classification is based solely upon an unsophisticated appraisal; thus he has merely gathered together those languages whose mutual relationship would stand out for any observer; where an investigation would be required in order to determine the kinship he did not discover it. Thus he did not observe that Hungarian belongs together with Finnish and Lappish nor that Irish and Breton are closely related. Nor was he aware of the fact that the majority of his matrices contain a number of correspondences which cease to be valid as soon as we move on to Turkish, Hungarian, Finnish and Lappish, and Basque. Quite the contrary, he declares that all eleven classes (matrices) are without mutual relationship (nullo inter se cognitionis vinculo conjunctae). Of course, he must have observed certain correspondences between Greek and Latin and, for example, the Germanic languages (Teutonica matrix), but since this still does not lead him to conclude anything concerning a relationship, he must have explained all these similarities as the result of borrowing (having in mind the cultural dependency of these peoples upon the Romans and Greeks). In other words, [37] he does not progress beyond antiquity's conception of language; cf. pp. 11ff. above.

That one still, in spite of the broadened perspective, remained anchored in antiquity's beliefs can, for example, be observed in the Dutchman Gerhard Johannes Voss (1577-1649) who authored an Etymologicum Latinæ linguae (which appeared in a second edition in Leiden, 1664). I mention him honoris causa. Many of his remarks are quite reasonable. For example, having referred to antiquity's etymology of the
word *bellum* 'war', which operated with the principle of ἄντιφράσις (deriving the meaning of a word from its opposite) and maintained that war was called *bellum* because it is *minime bellum* (by no means beautiful), he notes: "Sed ἄντιφράσις, ut saepe diximus, tantummodo inscitiae est asylum"; he is quite correct himself in explaining *bellum* from *duellum*. At times he is also successful in his attempts to find Greek words which correspond to those in Latin. But his notions concerning the relationship between Greek and Latin can be illustrated, for example, by means of his article on Latin *sūs* 'pig': "sūs ut quidem Donatus scribit factum a Graeco "C", converso spiritu aspero in s quomodo ab "C" est *sex*, ab ēntā *septem* etc. Sed nihil opus eo recurrere, cum Graeci ipsi et "C" dicant et "C". And on p. 18 Vossius provides a long list of words in which "h mutatur in s" (δαλα: *sal*, ἀλλομαί: *saliō*). To consider *h* older than *s* is excusable for the ancient Romans who had only two languages to compare, but for a European scholar, who had a third language available for comparison (the Germanic dialects), it is an indication of great historical uncertainty. This is a practice which corresponds to Scaliger's theory.
5. The 18th Century: Gottfried Wilhelm Leibniz

In the 18th century we encounter a more modern spirit. The famous philosopher G. W. Leibniz, whose lifetime falls primarily within the latter part of the 17th century (1646-1716), can be mentioned as the foremost figure in this development. He presented his system of linguistic genealogy in a treatise entitled *Brevis designatio meditationum de originibus gentium ductis potissimum ex indicio linguarum*, published in the [38] beginning of the first volume of the Berlin Academy's papers, which at that time had the title *Miscellanea Berolinensia*; the volume appeared in 1710. Leibniz assumes here that the languages in a great portion of "our continent" (Europe and Asia) descend from one and the same original language. He divides these languages into two groups, a northern group which he calls the Japhetic languages and a southern group which he calls the Aramaic and to which he assigns all the languages which we now group together under the term Semitic plus Coptic, which, as he notes, is quite different from these. "Quod si septentrionales ad Japhetum referas, meridionales fratrum Semi, Chami-que propaginibus non inepte tribuentur". As a substitute for the term Japhetic he also employs Scytho-Celtic, "Scythian" indicating the eastern and "Celtic" the western section of the area. To the western section he assigns (Italic), Celtic (in the true sense) and Germanic, these last two being closely connected; he correctly characterizes Basque as totally different from the other European languages, noting that it perhaps stems from Africa. To the eastern section he assigns (Greek), Slavic, Turkish with Mongolian, and Finno-Ugrian with Samoyedic, pointing out, however, that Turkish, Slavic and Finno-Ugrian are mutually quite different. Regarding Persian, Armenian and Georgian he assumes that they are a mixture of "Aramaic" and "Scythian" (there
is some truth to this, since Modern Persian, although an Indo-European language, abounds in loan-words from Arabic. He also notes with respect to Persian that with the exception of the word 'God' he has been unable to find any more Germanic elements than can also be found in Greek.\footnote{13}

Leibniz' classification of these languages differs, as we observe, quite radically from Scaliger's. It is broader by far, but in turn its conclusions (as opposed to Scaliger's) are incorrect in several instances. Leibniz (or the sources upon which he built) obviously had difficulty discerning between correspondences due to borrowing and more truly profound correspondences; otherwise he would not have been capable of placing Turkish, Slavic and Finno-Ugrian in one and the same group. And the huge grouping together of everything which we now designate as Hamitic and Semitic, Caucasian, Indo-European, Finno-Ugrian with Samoyedic, Turkish with Mongolian (five to eight language families) rests, of course, not upon a thorough-going analysis but merely upon divination. It is not acquired knowledge but an intuition.

But at any rate Leibniz' advancements reveal that men's minds were now open to a deeper understanding of the relationship between languages. And now a systematic collection of material commences, partly due precisely to Leibniz' initiative. The result is a series of huge works containing surveys of hundreds of languages; the last was Adelung's Mithridates, Berlin 1806-1817, which treated close to 500 languages. I refer in this connection to Vilh. Thomsen, Sprogvidenskabens historie, pp. 34-37.
And the work of collecting is fruitful. The attempts to prove linguistic kinship by means of etymological tricks according to the phonetic method initiated in classical antiquity and further developed thereafter, tricks which had permitted the derivation of every possible language from Hebrew as well as every other conceivable naivety, even Leibniz had to find controversial; but these things naturally came to a halt once the material could actually be surveyed. The difference between that type of raving and the solid investigations concerning linguistic kinship could not help but be noticed. The conclusions of the visionaries were found to be fallacious; but where the fault lay was only noticed in part; even though it was clear that their treatment of phonology was far-fetched, the awareness of where the fundamental error lay was still absent; there was no notion of method in the study of phonology. On the other hand it became clear time after time that the languages which could reasonably be considered related agreed with respect to their inflectional systems, and thus agreement in the inflectional system was set up as a criterion for determining linguistic kinship. This notion was voiced from various quarters toward the close of the 18th century, for example quite pointedly by Gyarmathi in his book *Affinitas linguæ Hungaricae cum linguæ Fennicae originis grammaticae demonstrata*, Göttingen 1799.

And it was this methodological principle that pulled the science of etymology out of the swamp in which it had been firmly stuck ever since antiquity and that made possible the emergence of a science of comparative linguistics. For it is indeed a fact that if you stick to this principle, you will never come to compare unrelated languages; and he who looks only where something is to be found will certainly
make discoveries, even if his method in its details merely rests upon intuition, not upon theoretical clarity.

But this principle does not contain the whole truth. It is true that languages with similar inflectional systems are related; but it is not true that languages with differing inflectional systems are necessarily unrelated or absolutely cannot be recognized as related. It is totally without justification that the distinguished French linguist Meillet in the periodical *Scientia* 1914 p. 417 declares: "A en juger par le vocabulaire, personne ne saurait dire si l'anglais est une [41] langue germanique ou une langue romane", and likewise p. 422: "Si presque toute la grammaire proprement dite tient en quelques règles de position relative des mots ... la question des parentés de langues est pratiquement insoluble". That English is a Germanic and not a Romance language can easily be proved even without consideration of its inflection; the entire central portion of the vocabulary agrees, of course, in accordance with precise phonetic laws, with other Germanic languages. And even more distant kinship can be demonstrated without the help of inflection. English and Italian are both Indo-European languages and as such related to each other. And I have no doubt that this kinship could be proved even if these two languages were quite isolated, even if there were no other Indo-European languages in existence and even if we had no knowledge of the older forms of these two languages; but in proving this the inflectional system would only come to play a very insignificant role; the only inflectional endings in Italian which have correspondences in English are the ending -to in participles like amato 'loved' and the comparative ending (migliore 'better'), which even then is rare in Italian.

Correct assumptions concerning the evidence for proving linguistic kinship are found in Rask. In his *Undersøgelser*
om det gamle nordiske eller islandske Sprogs Oprindelse he points, on pp. 34-35, quite clearly to the inflectional sys­
tem as a sign of linguistic kinship. Subsequently, however, on pp. 35-36, he adds a remark which shows that he was com­
pletely aware of the fact that also another firm methodolo­
gical basis exists in addition to the inflectional system:
"When, in the most essential, most carnal, most indispensible and original words, the foundation of the language, there are similarities between two languages, and then a suffi­
cient number to allow for the formulation of rules for the changes in letters from the one to the other, then there is a basic kinship between these languages". This statement sounds (aside from the use of the word letters instead of sounds) perfectly modern; but Rask was ahead of his time. In reality it is merely the principle concerning the inflec­
tional system as a touchstone for linguistic kinship, which lent stability to the science of linguistics around the close of the 18th and the beginning of the 19th century. [42]
7. Early 19th-Century Linguistic Comparison

However, this fine principle [concerning the inflectional system] would not have been sufficient to allow for the development of comparative linguistics. There was also a need for suitable material to work with. I mentioned above that linguistic science would be able to establish the kinship between English and Italian, even if these languages were completely isolated. However, it is obvious that material of this nature would have been extremely ill-suited for the beginning science; the distance between the languages compared is far too great, the tasks far too difficult. But the material with which Gyarmathi worked in his excellent investigation of the relationship between Magyar and Finnish was actually not much easier to grapple with, and this is sufficient explanation for the fact that Gyarmathi's book did not lead to a breakthrough. The Finno-Ugrian language family was not suited to provide the infant science with its first nourishment; and in addition the Finno-Ugrian languages lay too much on the periphery of most European philologists' horizons and interests; the family numbers too few cultural languages and must thus be content with relatively few workmen. And we can quickly add that neither was the Semitic language family suitable as an initial field of investigation for linguistic comparison. To be sure, where this family is concerned, we can refer to a methodical study which is nearly 100 years older than Gyarmathi's: Job Ludolf, Dissertatio de harmonia linguæ Aethiopicae cum ceteris orientalibus, 1702; but here the likeness between the two languages is too great; it is immediately obvious, just as obvious, for example, as the likeness between the different Germanic languages; thus the impetus for detailed analysis was too slight in this instance. And neither was there within the area of the Indo-European family any abundance of
clear and serviceable material around the beginning of the 19th century. The direct comparison of the modern European languages with Greek and Latin leads to too poor results; and the majority of the ancient linguistic monuments upon which we now build were at that time either totally unknown or not easily accessible and inadequately studied. The situation was perhaps best where our own language group was concerned; [43] the inestimable Gothic Bible translation had already been published several times in the 17th century (for example by Stiernhielm: *Evangelia ab Ulfila . . ex Graeco Gothice translata, nunc cum Parallelis Versionibus Sueo-Gothica, Norræna seu Islandica et Vulgata Latina edita*, Stockholm 1671); but the study of the ancient Germanic languages was by no means simple; we recall that it was first Rask who provided us with an acceptable Old Norse and Old English (Anglo-Saxon) grammar. Nothing could be done with Celtic; the ancient Old Irish manuscripts, which made a comparative treatment possible, were first dealt with grammatically in 1853. Even in the beginning of the 19th century the Oscan-Umbrian linguistic monuments, which are of such great significance with respect to the history of the Latin language, could still not be kept distinct from Etruscan; K. O. Müller was the first to untangle this puzzle in his work *Die Etruskier*, 1828, and not until the 30's did Lassen, Grotefend and Lepsius commence their penetrating studies of these dialects. The prominent Slavic philologists in the beginning of the 19th century still had to struggle forth with great difficulty toward correct assumptions concerning Old Slavic. The ancient Iranian language was still basically a closed book; during the reign of the Danish king Frederik V Carsten Niebuhr brought back reliable transcriptions of the Old Persian cuneiform inscriptions, and in 1802 Grotefend took the first steps toward a reading and interpretation; but there was still much to be done; knowledge of Avestan was in a misera-
ble state; there was the tendency to assume that Anquetil du Perron had been the object of a deception, and this amazingly persistent and dedicated pioneer lay in his grave before amends were made. These came in 1826 with Rask's work *Om Zendaprogets og Zendavestas Ælde og Ægted* , which simultaneously brought the understanding of the Persian cuneiform inscriptions a giant step forward. But most importantly, knowledge of Sanskrit was still lacking at the beginning of the 19th century.

That much could be accomplished, even with this defective foundation, was demonstrated by Rask. His *Undersøgelse om det gamle nordiske eller islandske Sprogs Oprindelse* (evoked by an [44] essay competition in 1811, completed in 1814, published in 1818), written with no knowledge of Sanskrit, can quite justifiably be termed a comparative grammar for our language family, or at least a comparative grammar in embryo containing the beginnings of both comparative morphology and comparative phonology — a comparative grammar which had no competitors during Rask's lifetime. But it was clearly only Rask's superior mind that made this possible; and even then it is questionable to what extent Rask's work, without a broadening of its foundation, could have become the starting point for an up and coming comparative linguistics. Was it at all possible, on this basis, to progress appreciably further than Rask had done? If the answer is that it was impossible or at least impossible for most scholars, then this is simultaneously an admission that Rask's work did not contain to a sufficient degree the impulses for further research which were necessary to allow for the creation of a comparative linguistics.
8. THE ACQUAINTANCE WITH SANSKRIT

In contrast, Sanskrit was unsurpassed in its power to stimulate investigation. The mere acquaintance with this language had to seem revolutionary, not only because it was something new, something which lay quite outside the long familiar cultural sphere, something which the scholar faced freely and unbiased by the ancient delusions which were not so easy to shake off where Greek and Latin were concerned, but also because Sanskrit, as mentioned above, is structurally an extremely transparent language. Just as this clear structure had given rise to an admirable Indic grammar, likewise, when it came to influence European linguists, it almost of necessity gave rise to Indo-European grammar.

But Sanskrit became known relatively late in Europe. That it was unknown in antiquity in spite of the Greeks' reign in Bactria during the period subsequent to Alexander the Great is self-evident in view of our previous discussion of antiquity's mind and predisposition. Not only did the Greeks view the Indians as "barbarians", but also the Indians looked upon the Greeks in a similar fashion and preferred to have as little as possible to do with them. In the course of the first centuries of more modern times a few Europeans had the opportunity to acquire a more or less detailed knowledge of Sanskrit (the earliest was probably the Italian Sassetti, who was in India 1583-88 and who speaks of the similarity between Sanskrit and Italian in his Letters, published in Florence in 1855). The first European to write a Sanskrit grammar was "natürlich ein Deutscher" (this "natürlich" stems from Johannes Schmidt, who once uttered this expression with every possible emphasis from the rostrum at a public lecture). But was it Father Heinrich Roth from Augsburg in the 17th century or was it first the Jesuit Hanxleden in the 18th century? It is certain, at any rate, that
neither of these men managed to publish his grammar. The English dominion in India was the first to gradually bring about more contact between India and Europe, and English scholars were the first to spread a knowledge of Sanskrit in Europe. How this language affected these scholars can best be illustrated by the often quoted statement of William Jones from 1786 that Sanskrit, in relation to Latin and Greek, showed "a stronger affinity, both in the roots of verbs and in the forms of grammar, than could have been produced by accident; so strong that no philologer could examine all the three without believing them to have sprung from some common source which, perhaps, no longer exists; there is a similar reason, though not quite so forcible, for supposing that both the Gothic and Celtic, though blended with a different idiom, had the same origin with the Sanskrit" - an extremely clear statement in which merely the ill-placed perhaps and the conviction that the more deviating appearance of Gothic and Celtic was necessarily the result of linguistic blending point toward notions of the past (and even this no greater than so many things among the very founders of comparative linguistics). But not until the first decade of the 19th century did Sanskrit grammars written by Englishmen begin to appear, and because of the continental embargo these books were unobtainable on the continent, and in addition dictionaries and texts were lacking. However, the library [46] in Paris had a few Indic manuscripts, so that Sanskrit studies could be conducted there. Friedrich Schlegel studied these during his stay in Paris beginning in 1803, aided by the Englishman Alexander Hamilton. As the result of his studies Fr. Schlegel published Über die Sprache und Weisheit der Indier, Heidelberg 1808. The significance of the book was essentially that of awakening an immense interest and enthusiasm for the Indic culture, but it also expressed certain correct linguistic notions; among other
things the expression "vergleichende Grammatik" is employed here for the first time. These words contain a complete manifesto: "Die vergleichende Grammatik wird uns ganz neue Aufschlüsse über die Genealogie der Sprachen auf ganz ähnliche Weise geben, wie die vergleichende Anatomie über die höhere Naturgeschichte Licht verbreitet hat". Sanskrit had made its mark on Fr. Schlegel as it had on William Jones.

But the first to introduce true Sanskrit scholarship in Germany was Fr. Schlegel's brother August Wilhelm Schlegel, and the first to initiate a comparative analysis of Sanskrit was Franz Bopp with his treatise Über das Conjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache, Frankfurt am Main 1816.

Über die Sprache und Weisheit der Indier.

Ein Beitrag zur Begründung der Alterthumskunde
von Friedrich Schlegel.

Heidelburg, bei Mohr und Zimmer. 1808.
9. Bopp's Position in the History of Linguistics

It was unavoidable that this work, in spite of its neither few nor insignificant errors, would have a far stronger effect in inspiring further research than Rask's far superior, more mature and more significant work came to have, also stronger than Rask's work could possibly have had even if it had been written in a major language. Bopp's little treatise is the true start of modern linguistic comparison. Sanskrit won the victory, and if any one man is to be singled out as the founder of comparative linguistics, it must be Bopp.

It would perhaps be best, however, to avoid this ambitious title. It would only be appropriate, if Bopp had introduced something entirely new. But he did not. [47] Comparative linguistic analysis had been practiced by others before Bopp, and more ably, and neither was Bopp the one to discover the kinship between Sanskrit and the European languages. And this catchy title awakens far too easily the naive notion that Bopp was the most significant linguist of his age, and from this quite natürlich certain further conclusions are drawn concerning the relationship between German and non-German brilliance. To counter such notions it might be useful to undertake a brief comparison between Rask's Undersøgelse and Bopp's Conjugationssystem: Rask's work is a fairly complete presentation of the comparative grammar of our language family, though naturally only in its main outlines (the book fills 312 pages); it considers both phonology and morphology. Bopp's work (or rather the portions with which we are concerned in this connection, 157 small pages; Bopp's translations from Sanskrit are of no concern to us) includes merely a portion of the morphology and deserves absolutely no praise for its phonology. Rask dealt with 5 language branches: Germanic, Slavic, Baltic, Greek,
Latin; Bopp likewise with 5 language branches: Sanskrit, Persian, Greek, Latin, Germanic; but Persian, which he naively treated in connection with Germanic, should actually have been omitted. Rask's broad knowledge outside Indo-European is not paralleled by Bopp. And instead of Rask's clear, empirically founded, almost modern methodological introduction we find with Bopp an erroneous philosophical theory (philosophy has never produced anything but trouble when it has raised its head in the area of linguistics) which causes him to explain every possible ending as the suffixed verb 'to be' (in no case of common Indo-European justifiably). I shall refer in this connection to the manner in which Jacob Grimm, *Deutsche Grammatik*, I, Göttingen 1819, comments upon Rask's and Bopp's writings in his preface, p. XVIII; there can be no doubt that Grimm's words designate Rask's work as the primary treatise and Bopp's *Conjugationssystem*, in contrast, more as a kind of appendix to it, but to be sure as "Schlussstein der ganzen Untersuchung".

But on the other hand we must not forget that *Conjugationssystem* was merely the introduction to Bopp's activities and that Bopp, in his huge work, *Vergleichende Grammatik des Sanskrit, Zend, Griechischen, Lateinischen, Litthauischen, (Altslavischen), Gothischen und Deutschen*, 1833-1852 (a total of 1500 pages), laid the groundwork for the systematization and organization of the new science, which will always be to Germany's credit. The same systematization of linguistic comparison was carried out within a more limited area (the Germanic language branch) by Jacob Grimm in his *Deutsche Grammatik*, which is a comparative presentation of Gothic, Old High German, Old Saxon, Old English, Old Frisian, Old Norse, Middle High German, Middle Saxon, Middle English, Middle Dutch, Swedish, Danish, New High German, Modern Dutch, Modern English. With reference to these works we can justifiably place Bopp's and Grimm's names at the head of the new
breakthrough in comparative linguistics.

Franz Bopp
über das
Conjugationssystem
der Sanskritsprache
in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache.

Herausgegeben
und mit Vorreden und Erinnerungen begleitet
von
Dr. F. J. Windischmann.

Frankfurt am Main,
in der Andreäischen Buchhandlung,
1816.
How then do these two great German linguists deal with phonology? The answer must be that they initially adhere to a practice which by no means supercedes that of the previous centuries. Grimm displays, even in the second edition of his first volume (1822), astonishing phonetic uncertainty; thus he says on p. 3: "In unserem worte: schrift drücken wir acht laute mit sieben zeichen aus, f. nämlich steht für ph.". The 8 sounds in this word are thus: s, c, h, r, i, p, h, t. The Russians, Grimm notes, would express the three sounds s, c, h with one symbol: "dergleichen eigene buchstaben zu sp, st und anderen lieblingslauten unserer sprache wären ihr so dienlich, als es dem Griechischen sein Ψ für ps. ist". The long vowels are viewed on p. 6 as two short vowels; one proof of this doubled characteristic is the fact that a becomes æ by umlaut, i.e., ae (æ is thus in Grimm's opinion a group of two vowel sounds). On p. 11 f and þ are viewed as double sounds, but, Grimm notes, the frequent use of the single symbol was the reason "dass man sie faktisch in den meisten mundarten für einfache gelten liess, daher sie theils keine position machen, theils sich geminieren können". On p. 133 the Old High German orthographic use of ff is considered as being "in der theorie ganz verwerflich", because f is a double sound; ff is phph, "folglich (in einer silbe) so unaussprechbar als es thth und əhəh sein würden". pf is considered as a triphthong.

As an example of Grimm's original practice regarding the assumption of phonetic changes I will quote a passage from the first edition of the first volume, pp. 386-87. Here Grimm compares Germanic hw-, later w-, in interrogatives with Latin qu-, and sets up correctly the correspondences cornu: horn, ἡρώς: OHG hersa, caper: ON haf, but adds immediately thereafter incorrectly: "Auf ähnliche Wei-
se verhalten sich das englische well, deutsche quelle, dänische kilde; das deutsche weinen, gotische quainon; das lateinische vivus, gotische quives, deutsch quiek und keck, angelsächsisch owo, cuic, daher auch vacca zu kuh und quiek gehört u.a.m. Im Latein ist mitunter noch mehr geschehn und sogar das v in u verweicht, denn die interrogativen und relativen Partikeln: unde, ubi stehen für quunde, quubi, cubi. It is evident that here there is no trace of a suspicion concerning the regularity of phonetic development or the historical nature of sound laws. After noting that Latin qu- in the interrogatives corresponds to hw- in Germanic, Grimm does not conclude that an original kw- in Germanic was shifted to hw- wherever it occurs; on the contrary, he concludes, just as had been done during all preceding centuries, that kw- throughout the world now and then, at a whim, was exchanged with hw- or w-; and he finds support for this assumption partly in a series of incorrect etymologies and partly in a few correct etymologies which he is incapable of understanding in terms of phonetic history.

With Bopp things take an even worse turn. He failed completely to observe the phonetic rules which actually existed; German [50] schwitzen beside English sweat and Latin sudor he explains by assuming, Conjugationssystem p. 138, that an s has been added to the root (it is the High German sound shift he fails to recognize here). He is not the slightest disturbed in allowing the same starting point to lead to different results; both Latin legat and leget are said to derive from *legait (p. 98). Sound changes are a liberty which language can make use of or not at will: on p. 152 as an explanation of Gothic og 'I fear', preteritum ohta, he notes that g before t can change into h and that here linguistic usage has taken advantage of this possibility; if g had not changed into h an i would have been inserted. Bopp says expressly on p. 57 that a sound "nach
Willkür" or "accidentally" changes into another. He uses figurative and vague expressions, which makes it almost impossible to determine whether he is merely describing how one form or another sounds or whether he wants to indicate how it has developed. Concerning Sanskrit 3. person dual perfect nanardatur and 3. plural nanardur (of nardati 'he roars') Bopp comments on p. 25: "Die dritte Person Plur. unterscheidet sich von der dritten Person Dual, blos dadurch, dass sie das t der dritten Person, welches der Dual beybe­hält, abwirft". If this were merely a description of the forms (= "there is a t present in the dual, but it is absent in the plural"), then it would be correct; but Bopp's intent with these remarks is that of an explanation: both forms are actually identical, but by chance the t has been lost in the one form (a fanciful sound change, but not too fanciful for Bopp). On the same page we read concerning nanardur: "In der dritten Person Plur. wird durch die Endung -ur blos Vielheit angezeigt, das t der dritten Person aber ist ver­schwunden"; here it seems unquestionable that Bopp did not distinguish clearly between these two things: "t is not present" and "t has been lost". On p. 64 he treats the forms οτικατον and οτατον in the perfect 2. dual of Greek οτατι; χ, Bopp assumes, has been inserted, to make it sound better, into the perfect between the final root sound and the a which connects "die gebeugte Stammsylbe mit dem Per­sonskennzeichen"; but when the root ends in a vowel this a is less of a necessity; "mit dessen [51] Verschwinden muss nothwendiger Weise das zwischen die zwey Vokale eingeschal­tete χ wegfallen, und das in η verwandelte α der Wurzel kehrt zu seiner ursprünglichen Form zurück". If we take Bopp at his word he is apparently saying here that the form ο-στα­τοv arose in the following manner: an α was inserted in order to connect the root (στα-) with the ending; to make it sound better a χ was inserted between the root and this
α; the final α of the root was lengthened to η; in this way
we got ἔστηκατω; but then it became apparent that the con­necting vowel α was superfluous; it was thus allowed to dis­appear taking υ along with it; then η became α once more.
Perhaps, however, Bopp would prefer to acknowledge the in­terpretation of his words which says that in ἔστα­τον the
connecting vowel α with its preceding υ was never present
and that as a result the root vowel α never changed into η.¹⁸
One thing is certain, Bopp was quite muddled in his thinking.
His practice regarding phonetic history is a mess. And it
remains so in his major work, Vergleichende Grammatik. It
is already characteristic that his huge work contains no
real phonology but merely a section of 100 pages with the
characteristic title "Schrift- und Lautsystem", 60 pages of
which deal with Sanskrit and Avesta while Greek, Latin and
Lithuanian are passed over (in the second book, to which Old
Slavic, originally omitted, has been added, there are, in ad­dition a few comments on Slavic phonology). Thus, Bopp
wished to treat only morphology; that phonology could be
dealt with scientifically at all did not occur to him. The
same was true for Grimm; the first volume of his Deutsche
Grammatik contained no phonology in its first edition. But
in practice Bopp is the worse of the two. A single refer­ence will be sufficient to demonstrate that he is the same
in his Vergleichende Grammatik as he is in his Conjugations­system: on pp. 197ff. he assumes that -sm- in certain Indic
pronominal forms corresponds to Gothic -ns-, -sw-, -gk-, 
-gq-, -mm- (only the last is correct), but it does [52] not
occur to him to search for reasons for the differing devel­opment or to look for additional examples of the same change
in Gothic (only for -mm- would it have been possible to find
such examples); -ns- he supports with the fact that Sanskrit
-sm- in the Prakrit became -mh-; -sw- derives from the
changes from s to z and from m to w, which he terms "not
surprising" and "quite normal" (it cannot be denied that there are examples enough showing a change from m to w, but just not in Gothic; Bopp did not recognize that it was this point which was crucial). Precisely because Bopp had absolutely no other Ariadne-thread in the labyrinth of the linguistic world than the observation of agreement in the inflectional system, precisely for this reason he was bound to commit frightful acts once this Ariadne-thread came to an end; his undertakings in the area of the Malay-Polynesian languages are no chance misfortunes.*

* [Editor's note: Pedersen is referring to two papers by Franz Bopp presented to the Königlich-Preussische Akademie der Wissenschaften in Berlin in 1840, and published two years later in the Abhandlungen of its Philologische und Historische Klasse (pp. 171-246 and 247-332 respectively): "Über die Verwandtschaft der malayisch-polynesischen Sprache mit der indisch-europäischen" and "Über die Übereinstimmung der Pronomina des malayisch-polinesischen und indisch-europäischen Sprachstammes". - Modern historians of 19th-century linguistics such as Anna Morpurgo Davies of Oxford might no longer regard this rapprochement as misleading as Pedersen did.]
11. Rask's Significance in Phonological Study

The above is sufficient illustration of the fact that linguistic comparison in Bopp and Grimm had precisely the same starting point with respect to an understanding of phonetic history as did the etymological visionaries of the preceding age. But now the correct objects for comparison had been discovered, and therefore it was inevitable that numerous correct discoveries would be made and inevitable that the material collected little by little would instruct linguists in the laws of development. But even if this is so very certain, we should not for this reason forget the men whose keen eyes first recognized these laws and spared their fellow scholars from numerous wanderings in the wilderness. And as the first of these men we must mention Rask. His main work belongs to the Sanskritless period and as such is merely a precursor of the new science; but his genius was far ahead of the works of 1816 and 1819.

To be sure, Rask is also far from being modern in his historical phonetic practice. But on the one hand he possesses common sense and caution, which protects him against offences like those committed by Bopp. On p.45 he denounces the phonetic treatment of the older etymologists by [53] commenting that there hardly exist two sounds "so dissimilar that they cannot be found to alternate with each other in one language or another. Now if one should take the liberty to change all the letters in a word at will to whatever word one desires, then it follows that there can hardly exist two words so different that you would not be able to etymologize them together, and this, which is the most remarkable of all, in spite of the fact that for all of these changes other unquestionable examples can be found". And on the other hand Rask continually has his eyes open to observe the phonetic relationship between the languages he is comparing: he
speaks, on pp. 140-41, of Slavic o from a, of Russian ĺ from kt; he correctly identified some of the most important sound laws separating Slavic from Lithuanian (for example the fate of the s-sound, pp. 127, 129; thus it is quite unjustified when Bopp in the preface to volume 2 of his Vergleichende Grammatik praises himself for having been the first to observe that Slavic ŏh can derive from s); an Armenian sound law (h from p) is mentioned by Rask on p. 51. And it was Rask who pointed out the sound laws which in their scope and clarity appeared as the first revelation from a world of regularity, the sound laws which we now group together under the name of the Germanic consonant shift; he discusses these on p. 169:

\[
\begin{align*}
\text{p} & > \text{f}: \text{παθρ, Old Norse } f\ddot{a}dr \hfill \\
\text{t} & > \text{b}: \text{τρες}, \text{brír} \hfill \\
\text{k} & > \text{h}: \text{ορνυ, horn} \hfill \\
\text{d} & > \text{t}: \text{δαμῶ }'\text{I tame'}, \text{tamr 'tame'} \hfill \\
\text{g} & > \text{k}: \text{γυγ, kona} \hfill \\
\text{φ} & > \text{b}: \text{φέρω, bera} \hfill \\
\text{θ} & > \text{d}: \text{θόρα, dyrə} \text{ (on Latin } \text{for}s \text{ p. 288)} \\
\text{χ} & > \text{γ}: \text{χέω, χυτός, } \text{gjóta} \text{ (Latin } \text{hosti}s, \text{ Old Norse } \text{gestr p. 283)}.
\end{align*}
\]

That Rask was aware of the full significance and extent of these sound laws is clearly evident throughout his discussion. Indeed, as noted above, he mentioned as a criterion for linguistic kinship precisely "that one can formulate rules for changes in the letters", and on p. 161 he stresses in the introduction to the section on Greek and Latin that in [54] comparing these two languages with "the Gothic and Lithuanian" language branch "the knowledgeable investigator discovers not only a group of words which resemble each other somewhat in form and meaning, but such a quantity of similar-
Rask's Significance in Phonological Study

Ities that rules for the changes in the letters can be formulated and nearly the entire linguistic structure shown to be the same in both". The actual presentation of the sound shift is quite concise; it hardly occupies a page. It consists of an outline like the one indicated above with a number of additional examples and with the addition of a remark stating that within the word other changes occur:

$$k > g: \text{Latin} \, \text{macer, Old Norse} \, \text{magr}; \text{taceō 'I am silent'},$$
$$\text{Old Norse} \, \text{þegja, þagða}$$
$$t > ð: \text{pater, faþir; fráter, brðýr}.$$  

This remark is slightly incomplete. But everything Rask says concerning the sound laws is still correct, and the presentation is so sound and clear that it was not until the 70's (with Verner's law) that it was surpassed. Only on one point was Rask confused, the original b; it actually follows the same direction as d and g, thus becoming the voiceless sound p in Germanic. But b was a very rare sound in Indo-European; it rarely occurred word-initially, and Rask did not find good examples; although he, on p. 281, makes the comparison between θάυμας: hampr, he states on p. 169 that b "is often retained" and cites several incorrect examples (βλαστάω 'I sprout', Old Norse blað etc.). And Rask did not have the idea that sound laws have no exceptions; thus now and then he can be seen assuming other correspondences than those indicated in the outline (by failing to recognize a loan word or being led astray by a chance similarity). Naturally Rask was also aware of the High German sound shift; he mentions it briefly on pp. 68-69 giving these examples:
pf from p: Kupfer, Pforte
z from t: Zoll, zittern, setzen, reisen (on -s from t
p. 189: Gothic godata 'good', German gutes).
ch from k: weichen [55]

This too is correct, but admittedly somewhat incomplete;
In his Undersøgelse Rask generally shows little concern for
German; Old Norse (Icelandic) took precedence for him over
all other Germanic languages.

Undersøgelse
om
det gamle Nordiske
eller
Islandske Sprogs
Oprindelse.

Et af det Kongelige Danske Videnskabernes
Selskab konet Prisskrift,

forsatdet
af

R. K. Rask,

Anden Bibliotekar ved Kjøbenhavns Universitets-Bibliothek.

Kjøbenhavn.

Paa den Gyldendalske Boghandlins Forlag.
Trykt hos H. F. Popp.
1818.
The great extent to which Rask's presentation distinguishes itself from the remarks of Bopp and Grimm on this subject cited above needs hardly to be expressly stated. And that which Rask perceived was not lost. In the second edition of the first volume of his *Deutsche Grammatik* (1822) Grimm had become aware of the independent significance of phonology; he devotes a section of 595 pages to it in which much of value for historical phonology (in particular the history of the vowels in the Germanic dialects) is to be found and provides full tables of phonetic comparisons (pp. 573-578). In these new surroundings the laws discovered by Rask for the development of the stops have also been included under the heading which they have retained ever since: the Germanic sound shift. Grimm presents a long list of examples not only those found in Rask but also many others, most of them correct. In reality he surpassed Rask merely on one point: he recognized that $b$ was not found word-initially in Indo-European and that this is the reason why $p$ is originally absent initially in Germanic; and he correctly assumes that $b$ wherever else it was to be found (medially) must have become Germanic $p$: Ṣ.VALUE, Old Norse *hampr*, OHG *hanaf* (the remainder of Grimm's examples of $b > p$ are wrong). Regarding the exceptions to the main rules in medial position which Rask merely ascertained briefly (*maer: magr; pater: faðir*), Grimm is by no means clearer than Rask. From his position he has access to a larger body of material, since many important aspects in the development only appear outside of the Nordic languages, which were Rask's primary concern, and are most evident in German (Old High German). Thus, there was originally an alternation in medial position between $d$ ($ð$) and $p$ which has been obliterated in the Nordic languages:
But in Nordic intervocalic \( b \) became \( ð \); thus:

Old Norse \( fãðir \) mðir brðir.

The distinction has been preserved in German to this very day; in English the distinction has actually been preserved (\( d : ð \)) too, but not in the three examples cited here in which it has been neutralized by a change from \( dr \) to \( ðr \) such that all three words have \( ð \):

\( fath êr \) modor brðor.

For Rask, whose point of departure was Icelandic, \( fãðir \) and \( brðir \) had to appear equally irregular. Grimm, however, had to see the difference; but he knows no other explanation than the question (p. 590 footnote) of whether the Greek forms should have been \( *παυρ\), \( *μήυηρ\), a possibility which he nevertheless discards in view of Sanskrit, where all of these words have \( t \) as in Greek and Latin:

\( pîtû \) mûtû bhrûtû.

From the beginning there was a similar alternation between \( b \) and \( f \) in medial position, which was also obliterated in Old Norse, where both sounds became \( [v] \), written \( f \); likewise in Old English. Concerning this Grimm merely says that the Gothic \( b \) is "ungenauer" than the Nordic \( f \) (p. 585; in spite of the fact that Grimm really knew that Old Norse \( f \) in medial and final position did not represent \( [f] \)). He therefore cites Old Norse \( jõfurr 'prince' \), Old English \( eofor \).
'boar' as regular along side of Latin *aper* 'wild boar' in spite of OHG *ebur*.

And finally, Grimm whisks away the third instance of irregularity, *g* instead of *h* in medial position, by saying that the Germanic correspondence to Greek and Latin *k* actually should be *x*, but since this sound is absent Gothic sometimes resorts to *h*, sometimes to *g*. As if the Gothic sound system with this "lücke" [gap], which consists of the lack of *x*, can be assumed prior to the sound shift! did Gothic possess *h* (and *p* and *f*) prior to the sound shift? [57] Grimm employs the same reasoning on the gradual shifting of sounds within the language itself as might be fitting in instances where attempts are made to appropriate a difficult sound from a foreign language. Here there is an extreme lack of clarity which is truly no advancement.

Grimm's discussion of the High German sound shift is naturally much more detailed than Rask's brief comment on this subject. And since German is Grimm's very own area, it would not be unfair to expect him to excell here. But the Old High German dialectal diversity was too complex for him; he was incapable of dealing with it. And he also commits quite unnecessary errors. Thus, already in the following table on the High German sound shift there is a mistake:

<table>
<thead>
<tr>
<th>Gothic</th>
<th>OHG</th>
</tr>
</thead>
<tbody>
<tr>
<td><em>t</em></td>
<td><em>z</em> (tains 'branch', <em>zain</em>)</td>
</tr>
<tr>
<td><em>d</em></td>
<td><em>t</em> (dal, <em>tal</em> 'valley')</td>
</tr>
<tr>
<td><em>p</em></td>
<td><em>d</em> (þaurne 'thorn', <em>dorn</em>).</td>
</tr>
</tbody>
</table>

The shift of *p* to *d* is not a part of the High German sound shift which separates High German from both Low German and Dutch; it is a later change which is not limited to High German nor to German exclusively but which extends over both the German and the Dutch area. In addition Grimm incorrectly connects *v* for *f* (*filu*, *vihu* 'much') with the High German...
sound shift and even maintains that \( v \) has spread from High German to the Low German and Dutch area (p. 490), which is as incorrect as it could possibly be (\( v \) in Dutch, for example in *vader* 'father', is a peculiar semi-voiced sound just like \( z \) in *soon* 'son').

Grimm's discussion of the Germanic sound shift is thus no more advanced than Rask's; to be sure, Grimm makes use of a much larger body of material than Rask when dealing with the High German sound shift, but he is unable to master it. We are then merely left with the question of whether Grimm more clearly than Rask perceived the great significance of these laws. This is, however, by no means the case. For Rask these laws were a corner-stone in the proof of the relationship between Græco-Latin and Germanic; Grimm [58] placed special emphasis on the significance of these laws for strict etymology ("streng der etymologie" p. 584) and gives a few examples to show that cases which do not agree with the sound shift are either due to loan or to purely accidental similarity (p. 588), but in reality he completely invalidates the methodological consequences of this by stating on p. 590: "Die lautverschiebung erfolgt in der masse, thut sich aber im einzelnen niemahls rein ab; es bleiben wörter in dem verhältnisse der alten einrichtung stehn, der strom der neuerung ist an ihnen vorbeigeflossen", whereupon he then cites a long list of examples supposedly showing that the sound shift or both sound shifts did not take place. This explicit pronouncement concerning the unpredictability of the development is significantly worse than Rask's tacit assumption of certain exceptions; silence at least does not exclude the notion of specific conditioning, even if this idea hardly occupied Rask to any particular extent.\(^20\)

After this there can be no doubt that Bopp, *Vergleichende Grammatik* I\(^1\) [1833] p. VII, is misleading when he says: "Wir verdanken ihm (Rask) eine Andeutung des von Grimm schär-
fer aufgefassten und gründlich entwickelten Consonanten-Verschiebungs-Gesetzes". It is not a question of greater precision but of greater breadth. And since it is certain that Grimm read Rask's considerations on this subject at a time when he himself was completely in the dark, it is not correct to refer to the rules for the sound shift as "Grimm's law". It is Rask's law. And Rask deserves to be recognized as the first Indo-European comparativist to have an eye for the significance of sound laws.

DEUTSCHE GRAMMATIK

VON

DR. JACOB GRIMM

KURHESS. BIBLIOTHECAR, MEHR. GEZ. GES. MITGL.

ERSTER THEIL; ZWEITE AUSGABE.

GÖTTINGEN

IN DER DIETERICHSCHEN BUCHHANDLUNG.

1822.
13. August Friedrich Pott

As the next chief representative for the inevitably growing significance of phonology within the field of Indo-European linguistics we should mention Pott. Pott’s major work, [59] *Etymologische Forschungen auf dem Gebiete der Indogermanischen Sprachen* I-II, 1833-36, is (in the first edition) not, as one might expect, an etymological dictionary or a collection of etymologies but a comparative Indo-European grammar with special emphasis on phonology. If we disregard Pott’s unavoidable digressions, the contents of his first volume can be said to contain: 1) a systematic treatment of vowels and consonants (144 pages in very small print); 2) a comparative presentation of 375 verb roots; the second volume contains: 1) a section on what we now term combinatorial sound changes; 2) derivational and inflectional morphology (the morphology is brief because of Bopp’s prior treatment).

Already the title page for volume I points in the direction characteristic for Pott. It bears the motto “Literae suus honos esto; litera animi nuntia”. When we recall that “letter” was used to mean “sound” by the first comparative linguists, it becomes clear that this motto is a call to arms; Pott anticipates that his preoccupation with sounds will be criticized as being an overwhelmingly dull pursuit unworthy of a philologist. And the introduction to the book begins with a figurative and impassioned protest against the application of the phrase “The letter kills, but the spirit gives life” as a condemnation of phonological studies. That the concept of sound laws is now beginning to become clear in linguistics is evident in several of Pott’s felicitous remarks. He warns (I, p. XII) against “die Sirene des Gleichlauts”; what matters is actually not identical sounds of words but the agreement according to definite laws in these
words, "der etymologische Parallelismus der Buchstaben" (p. 73). The complete identity of sounds can be accidental (Persian beh-ter 'better' has nothing to do with English better, p. 70), and conversely, etymological agreement is often not at all directly conspicuous (Persian x\^a\text{h}er 'sister', Ossetic xo: English sister, p. 71). Furthermore (p. 73) Pott stresses the historical nature of the sound laws, which previously had often been overlooked: "Der erwähnte Lautparallelismus ist ein geschichtlich Gegebenes und muss daher auch nach den Gesetzen der Geschichtsforschung aufgesucht und erforscht werden". Thus one cannot be content with merely [60] ascertaining the relationship between two sounds, for example s and r, but must pay attention to chronology; in Latin r often came from s, but s never from r. Pott stresses repeatedly (for example II 349) that phonology is the surest and most important key to etymology.

However, Pott, like his predecessors, has no idea that the sound laws operate without exception. Thus, along with much useful material for historical phonology we also find some things which are quite worthless. Pott contains nothing on reconstruction of the Indo-European phonological system; in his tabular survey of the consonants (I, pp. 82-83) he relies entirely on the Indic phonological system as starting point with the sole reservation that the retroflex sounds (t etc.), viewed as non-original according to a remark on p. 78, are not drawn into the comparison.21

And where phonetics is concerned Pott also stands taller than Grimm; his ideas of the nature of sounds are correct; but he is emphatically reluctant when it comes to making exaggerated claims in this direction: "Bei der Vergleichung verwandter Sprachen ist für uns die Kunde der etymologischen Uebereinstimmung der Laute in verwandten Wörtern und Formen Hauptsache, nach deren Erlangung wir eifrig streben müssen, die der phonetischen dagegen mehr ein Sumendum, das wir,
wenn es sich uns darbietet, dankbar annehmen, ohne darauf ein so grosses Gewicht zu legen, als auf das zuerst genannte, dem Sprachforscher durchaus unentbehrliche Gut. Ich habe mich ein wenig schroff ausgedrückt, aber es ist mit Fleiss geschehen, um jenen Klanghäschern und Ohraufpassern unter den Etymologen ihr elendes Banausengeschäft möglichst zu verkümmern" (I 69, cf. I 79). When Pott maintains here that, if necessary, the linguist can do without the precise phonetic understanding of the sounds he identifies etymologically (and thereby also do without the precise phonetic understanding of the posited sound changes), it cannot be denied that one is often forced to work under these conditions; but precise phonetic understanding is by no means merely a sumendum, which can be accepted if it accidentally presents itself; it is most definitely a quaerendum, upon which the continued progress of linguistics will depend.
14. GEORG CURTIUS AND AUGUST SCHLEICHER

It cannot be my task here to indicate step by step how understanding of the phonetic development in the Indo-European languages gradually increased and how as a result the suspicion of regularity in phonetic development gradually became formulated more precisely. The two scholars who can be said to conclude the first period of comparative linguistics, Curtius and Schleicher, announce as their platform "strengste Beobachtung der Lautgesetze", "strenges festhalten an den lautgesetzen"; both contributed to a certain extent toward furthering development in this direction, but neither of them acquired a complete understanding.

Curtius distinguished between "regelmässige oder durchgreifende Lautveränderungen" and "unregelmässige oder sporadische Lautveränderungen"; here at least a limit on arbitrariness had been set.

Of far greater significance than Curtius was Schleicher, whose Compendium der vergleichenden grammatik der Indogermanischen sprachen, Weimar 1861-62, replaced Bopp's already out-dated Vergleichende Grammatik. Schleicher's Compendium is an excellent book, clearly and concisely written, and providing what can be characterized as a good summary of the results in the field up to that time. The particular point on which Schleicher himself contributes toward the progress of method is in his deliberate and precise reconstructions.

On the basis of a comparison between the surviving Indo-European languages he reconstructs both the phonological system and the individual words and forms of the parent language. The need for such reconstruction had actually already been pointed out by Benfey in 1837 in his review of Pott's [62] Etymologische Forschungen (Kleinere Schriften I 2, pp. 5ff.; an example p. 8); but Schleicher is the first to carry out the method in practice (the practice of placing an asterisk
in front of the reconstructed forms also goes back to Schleicher). He introduces this procedure in the first edition of his Compendium completely without comment; in the second edition on p. 8 there is a rather lengthy note on the justification of the procedure: "Im vor ligenden werke ist der versuch gemacht worden die erschloßene indogermanische ursprache neben ire wirklich vorhandenen ältesten tochersprachen zu stellen. Außer dem vorteile, den disse einrichtung dadurch bietet, daß sie dem lernenden sofort die letzten ergebnisse der forschung in concreter anschaulichkeit vor augen stelt und im so die einsicht in das wesen der einzelnen indogermanischen sprachen erleichtert, wird noch ein zweiter, wie mich bedünkt, nicht ganz unwichtiger zweck durch die selbe sicher erreicht. Es wird nämlich so der augenfällige beweis gelifert für die völlige grundlosigkeit der noch immer nicht ganz verschollenen anname, daß auch die nicht indischen indogermanischen sprachen vom altindischen (sanskrit) ab stammen": Both of the reasons cited are correct and to the point. The reconstruction of forms considered as original is really the shortest method of showing later changes in each of the individual languages; and this method also has the great advantage of making it clear once and for all, also to beginners and outsiders who otherwise might easily acquire the childhood diseases of the science, that none of the languages compared is viewed as the source of the others. In addition - leaving a number of questions concerning the method of reconstruction aside as irrelevant in this connection - I must stress the fact that the need for reconstruction compels the linguist to give his complete attention to every detail in the phonetic development. This entire method and likewise most of the factual material contained in Schleicher's phonology, which makes up the first volume of his Compendium, impress us as being extremely modern. It is only slightly marred by the fact that the philosophy with which
Schleicher more than any other [63] of the more significant linguists was preoccupied can betray him in places. For the parent language Schleicher assumed three vowels (a, i, u), three sonorants (n, m, r), three spirants (j, v, s) and three sets of three stops (k, g, gh; t, d, dh; p, b, bh) and commented expressly: "Man übersehe nicht diese eigentümlichen zahlenverhältniss in der anzal der laute." Of course we can try to convince ourselves that Schleicher arrived at his phonological system honestly and that the philosophical joy over the triads first appeared afterwords; we could, for example, excuse him for not seeing that the Indo-European parent language had no v but merely a w and that neither j nor w were spirants. But we become somewhat uneasy in noting his re­tention of all of these triads in the second edition (1866), even after Grassmann in 1863 had shown that the voiceless as­pirates kh, th, ph, should also be added to the parent lan­guage, something which should have transformed Schleicher's last three triads into quadruplets; was it not under the in­fluence of his philosophy that Schleicher rejected Grass­mann's correct discovery?

However, this is rather insignificant. But that Schleic­cher no more than Curtius had overcome previous misconcep­tions concerning the arbitrariness of phonetic development is most obvious when we move from his relatively modern pho­nology to his morphology. Here we see that Schleicher's pho­netic conscience did not stop him from incorporating some of Bopp's worst blunders. Schleicher II 402 still accepts Bopp's explanation of -lif in Gothic twa-lif '12' and -lika in Lithuanian dvý-rika '12' as stemming from the numeral '10' (Sanskrit dáqa Greek ὀξα Latin decem Gothic tainun Lithua­nian dešimt); he still believes that -ns- and -sv- in Gothic unsar 'our', izvar 'your' corresponds to Sanskrit -ṣr- (II 501); without blinking an eye he proclaims II 406 that Gothic puzzandi '1000', Old Bulgarian tysišta, Lithuanian tūškstantis
is a compound of the numeral '10', supposedly distorted into *tu-*, *tuk-*, and the numeral '100', supposedly distorted into *santi*; all this is "ein unregelmässig verändertes gebilde, das sich den gewönlichen lautgesetzen entzogen hat"; in Schleicher's phonology there is, of course, no indication at all that an original *d* could become *t* in Balto-Slavic [64] and *þ* in Gothic or that an original *k* could become *s* in Gothic. Because in his phonology Schleicher deals only with those phenomena he feels can be accounted for by rules; but he also recognizes other phenomena. The Old Adam has been exiled from the phonology but still frolics rather freely in the morphology.

Not until the great discoveries in the 70's did this state of affairs come to a halt.

**COMPENDIUM**

**VERGLEICHENDE GRAMMATIK**

**INDOGERMANISCHEN SPRACHEN.**

*vom*

**AUGUST SCHLEICHER.**

WEIMAR

HERMANN BÖHLAU

1881.
15. Linguistic Discoveries of the 1870's

The reason that no clear notion of the regularity of phonetic development had been achieved prior to Schleicher lay in the nature of the material, a collection of individual results, available for study. To be sure, this material provided numerous examples of regularity, but beside these at every turn were the most striking irregularities. In instances where it seemed reasonable to assume an Indo-European k, Sanskrit sometimes had k, sometimes ć, sometimes q (Ʌ), Latin sometimes c, sometimes qu, Greek sometimes κ, sometimes π, sometimes τ; everything seemed chaotic; Sanskrit κ, ć and q could all correspond to Latin c, κ, π and τ could all correspond to Sanskrit k etc. No one knew why f and b, Ʌ and d, h and g alternated in Germanic (see above pp. 55f.). And the vowels were hopeless; Schleicher set up three Indo-European vowels a, i, u; i and u presented no real problems, they performed pretty much like š and Ʌ in all of the oldest languages. But all the more complicated was the situation surrounding Schleicher's Indo-European short a; it appeared in the individual languages as a, c, u, e, i, and apparently in a mess at that; it was rather difficult to see any regular correspondence. A slight impression of how jumbled everything looked can be gained from a glance at the following chart in which, taking advantage of today's insight, I have arranged the varying vowels in four columns; those placed in the same column can correspond with each other (the a in the first, second and fourth columns, which I have placed in parentheses where Latin is concerned, is a still unsolved problem, which today, [65] however, since it has been isolated, can no longer confuse the overall picture; I refer to instances like quattuor, mare, manœð):
In order to fully understand how confusing this whole profusion was, we must imagine the deletion of the instructive division into columns; the result then is that everything corresponds pretty much with everything else. If you noticed that *e* in Latin *ferõ* corresponded with *e* in Greek *φέρω*, a moment later you were confronted with *tenuis*: τανυ- or *centum*: ἐ-κατόν or conversely *quattuor*: τέσσαρες; if you had noticed that Latin *o*, *u* corresponded with Greek *ο* in *hortus* 'garden': χόρτος 'enclosure' or in *sulcus* 'furrow': δόλχος, a moment later you could be confronted with cornus 'cornel tree': κῶδυς or *ursus* 'bear': ἄρωτος (in Sanskrit *ṛkṣa-s* with no vowel at all).

All this chaos with respect to consonants and vowels was clarified in the course of the 70's by a series of brilliant discoveries on the part of the Italian Ascoli, our fellow countrymen Vilh. Thomsen and Verner, the French Swiss Ferdinand de Saussure and the German Brugmann. It became evident that most of the difficulties lay in the incorrectly established starting points for the development; the reconstruction result was wrong. Schleicher had thought he could lay the ghost of the overestimation of Sanskrit, bury it and drive a sound stake through it with his method of recon-

<table>
<thead>
<tr>
<th>Indo-European (Schleicher)</th>
<th>a</th>
<th>a</th>
<th>i, a</th>
<th>i, u, a zero</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sanskrit..................</td>
<td>a</td>
<td>a</td>
<td>i, a</td>
<td>i, u, a zero</td>
</tr>
<tr>
<td>Greek.....................</td>
<td>e</td>
<td>o</td>
<td>a</td>
<td>α(dialectal)</td>
</tr>
<tr>
<td>Latin.....................</td>
<td>e, i (a)</td>
<td>o, u (a)</td>
<td>a</td>
<td>e, i, o, u (a)</td>
</tr>
<tr>
<td>Celtic....................</td>
<td>e</td>
<td>o</td>
<td>a</td>
<td>e, i, a</td>
</tr>
<tr>
<td>Gothic....................</td>
<td>i</td>
<td>a</td>
<td>a</td>
<td>u</td>
</tr>
<tr>
<td>Old Norse, Old English, Old High German</td>
<td>e, i</td>
<td>(or umlaut)</td>
<td>(or umlaut)</td>
<td>(or umlaut)</td>
</tr>
<tr>
<td>Lithuanian...............</td>
<td>e</td>
<td>a</td>
<td>a</td>
<td>i</td>
</tr>
<tr>
<td>Slavic...................</td>
<td>e</td>
<td>o</td>
<td>o</td>
<td>e, i, zero</td>
</tr>
</tbody>
</table>
struction, but it became evident that the ghost now sat in
the stake itself. His reconstructions [66] did not rest up-
on a sufficiently precise comparison of the individual lan-
guages; they had been dominated by Sanskrit together with a
naive conception of the primitiveness and great simplicity
of the parent language.

Ascoli opens the series of discoveries, attacking in
1870 in his Corsi di glottologia the $k$ problem which neither
Bopp nor Schleicher had been able to solve because neither
of them dared to venture the thought that Sanskrit, in this
instance, was far removed from the original language and not
at all transparent, if not with respect to the voiceless
sounds at least with respect to the voiced sounds (the $g-$
and $gh$-sounds). Ascoli had this thought and proved it. And
subsequently it became clear to him that there was not mere-
ly one $k$-series in the parent language ($k, g, gh$) but a to-
tal of three different series; $k$ and $q$ in Sanskrit reflected
a phonetic distinction which derived from the parent lan-
guage; $\check{c}$ and $qu$ in Latin (which corresponded to Greek $\varsigma$ and
$\pi$) reflected another original phonetic difference. But on
one point Ascoli overshot his mark; he thought that San-
skrit $\check{c}$ also reflected a shade in the parent language, the
same shade which had given rise to Latin $qu$. This was wrong,
and with this mistake the calculations went haywire.

In reality Sanskrit $\check{c}$ is due to an Indic sound law
which, however, could not be seen unless one dared to assume
that the vowel system in Sanskrit was not at all primitive
and that the Latin $e$ in $que$ 'and' was older than Sanskrit's
$a$ in $\check{a}na$ 'and'. Vilh. Thomsen was the first to venture this
revolutionary thought; later the same discovery ("the law of
palatals") was made by Verner, by Esaias Tegnér in Lund, by
Saussure and by the two German linguists Collitz and Johan-
nes Schmidt; the discovery was reported in print in 1878.

But before this discovery was published, the German
Fick managed to successfully botch up Ascoli's system. However, all was not lost; at least the separation into two $k$-series was retained (the two series which could partly be separated with the help of Sanskrit). Indeed, this was better than nothing and sufficed until 1890 when Fick's student Bezzenberger, and at the same time Hermann Osthoff and Sophus Bugge, once again [67] pointed out the necessity of assuming three series. But the three-series-system is Ascoli's discovery, not Fick's and Bezzenberger's.

In 1875, before the law of palatals had been made public, Verner published his famous article *Eine ausnahme der ersten lautverschiebung*. We have mentioned above (pp. 55f.) the irregularities in the Germanic sound shift which appear in medial position, citing German *Vater: Bruder* (Gothic *fadar: brôpar*) as examples. This, however, is merely a single example; these irregularities appear abundantly in the Germanic vocabulary and prevail throughout the inflection of the "strong" verbs right from the start; this is seen most clearly in Old High German, but it can also be observed in the other ancient Germanic languages (Old High German *slahan* 'to strike', *gislagan* 'struck', Old Norse *slá*, *sleinn*; Old High German *findan* 'to find', *funtan* 'found', Old Norse *fína* [nn from *np*, *fundinn*]; cf. New High German *ziehen, gezogen, schneiden, geschritten, sieden, gesotten*); and in the same manner $s$ alternates with $r$ (Old Norse *kjósa* 'to choose', *korinn* 'chosen' etc.). This was indeed an irregularity which of necessity had to affect scholars dealing with the ancient Germanic languages like a nose full of mustard powder, but no one had been able to explain it. For this reason the effect of Verner's article, in which, to everyone's surprise, the solution to the problems was clearly given, was immeasurable. Now there were no longer any exceptions to the Germanic sound shift, and this absence of exceptions necessarily had the the same great effect upon the whole conception of
linguistics as the major laws of the sound shift had had in their time. Back then the notion of sound laws was awakened, now the recognition dawned that phonetic development is perfectly regular. And the underlying explanation discovered by Verner could only contribute to this: the cause was the original Indo-European accent which was preserved in Sanskrit but lost in Germanic in prehistoric times. It gave an impression of how delicately one had to scrutinize and how deep one had to delve in order to fully understand the phonetic development. It was a forceful warning not to regard any hitherto incomprehensible development as a mere whim of language. [68]

The following year (1876) Brugmann wrote an article entitled *Nasalís sonans in der indogermanischen Grundsprache* in which he maintained that there must have been syllables without vowels in the parent language of our language family, syllables in which an *n* or an *m* made up the syllable; similarly, he assumed syllables with *r* (?) as syllabic nucleus. Examples, written according to current conventions, are:

\begin{itemize}
  \item *\textit{tē}-\text{tō}-\text{ς} = \text{Greek} \text{ τα}-\text{τό}-\text{ς}, \text{ participle of τέλνω}\
  \item *\textit{k}′\text{μτό}-\text{m} \ 'hundred', \text{ Greek} \text{ ε}-\text{κατόν}\
  \item *\textit{e}-\text{δρκ}′\text{om} \ 'I saw', \text{ Greek} \text{ έ}-\text{δρακον}, \text{ Sanskrit} \text{ a}-\text{dr̥gam}\
  \item *\textit{υλκ}′\text{ο}-\text{ς} \ 'wolf', \text{ Sanskrit} \text{ vr̥ka}-\text{ς}, \text{ Gothic} \text{ wulf}-\text{ς}.
\end{itemize}

Setting up a syllabic *r* in the parent language was not entirely new. This sound was, of course, preserved in Sanskrit (for that matter also in the oldest Slavic languages and in some of the Slavic languages spoken today), and there had been hints from various quarters that it belonged to the parent language (from Benfey in 1837, from Ebel in 1852). Naturally Schleicher did not include this view in his \textit{Compendium}; it was too much in conflict with his theories; indeed, *r* looks like a weakening of \text{vowel} + \text{r}, and according to
Schleicher no weakening had taken place in the parent language; the language knew only "das wort in allen seinen teilen noch vollkommen unversert" (Compendium I 10). In the 70's several scholars had touched on the question again (G. Humperdinck in 1874 in a school report from Siegburg, the famous Slavic scholar Miklosich in 1875, Brugmann's friend Osthoff in 1876). But the idea of a syllabic n and m was quite new (Saussure made the same discovery while he was still in school). - In many respects Brugmann's article can be criticized; it was anything but classical in its form, and for this reason it makes for very heavy reading today (in this respect it contrasts strongly with Verner's article); it must be admitted, too, that the conclusive proof which can be adduced for the correctness of Brugmann's view is actually not to be found in his article. Nevertheless he had perceived the truth and presented it in a form which could seem convincing to others. And the significance and scope of his discovery can scarcely be overestimated; with one [69] blow it removed all the irregularities indicated in the fourth column of the vowel table on p. 68. After that it was not difficult to sort out the remaining material (the first three columns in the table) and to see that they required the setting up of three Indo-European vowels, e, ø, a.

This survey should not conclude without mention of Saussure's brilliant work: Mémoire sur le système primitif des voyelles dans les langues indo-européennes, Leipzig 1879. It contained a discussion of the vowel alternations which had been present in the Indo-European parent language (that which Grimm had referred to with the unfortunate term ablaut). This book (which, among other things, inevitably put an end to Schleicher's view that no weakening had taken place in the parent language), however, points far beyond the period which concerns us here.

After all these discoveries the picture which met the
linguist's eye in his exploration of the ancient Indo-European languages was quite different from the previous one. Before irregularity everywhere, now the most conspicuous regularity. The material now induced scholars to assume complete adherence to laws in phonetic development and to seek an explanation for every deviation from the usual.

MÉMOIRE

SUR LE

SYSTÈME PRIMITIF DES VOYELLES

DANS LES

LANGUES INDO-EUROPÉENNES

PAR

FERDINAND DE SAUSSURE.

LEIPSICK
EN VENTE CHEZ G. TEUBNER.
1879.
16. Wilhelm Scherer

Older than all the works mentioned in the previous section is Wilhelm Scherer's book *Zur Geschichte der deutschen Sprache* (Berlin 1868) which also contributed to a certain extent to the creation of the new period in linguistics. It contains a series of articles on various aspects of phonology (Der Ablaut, Grimm's Gesetz, Die germanischen Auslautgesetze) followed by consideration of a series of morphological problems (Zur Conjugation, Das Personalpronomen, Die Pronominalflexion, Die Nominalflexion, Numeralia und Adverbia). Much of the book's content is nothing but sheer fantasy (the attempt to explain the phonetic development of the individual languages on the basis of the spiritual make-up of the respective nations; the attempt to explain the origin of the inflectional endings); actually, there is very little in Scherer's book which can be of use to us today. But it made a strong impression on his contemporaries, among others on both Verner and Brugmann. We find it more difficult to continue the admiration of his contemporaries; this was probably evoked primarily by the vigorous energy with which Scherer liberated himself from the shackles of Schleicher's philosophy and drew on the living source of experience. We find him consulting phonetics (Brücke) on every question involving the history of sounds, and he denounces most emphatically Schleicher's distinction between a prehistoric period of linguistic creativity and a historical period of linguistic decay: "Ich vermag keinen anderen Unterschied zwischen Vorhistorisch und Historisch zu erkennen als die wesentlich andere Beschaffenheit der Quellen und die entsprechende stärkere oder geringere Beteiligung des combinirenden, construierenden Subjects". Thus without hesitation Scherer also applied certain principles to explain the older stages of language which had been avoided previously because
they smelled of the linguistic "decay" of historical times; one example is the assumption of analogical formation, "false analogy" as it was called at that time.

One very instructive example is to be found in the beginning of Scherer's section on morphology. Here he is concerned with the first person present verb endings. In Greek there is a very distinct opposition between verbs in -ω and verbs in -μι, but in Sanskrit all verbs end in -ми:

φέρω εἰμι εἶμι δίωμι
bharāmi asmi ēmi dadāmi

Schleicher proceeded from the apparently more transparent Indic forms and assumed that Greek φέρω arose through the loss of -μι (once again a view which could only be an obstacle to the recognition of the regularity of phonetic development). Scherer, [71] on the other hand, shows that the Greek distinction between verbs in -ω and verbs in -μι encounters us again in all the European languages as well as in the most ancient portions of the Avesta and must therefore be original (from which it naturally follows that Sanskrit bharāmi is not the primitive form); he shows how the -μι-type spread in Old High German and Slavic by "false analogy" at the expense of the -ṃ-type; it is quite clear that he also considers Sanskrit bharāmi a product of false analogy. And he is obviously correct; and it is easy to understand that this new view would have a liberating effect upon phonology: one no longer had to assume an extremely irregular and unmotivated loss of the syllable -μι.

In general we find that the more the progressing science opened its eyes to the great significance of the principle of analogy in linguistic development, the more evident the regularity in the purely phonetic development became. The theory of the great frequency of analogical formation in
all linguistic periods is the precursor of the theory that sound laws are without exceptions. In his book *Zur Geschichte der deutschen Sprache* Scherer took only the one step (just how far he was from the complete understanding of the regularity of phonetic development is evident, for example, in his deliberations on pp. 107-12). But in the atmosphere of the 70's the idea that sound laws had no exceptions would naturally assert itself; Scherer announced this theory in 1875 (*Preussische Jahrbücher* XXXV 107): "Die Veränderung der Laute, die wir in beglaubigter Sprachgeschichte beobachten können, vollzieht sich nach festen Gesetzen, welche keine andere als wiederum gesetzmässige Störung erfahren".

Verner had said something similar in 1872 in a letter to C. W. Smith by suggesting that the ancient proposition *nulla regula sine exceptione* be reversed to "*nulla exceptio sine regula*": every exception to the rules operating in a language must have a cause" (Vernerbogen p. 148) [Verner 1903]. The new ideas were in the air. And just as one before had passed from Bopp's wild phonetic practice via Rask, Pott etc. into Schleicher's period of laws with frequent, unmotivated dispensations, likewise it would be reasonable to assume that it was possible to pass once more from [72] Schleicher's practice into our present method which obliges the scholar to seek an explanation for each and every irregularity in the phonetic development. But things happened differently. The last act did not transpire without clamor and rumbling.
17. Methodological Discussions of the 1870's and 1880's

Brugmann's article *Nasalis sonans in der indogermanischen Grundsprache* appeared in volume IX of the periodical edited by Curtius, *Studien zur griechischen und lateinischen Grammatik*. Brugmann had just become co-editor with volume IX, and Curtius was away when the issue in question was edited and printed. When Curtius got a look at Brugmann's article he must have become quite angry; for he added a statement at the close of the volume to the effect that due to his absence from Leipzig he had not been able to state his reservations regarding his co-editor's articles: "Ich muss daher die Verantwortlichkeit für seine weit greifenden Combinationen ihm allein überlassen". Only one volume of Curtius' *Studien* appeared after that, with the names of Curtius and Brugmann on the title page but without any contribution from Brugmann; Curtius decided "mit bedauern" to cease publication of the periodical. It was replaced by *Leipziger Studien*, published by Curtius together with L. Lange, O. Ribbeck and H. Lipsius. In the time to follow we find Brugmann contributing industriously to [Kuhn's] *Zeitschrift für vergleichende Sprachforschung*, but in addition, together with Osthoff, he started to publish a sort of periodical, *Morphologische Untersuchungen*, containing exclusively articles by the two editors. The first volume (1878) is introduced with a preface, which is a kind of methodological platform for the new school in linguistics. Here we find among other things, the often quoted passage: "Nur derjenige vergleichende sprachforscher, welcher aus dem hypothesentrüben dunstkreis der werkstätte, in der man die indogermanischen grundformen schmiedet, einmal heraustritt in die klare luft der greifbaren wirklichkeit und gegenwart, um hier sich belehrung zu holen über das, was ihn die graue theorie nimmer [73] erkennen lässt, und nur derjenige, welcher sich für immer lossagt von
jener früherhin weit verbreiteten, aber auch jetzt noch anzu­
treffenden forschungsweise, nach der man die sprache nur auf
dem papier betrachtet, alles in terminologie, formelwesen,
und grammatischen schematismus aufgehen lässt und das wesen
der erscheinungen immer schon dann ergründet zu haben glaubt,
wen, man einen name für die sache ausfindig gemacht: -
nur der kann zu einer richtigen vorstellung von der lebens-
und umbildungsweise der sprachformen gelangen und diejenigen
methodischen principien gewinnen, ohne welche man überhaupt
bei sprachgeschichtlichen forschungen keine glaubwürdigen re-
sultate erreichen kann und ohne welche im besonderen ein vor-
dringen in die hinter der historischen sprachüberlieferung
zurückliegenden zeiträume einer meerfahrt ohne compass
gleicht".* Wilhelm Scherer is mentioned as the one deserving
praise for having initiated a better method. A number of
linguists, however, had left the new method unheeded or
pushed it aside as something uncomfortably disturbing to
their accustomed practices. Among others, particularly
younger scholars, Scherer's words had fallen upon fruitful
soil; the preface mentions, for example, Leskien, Osthoff,
Paul and Brugmann, continuing afterwards to clarify more ex-
plicitly "die methodischen grundsätze der 'junggrammatischen'
richtung". [* For an English translation, see Lehmann 1967:202.]

In reality this entire preface was extremely provoca-
tive. Taken quite abstractly, the contents of the lengthy
passage quoted above, for example, was no doubt inoffensive;
but it had to seem extremely insulting if taken personally
as a derogatory characterization of those linguists who did
not belong to the 'young grammarian' movement (or clique, as
it was more likely to appear in the eyes of most outsiders).
The result was a strong feeling of antagonism between Brug-
mann and Osthoff on the one hand and on the other not only
linguists like Curtius who could not keep abreast of the de-
velopments, but also linguists outside the Leipzig circle
who in their methodology were by no means inferior to the "young grammarians", like Schleicher's student [74] Johannes Schmidt or Fick's school (Bezzenberger, Collitz).

Many years later Brugmann (Indogerm. Forschungen, Anzeiger XI [1900] 131-32) made a statement, in reply to suspicions that the preface had been written by Osthoff, making it clear that it he wrote it himself; Osthoff had read it through, but had "sich derauf beschränkt, eine oder zwei meiner Perioden, die zu lang ausgefallen waren, in zwei Sätze auseinanderzulegen" (were they longer than the tirade quoted above?). On the same occasion Brugmann revealed how he had come to employ the word "junggrammatisch". According to this he used it with an intention which was the direct opposite of the result. He lacked a short designation for the linguists with whom he and Osthoff agreed methodologically; "'Gesinnungsgenossen', 'Freunde' und alles solches, was dar­nach schmecken konnte, als wollten wir eine 'Partei'konstru­ieren, verbot sich". Then he heard that Zarncke, in his written statement concerning R. Kögel's dissertation, had used the phrase: "Der Verf. ist ein begeisterter Anhänger unserer junggrammatischen Schule. Seine Arbeit steht in dieser Beziehung durchaus à la hauteur". This jaunty Leipzig expression, which was fairly well-suited within initiated circles as a designation for those young linguists who had revolted against Curtius (cf. the periodical title Studien zur griechischen Grammatik and Curtius's seminar "Grammati­sche Gesellschaft" which in Brugmann's hands later became "Sprachwissenschaftliche Gesellschaft") could not possibly, in the moving preface to Morphologische Untersuchungen, be taken in wider circles in the humorous manner intended by Brugmann, but would inevitably evoke ideas of a factional group or rather a clique. Thus interpreted the name wan­dered like a slogan out into the wide world. Some Danish writers found it untranslatable and retained the forms 'jung-
grammatisk' [junggrammatischer] and 'junggrammatiker' [Jung-grammatiker]. But the German word jung is by no means un-translatable; it is rather the final element which is un-translatable, and no doubt it would be best to render 'jung-grammatisch' as [75] 'ung-leipzigs'k' [of the Leipzig school] or 'ung-curtius'sk' [of the school of Curtius] and 'Junggrammatiker' as 'ung-Curtianer' [a follower of Curtius]. Here-with enough concerning this silly clique name.

The main point in the new linguistic platform developed by Brugmann was the theory that sound laws are without exceptions. This was the recognition which had been working its way forward for so long, which could not be held back in the 70's and which also had been pronounced a few times already. The infant had already been born, but Brugmann's preface was the first powerful cry with which the newborn announced its presence. At the cry everyone rushed in from all sides to see the child, which, in the opinion of many, was in extreme need of expert care and a thorough cleaning up.

The theoretical discussion of the laws governing the development of language which ensued, and which as late as 1885 still called forth polemics from Curtius and Brugmann, was interesting in many respects. But I may be excused from pursuing this further by referring to two Danish contributions: Kr. Nyrop's introduction to his work on *Adjektivernes kønsbøjning i de romanske sprog* (1886) and Otto Jespersen's article in *Nordisk tidskrift for filologi*, new series VII 207-45.

It would be senseless to deny that this theoretical discussion had its significance in the further development of comparative linguistics. But we must be on guard against the exaggerated notions to this effect which seem to be cultivated even in the neo-Leipzig camp where there is almost the opinion that it was the theory that sound laws are without exceptions which created the new linguistics and that
the man who first formulated the theory is to be considered the father of modern linguistics, regardless of how little he otherwise may have participated in the work of linguistic comparison. This is an extremely suspect confusion of cause and effect. It is not theoretical clarity which brought about the great concrete advances, but it is the concrete advances which brought about the theoretical clarity. It was experience that called forth the notion of regularity in phonetic development, and the theoretical discussion was for the most part merely an attempt to grasp how this regularity could be explained, in fact, you might even say that the attempt was largely unsuccessful; in the course of the discussion such incorrect ideas have emerged as the thought that phonetic changes were physiological processes, or that they were due to changes in the speech organs or to influence from prehistoric populations of a foreign race etc.

There is agreement on one point, however: that phonetic development (the changes in pronunciation) actually takes place regularly and that the investigation of and the strict adherence to sound laws is the comparative linguist's primary obligation. But it follows from this that the significance of etymology for the recognition of linguistic kinship is quite another now than in the beginning of the 19th century. Back then etymology could only be rejected as an independent means of proof; the use to which it could be put by visionaries was well-known, and there was no way to make it scientific. But this way exists now in the strict observance of sound laws. Rask's theory: "When, in the most essential, most carnal, most indispensable and original words, the foundation of language, there are similarities between two languages, and then a sufficient number to allow for the formulation of rules for the changes in letters from the one to the other, then there is a basic kinship between these languages", - the theory which even he employed only in con-
nection with a comparison of the inflectional system, can acquire independent significance today; linguistic kinship can be proved on the basis of sound laws alone, even though agreement in the inflectional system will continue to provide the most lucid proof.
18. Tasks for the Future

But we can surely progress even further in the historical study of the sounds of language. To date we have essentially been concerned with detecting the sound laws; but our method can surely reach the point where not only the detection of sound laws, but also their phonetic explanation will be looked upon as a primary obligation. It is already rather [77] common for linguists to turn their attention in this direction, but the result is as yet merely a series of individual comments which are often difficult to verify. If this aspect of our research is to become serious, the work will require organization. A systematic collection of sound changes must be brought about, a summary of as many established sound changes as possible accompanied by attempts at phonetic explanation. Such a handbook would be useful in two respects: theoretically and practically. It would provide the theoretician with material for inductive determination of the nature of sound changes (it will become evident that these always depend upon psychological factors) and for explanations of why they have the regular nature they actually have. And it would aid the practitioner in his investigation of difficult or dubious phonetic developments by means of its overview over changes well documented and explained. 24

Efforts in the direction indicated here could be found particularly in the circle of younger French linguists; but the war, which, among other things, has deprived us of one of the most excellent of Meillet's students, Robert Gauthiot [d. 1916], has now postponed all linguistic expectations indefinitely.

For my own part, I actually intended to present some of my views here concerning how a rational survey of the phonological changes should be organized and to give some examples of the phonetic explanations which I consider to be
correct. The preceding was merely to have been an introduction; but time forces me to break off, and thus we must be content with the introduction.

* * * * *
NOTES

1 My original intention was to consider only this last point; my work was to bear the title "The systematic treatment of sound change" or more pretentiously "The psychology of changes in pronunciation". But I soon realized that time would not permit me to work out a scholarly monograph on this topic. Being forced to limit myself to a few indications of what I had originally envisioned as my topic, I came to expand what was to have been an introduction, giving it a more popular appearance and not shying away from digressions. I have, however, seen to it that the digressions are clearly marked typographically and thus expect no criticism [[10]] on their account; I have been forced to adhere to Pilate's maxim: "What I wrote, I wrote" ["Quod scripsi, scripsi" (John 19,22)] there was neither time for deletions or additions nor for the slightest reworking of portions of my work, even though in places the composition resembles the presentation of a filibuster. — The title I have chosen in no way implies that I wish to compete with Vilhelm Thomsen's Sprogvidenskabens historie [1902]; I have centered my attention solely upon one specific aspect of the history of linguistics: the development of notions concerning linguistic kinship and the history of phonology, and I have placed special emphasis on underlining the causality involved in this development.

Allow me at this point to comment on my orthographic practices: I write av instead of af, and I ruthlessly spell foreign loans in Danish. The former I find quite imperative now, since both Swedes and Norwegians write in this fashion; when our Scandinavian neighbors introduce something which suits us just as well, it provides us with an excellent opportunity to follow their lead. And regarding the latter, I am pleased to be in agreement with widespread Swedish practice and for that matter with nearly all civilized nations (yes, even the Germans are unquestionably heading in this direction). I must admit to an inner chuckle when I see Danish authors conscientiously following Greek, Latin and French orthography in all words stemming from these languages; it reminds me of the Turks' tasteful embellishment of their language with undigested Arabic and Persian. Neither can I deny feeling a certain malicious pleasure upon encountering, even in the students' own publication, such fruits of the embellishment-principle as Face from French phase, φάσις,
Præces, Reflektion. Wouldn't it really be better to refrain from acting learned?

2 The view that the Greek alphabet was involved, or that the runes actually were essentially created with a basis in the Greek alphabet (Sophus Bugge, Otto v. Friesen) is untenable.

3 Hay as a name for the Armenians would thus be comparable to Південні ('Romans') as a name for more recent Greeks or to names such as French, Russians, Bulgarians, which call to mind the foreign rulers of ages past over nations which now bear these names.

4 This wondrous argument (cf. English a big one, Danish informal speech hvad er det for en Øn?) seems to have impressed Thumb, who in his *Handbuch der neugriechischen Volksprache*, Strassburg 1910, declares the etymology of ἀ, ἄv (Thumb's orthography) as dim, hinting only cautiously that it may have come from Ἔνα. It is – in passing – truly fascinating to see spiritus asper and spiritus lenis, which are both equally silent, employed not only in the modern Greek orthography but also in the recording of dialects.

5 Just how far this connection goes is difficult to determine, since Karolidis gives no indication at all of the regular form of the cardinal numbers in the dialect and only gives a little vague information concerning the ordinals.

6 The jingle sounds like this:

   inomana dinomana
treizeri karuferi
dudzulina šiate k′inda
urpi gurpi kokolá
ma purpi e ni lik′e
meŋ e hilk′e.

Another informant structured the jingle as follows:

   intimina dinimina
troiró sotikó
dudzulina šiate k′inda
urpi škurti kotšeka
ma surpi n′e felik′e
di felik′e
meŋ e hik′e.

And finally an Albanian woman reported: [[28]]

   intimina dinimina
dioró sotikó
kojantisa diliplas
tsinganidá ma tsingár
delomare kokosh
meŋ e iko
defelik.
Of course, there are a few real Albanian words in these jingles, for example stat k'inda 'seven clocks'; really only the conclusion is fully comprehensible mer e hîk' (hîk') e 'go and lead him away', mer e òk 'go and go away'.

7 Precisely the same judgement which I have passed above (and which I have continually passed ever since 1903 when Kretschmer's book came into my possession) is also passed by H. Grégoire, Bulletin de Correspondance hellénique 33 (1909) pp. 148-49. He adduces various striking analogies and notes that tatli, matli seem to be connected with Turkish alîy '6'.

8 From Turkish boyza 'neck, narrow passage, pass' (pronounced with an open g as in Danish bog and with voiced z) and köj 'village'.

9 There may very well be something to the suspicion that the names Kαταυία and Kαππδαυία (in the ancient Persian inscriptions Katpatuka) contain the name of the Hatians; the Greek presents no obstacle; the Hatians' name began with an ë-sound (like German oh), and we would in fact expect this to be rendered with x in Greek; more striking at first glance is the ancient Persian k, since the Persians were in possession of an ë-sound; but this can be explained by the fact that the Elamians (the inhabitants of Elam, 'Elumâţ with the city of Susa), as in so many other instances, were an intermediate link between the Persians and the countries further to the west; the Elamians rendered the ë-sound of foreign languages with k, and in the Elamitic column of Darius' great inscription the name is spelled Ka-at-pá-tú-kas.

10 The first instance I refer to is: Sieg and Siegling, Tooharisch, die Sprache der Indoskythen, Berlin 1908, which to this very day has not been followed up by the publication of the texts from which the results were derived. Our documented knowledge of Tocharian thus stems solely from non-German scholars, primarily from the Frenchman Sylvain Lévi [cf. Lévi (1911) and (1913) listed on p. 96 of this volume].

11 The system of syllabic writing which is known from Greek inscriptions on the island of Cyprus dating from the 6th to the 4th century before Christ and which, according to the most recent discoveries, was also employed by a non-Greek people on the island, probably has its origin in the Hittite hieroglyphs. This syllabic system of writing probably exerted a certain influence on the Greeks' reshaping of the Semitic alphabet. Deecke's comments on this subject (in his reworking of Karl Otfried Müller, Die Etrusker, II, pp. 514 ff., and in Beiträge zur kunde der indogermanischen sprachen VI, pp. 150 f.) can, of course, not be swallowed whole; nevertheless one is struck by the fact that one of the Greek letters which cannot conclusively be derived from the Phonecian system of writing, namely the character Y, of which the oldest Greek phonetic value is u, has more or less the same form as the Cyprian character for the syllable u. The remaining ancient vowel symbols are, as we know, reinterpretations of Semitic characters containing a consonant; but the very idea of introducing symbols for a, e, i, o could well have
been inspired by the Cyprian writing system which contained symbols for precisely $\alpha$, $\epsilon$, $\iota$, $\omicron$ (but like the oldest Greek alphabet, lacked specific symbols for $\delta$ and $\delta$, $\eta$ and $\omega$). This idea was as straightforward as Columbus’ egg, but perhaps no plainer than that either; but to get this idea even such a gifted people as the Greeks would possibly require both the hint contained in the Semitic spelling conventions and additional impulses as well. Once the idea dawned upon them, the choice of symbols followed fairly automatically (where $\imath$ and $\omicron$ are concerned, however, no doubt just barely) as a consequence of the names of the letters. The names of the letters also played a role with kappa and koppa; but the very way in which the Greeks made use of these symbols smells distinctly Cyprian. If we have no other premises than the Semitic alphabet and the Greek language, it is incomprehensible that koppa would not be used to represent the aspirated $\kappa$ in preference to a meaningless orthographic squiggle; because the use of koppa before $\omicron$ (and $\upsilon$) is no more than that; there is nothing at all to indicate there was a readily discernable difference in the pronunciation of $\kappa$ before $\alpha$ and before $\omicron$ or $\upsilon$ in Greek. But the use of koppa becomes clear if the first people to utilize the alphabet had Cyprian spelling habits in their minds; because in Cyprian there were, of course, different symbols for $\kappa\alpha$ and $\kappa\omicron$, and a Cyprian-trained mind needed only the prodding of the parallel letter names in order to let the elf move along to his new house and to employ koppa not only before $\omicron$, but also in cases like $\lambda\kappa\rho\omicron\iota\varsigma$, $\lambda\kappa\rho\omicron\upsilon\nu$, because the Cyprian spelling rule which was in his blood required $\kappa\upsilon\rho\omicron\kappa\rho\upsilon$- here; he had no qualms about doing without a symbol for the aspirate; [33] the Cyprian system of syllabic writing had taught him to be stingy when it came to symbols for the stops. Furthermore, it is a strange coincidence, that the Greek letter $X$, which signified $\kappa's$ in the West Greek alphabets, is nearly identical in form to the Cyprian syllabic character for $\kappa's\alpha$. This is all the more unusual, since in an alphabetic writing system there is no need for single symbols to represent two consonants ($\kappa's\alpha$ and $\rho's\alpha$; $Z$ is quite another matter; when the alphabet was adopted it probably had the pronunciation $dz$, which subsequently in most of Greece was replaced by $zd$; but what is rendered in rough phonetic notation as $dz$ is not at all a group of two consonants; it is a single sound, an affricate). However, in a system of syllabic writing like the Cyprian, which expresses only syllables ending in a vowel, there is, of course, abundant demand for any trick to keep you from breaking your bones on the languages' numerous consonant groups. I assume then that $\kappa's$ is the oldest significance of the Greek symbol $X$, and that the significance of $\kappa's$ in the East Greek alphabets is derived from this. Naturally, I must save the detailed justification of my view concerning the Greek alphabet until another occasion. I hasten to mention here, however, that I take the full consequences of my arguments concerning $X$ and consider all new aspirate symbols as deriving from older symbols for groups of aspirate plus $\alpha$. Thus, I hypothetically assume an original significance of $\kappa's$ for the three-prolonged $\Psi$ signifying $\kappa's(x)$ in the West Greek alphabets. Just as there were two symbols for $\kappa$ (kappa and koppa), there were also two symbols for $\kappa's$; since $X$ derives from a Cyprian syllabic symbol $\kappa's\alpha$, it is possible that $\Psi$ de-
rives from a syllabic symbol (unknown to us) for \( k's \); in the East Greek alphabets it has arbitrarily come to signify \( p's \). In my opinion the original symbol for \( p's \) was \( \Phi \), which in turn stems from a Cyprian syllabic symbol unknown to us. I need not be reproached for operating with two unknown Cyprian symbols. It is clear that we still do not know all the symbols in the Cyprian writing system, and in fact a short time ago we had no knowledge of \( k's \). The discrepancy between the West Greek and the East Greek alphabets is thus a result of the differing way in which the task of extracting aspiration symbols from the ancient \( \xi \)- and \( \Psi \)-symbols was carried out; the West Greek alphabets simply did without the \( \Psi \)-symbol, but the East Greek alphabets incorporated both a \( \xi \)- and a \( \Psi \)-symbol by means of random shifts in representations.

The South Greek tribes, quite insignificant after the Doric migration into Peloponnnesus, found themselves in their distant colonies on Cyprus (and in Pamphylia) precisely in the area where the very ancient Hittite culture met with the Semitic. They received cultural impulses from both, first from the Hittite, then from the Semitic; out of this contact came the final and decisive advances in the development of writing, which subsequently was carried around the Mediterranean at full speed while the parents of this prodigy still continued to exist at home on Cyprus.

12 I do not know who was the first to employ the term Japetic or Japhetic; perhaps it was the Swede Georg Stiernhielm, who notes in the preface to the Gothic Bible translation published in Stockholm in 1671: "Lingua Hetrusca, Phrygia, et Celtica affines sunt, omnes ex uno fonte derivatae. Nee Graecae longe distat. Japheticæ sunt omnes: ergo et ipsa Latina. Non igitur mirum est, innumer a vocabula dictarum linguarum communia esse cum Latinis". He finds fault with Voss, because the latter "infeliciter, et invitata Minerva" derives numerous Latin words from Greek words "qua commod et sine violentia duci potuerunt ex Celtica". By *Lingua Celtica* Stiernhielm means Germanic (and Celtic); but aside from this I am unable to go into further detail with respect to Stiernhielm's theories since his Ulfils-edition is not available in Copenhagen (the above quote, which I had from Lhuyd, *Archaeologia Britannica*, Oxford 1707, p. 35, has kindly been compared with the original by Professor Evald Lidén of Göteborg).

13 With this Leibniz argues against the view, the originator of which I do not know, that there was a particularly close relationship between Persian and Germanic; all that he is willing to recognize in this direction is that Persian *west* 'God' resembles German *Gott* etc. (and even this similarity is quite coincidental). The view rejected by Leibniz is still around in Bopp's *Conjugationsystem* where Persian and Germanic are in fact treated under one heading.

14 This naivety cannot be explained away. It is hardly any accident but rather an example of the fact that Bopp did not possess Rask's keen eye for linguistic kinship; cf. his attempt to label the Malay-Polynesian languages as Indo-European [cf. editor's note on page 50 (above)].
Three etymologically different words.

Etymologically different.

In Latin *vivus* an initial *g* has been lost.

This view, of course, is also distorted; α:η is an alternation inherited from the parent language, and η is in no way the result of the following α.

Grimm recognized the regularity of the Germanic sound shift; of the full regularity of phonetic development he has no notion; otherwise he would have realized that the task of explaining πατήρ from *παυήρ* was as difficult as the task of explaining Gothic *fadar* without presupposing *παυήρ*.

Thus I cannot agree with Vilh. Thomsen, who in his presentation of Sprogvidenskabens historie pp. 52-53 feels forced to acknowledge the fact that Grimm "was the first to see the extent and great historical significance of the sound shift".

It is curious that Pott in his comparative inventory of verb roots also takes Sanskrit as the starting point to the extent that roots found in various major Indo-European languages but lacking in Sanskrit are omitted, a fact expressly stated by Pott, II 476. Indeed he is so close to confusing Sanskrit with the parent language, even though he is to be commended elsewhere for his understanding of the application of a historical point of view to notions of linguistic kinship.

Where Verner is concerned this is evident in his statement from 1874 (Vernerbogen p. 221) [Verner 1903]: "Everything in it is brilliant (the style, the organization, the contents), and if its proliferation of hypotheses and suspicions were just as solid the work would be quite excellent". Scherer's influence on Verner is easily demonstrated; a linguistic reminder is no doubt the expression "hinübergespielt" KZ XXIII 103, cf. Scherer p. 29.

The late Slavic scholar Leskien [d. 1916] is the one to whom this disservice is to be rendered.

Pott would perhaps view all this as "ein elendes Banausengeschäft"; however, it would surely alter the appearance of linguistics so much that the sticklers would be forced to find a new nick name to replace "Lautschieber", which would not really be appropriate for the new form of phonetic investigation.
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*[In order to render the bibliographical references more useful to the present reader, a number of more recent items, in particular those in English (translation), have been added. Further useful information may be gleaned in Koerner, *Bibliographia Saussureana 1870-1970* (Metuchen. N.J.: Scarecrow, 1972), Part II ("Background Sources ... , 1816-1916"), pp.215/220-326. Editor.]


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Note: These above additions to the Select Bibliography consist of only those items mentioned, though only indirectly and in passing, in Pedersen's text, and which cannot be found in the list of secondary sources mentioned in the footnote on p.91 (above). — For other such oblique references, e.g., Brücke (1856), Collitz (1866), Leskien (1876), Osthoff (1876), Paul (1880), Verner (1886), and Wimmer (1871), the indicated section in Bibliographia Saussureana may be consulted.
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