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COMMUNICATION AND ‘FORESTRUCTURES’ AT THE GEOLOGICAL INTERSECTION OF CAVES AND SUBSURFACE WATER FLOW: HERMENEUTICS AND PAROCHIALISM

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ABSTRACT

The direction of cave and karst science throughout its history has been partly determined by communication—or, more commonly, the lack of communication—between non-scientist cavers and non-caving physical geologists writing about karst. Within each community, advancement of ‘cave awareness’ occurred through a hermeneutic circle in which ‘forestructures’ guided progress. One result was regionalism of speleo-genetic theories developed within karst science because of the weight of evidence placed upon local or regional observations. Many speleogenetic theories of the mid-1900s suffer from this parochialism, failing to take into account findings from karst of different geologic settings. During the past half-century, the accumulated worldwide data on caves and karst suggest larger, more encompassing theories of speleogenesis. One such example of how speleogenetic theories have changed, partly explored in this essay, is the relation of cave formation to the position of the water-table. In many karst aquifers, including but not limited to alpine systems, one modern view envisions the enlargement of caves to proceed in a punctuated manner, driven by floods.

1. INTRODUCTION

Caves are sources of fascination and wonder. We also now recognize their important ecological and hydrological function in karst landscapes. To this day, however, we as a society often have limited knowledge of these natural resources and their wide-ranging significance. A widely publicized ‘factoid’ asserts that “roughly 20–25% of the global population largely or entirely depends upon ground-waters obtained from [karst aquifers]” (Ford and Williams 2007, p. 1). In Florida, where more than 90% of 18-million residents rely upon the Floridan aquifer for drinking, industry, and irrigation waters (Scott et al. 2004), this is certainly an underestimate. In another example from Kentucky, home of Mammoth Cave, data compiled by the Kentucky Geological Survey reveal that limestones exposed at the surface underlie approximately 55% of the state, and the majority of these limestones are karstified. In fact, 4% of Kentucky lies within topographic sinkholes (Paylor et al. 2002 and 2003). Clearly, understanding the nature of caves and karst in places such as Florida and Kentucky would promote better knowledge and management of geological, ecological, and water resources. The recent establishment of state-level repositories of cave-related data in Kentucky in 2000 (Florea et al. 2002) and in Florida in 2003 (Krause 2008) is in part a response to the need.

Due to communication problems there has been a persistent disconnect between data acquisition and data interpretation in the study of caves and karst. The impact on cave and karst science was a regionalisation of speleogenetic theories. Such difficulties start with the fact that
cave and karst science has traditionally depended on amateurs and explorers to locate sites and acquire much of the basic data. Other branches of natural science face this dilemma, such as ornithology (Mayfield 1979), entomology (Brunelle 1997), and mycology (Watling 1998), where amateur naturalists collect significant amounts of information on species diversity and population, as well as astronomy, where amateur stargazers detect and provide a wealth of information on comets and asteroids (e.g. Orchiston 1999).

One may view the input from amateurs in a positive rather than pejorative viewpoint. For, as stated by one reviewer of this manuscript: “amateurs [may] often know a lot more than they publish while professionals can publish a lot more than they know”. In the context of caves and karst, however, the basic data amateurs collect often wind up in mostly unavailable or hard to find ‘grey literature’ (Chavez 2010) such as “consulting reports, expedition summaries, and caving-club newsletters” (Florea et al. 2007, p. 229). Such publications frustrate library scientists, who term them “sneaky, fly-by-night, changecoa publications [that are] hard to identify, hard to acquire, hard to catalog and retrieve, and hard to preserve” (Walcott 1990, pp. 185–186).

To further complicate the study of caves and karst, data collection in caves is often arduous, physically demanding, and time consuming. Exploring caves requires special equipment and techniques that call for training and a certain amount of daring, if not hubris. The fieldwork requires one to get ‘up-close and personal’ with the subject of study and is therefore a powerful emotional experience involved. Passages in caves therefore receive names that are often related to the tactile experiences involved in negotiating them, such as ‘meat cleaver’ or ‘knee shredder’. “As we get to know a cave, the kinesthetic patterns and the landmarks will become integrated until they are a contiguous space, a place we are intimate with” (Aitken 1986, p. 28).

Consequently, as proposed by another reviewer of this manuscript, one might suggest that there are ‘cavers’ that are interested in the sport or exploration aspects only; ‘caver scientists’ who are caving enthusiasts trained and practising in some outdoor science and occasionally dabbling in cave and karst science; ‘cave scientists’, with much caving experience and for whom caves and karst science are central to their careers; and ‘non-caving scientists’, who may include some resource managers and regulators who extract cave-specific material for study outside a cave but know little or nothing else about caves. Each group brings its particular set of experiences to the study of the natural phenomena of caves.

In this essay we present a case study of the impact of communication on cave and karst science—which historically has straddled disciplinary boundaries. We will reduce the aforementioned groups simply into those who are intimately familiar with caves, the ‘cavers’, and those scientists who may or may not know much about caves and karst, the ‘scientists’. Occasionally we refer to the subset of ‘caver scientists’ and ‘cave scientists’ to illustrate particular points of relevance or importance. We shall not investigate the role of the non-caving, non-scientific public, but acknowledge the important role they serve in providing site access and historic commentary.

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1. Surveys within the membership of the National Speleological Society (NSS) illuminate the role of amateurs. During a study conducted at the NSS Annual Convention in 1979 (Bunnell and Vesley 1985) questionnaires asked cavers how they became involved in caving. Out of 273 respondents, only 2.8% began caving for scientific purposes (p. 54). Later in the same article, responses indicate that ‘survey-type’ cavers rank science higher than ‘sport-type’ cavers as a motivation for caving (p. 56). In yet another measure, 14.9% of 726 members of the NSS that responded to an online survey during the spring of 2005 indicated that they considered themselves ‘professional scientists’. Finally, a review of occupations of the active members of the NSS in 2005 revealed that 13% have a scientific occupation, and 6% have an earth science or cave-management oriented occupation.

2. The first author is one of these.

3. The second author is one of these.
Herein, then, we explore two points of contact between our two major groups. First, we outline the roles of cavers and scientists in a field of study that depends upon vast amounts of field observations in difficult locations. We explore these roles from the viewpoint of hermeneutics, first advanced by the German philosopher Martin Heidegger in his 1927 text *Sein und Zeit (Being and Time)* and later applied to the earth sciences to explore the nature of ‘forestructures’—an individual’s set preconceptions, goals, and tools. Next, using the evolution of speleogenetic theories as an illustration, we examine the connection between communication and regionalism in cave and karst science and encounter the tendency toward a parochialism of theories, though that has recently been ameliorated by globalization. Historical facts are important to our essay. However, it is important to state that others have authoritatively documented the history of cave and karst science. We do not propose to retell that story here.

2. KARST STUDIES AND THE HERMENEUTIC CIRCLE

The feedback cycle of observation, theory, and renewed observation is a version of Heidegger’s hermeneutic circle, which Robert Frodeman⁴ introduced to the geologic community in the pages of the *Bulletin of the Geological Society of America* some sixteen years ago:

> [U]nderstanding is fundamentally circular; when we strive to comprehend something, the meaning of its parts is understood from the relationship to the whole, while our conception of the whole is constructed from an understanding of the parts. . . . Thus our understanding of a region is based on our interpretation of the individual outcrops in that region, and vice versa (Frodeman, 1995 p. 963).⁵

This understanding is attained by the iterative interpretation of signs:

> [H]ermeneutics is the art or science of interpreting texts. A text . . . . is a system of signs, the meaning of which is not apparent but must be deciphered (Frodeman 1995, p. 962).

In the following discussion, we subdivide signs into the set provided by Nature and the set provided by written texts. Both sets have different meanings to cavers and scientists, and thus both groups have distinct ‘forestructures’ composed of experiences and preconceptions that dictate what they will observe, goals that drive their investigations, and a set of tools or skills to interpret their observations.⁶ Moreover, cavers and scientists experience their own individual hermeneutic circles. The circles are interlinked through communication (see Figure 1). Both groups record or publish observations and interpretations (encoding) and compile and decipher the records or publications of the other group (decoding). Cavers and scientists may have drastically disparate ‘forestructures’ with respect to the encoding and decoding of texts.

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⁴ Frodeman’s foray into geology came after his PhD in philosophy when he enrolled in the geology graduate program at the University of Colorado to observe how the paleontologist and stratigrapher Erle Kauffman and his students went about thinking about the Earth and its history.

⁵ Heidegger, in *Time and Being*, claimed that there are three main circles in human thought: the ontological circle dealing with the circular nature of human existence; the hermeneutic circle, which proved the circular nature of human understanding; and the strategic circle, which applied the very same circular hermeneutics to investigate the meaning of human existence.

⁶ Within the hermeneutic circle, Heidegger argued for the existence of three types of ‘forestructures’: Vor-habe (‘fore-having’), refers to the act of possessing the holistic idea of the phenomenon prior to study (similar to Frodeman’s ‘preconceptions’); Vor-sicht (fore-sight), refers to seeing the general schema of a phenomenon in advance of a study (similar to Frodeman’s ‘goals’); and Vor-griff (fore-grasping) refers to having, in advance, a system of concepts to capture the details of a phenomenon under investigation (similar to Frodeman’s ‘tools’).
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Figure 1. The intertwined hermeneutic circles of cavers and scientists. Each group enters the field with preconceptions, makes observations, and develops interpretations of Nature. Cavers produce records of caves and scientists publish the results of their studies. Both are links that help mold the preconceptions of both groups.

To illustrate the contrasting ‘forestructures’ of cavers and scientists, we begin with the caver in a cave, where an observation of Nature’s ‘signs’ is made.

Cavers enter the field with their individual blends of experiences and preconceptions. Their motivation may be exploration. Many, including the first author and numerous colleagues, started caving as a means to search for mystery, thrill, and adventure by finding something new and discovering what is around the next corner. Simple questions drive this pursuit, such as ‘Where are the caves?’ or ‘Where does this cave lead?’. While seeking answers, cavers perform several tasks using the tools within their repertoire; and they record observations for later interpretation. They hike the landscape searching for new caves. They map the caves to see where they lead and where one may explore next. They make notes of features within a cave using maps or sketches (e.g., breakdown, formations, passage shapes, and sediment deposits). They may make an inventory of the biota within the cave. They often produce verbal or written accounts of air-flow patterns, stream-flow patterns, and responses to storm events. In many cases, this documentation serves as a tool for future discoveries.

During this ‘encoding phase’, errors and biases are introduced by the cavers’ ‘forestructures’—their experience-base and background. This background will strongly influence what they ‘see’ in the cave, and will thus affect the content of the data they record. Though one might expect a caver’s observations to speak to the facts, they must recognize a feature of science, memorably summarized by Peter Medawar: “Innocent, unbiased observation is a myth” (Medawar 1969, p. 28). Frodeman presents a similar notion: “In effect, hermeneutics rejects the claim that facts can ever be completely independent of theory” (Frodeman 1995, p. 962)—or biases. Thus, a caver who is a medical doctor may note the number of cave crickets or bats in a cave. In contrast, a caver who is a farmer may record the presence or type of soil on a cave floor. While this can be a very positive outcome, as it may lead to new discoveries or further scientific research, it may also lead to the omission of other data critical to a geological interpretation.

Thus a recurring question encountered during decoding (interpretation) is: ‘How reliable are the data recorded by the cavers?’ In fact, cave-exploration surveys vary in content and quality (see Figure 2). For the caver wanting to navigate through a cave, a simple line plot or sketch may be adequate. For the scientist, this type of map may convey little useful information (e.g. Chabert and Watson 1981). For example, passages too small to explore may not be noted on a map, even though they may play a critical role in the hydrology of the cave. A written-
communication with John Mylroie, a ‘cave scientist’ colleague at Mississippi State University, emphasizes this point.

The over-riding interest in the amateur for exploration creates... issues, such as passage description. ‘Crawlways’ and ‘walking passage’ are common terms, but the former has less importance to recreational cavers than the latter, even though the former could be a... major flow route, and the latter a... vadose canyon of only local [hydrologic] importance (John Mylroie, personal communication, 2008).

A second problem encountered during the encoding phase is the temporal and spatial density of data—a problem of scale and place that is well recognized in meta-analyses of geology such as the widely-read Ten Ways to Be Wrong by geologist Stanley Schumm (1991, p. 46). For instance, observations by cavers of water flow in air-filled caves, like those in Kentucky, are heavily weighted toward times of low flow. In contrast, cave divers wearing SCUBA gear in the underwater caves of Florida are less concerned with these limitations. However, the Florida cave divers are time-limited by their air supply and the decompression stops needed for greater depths. While staying out of Kentucky caves during floods and returning to the surface in Florida with air still in the tank obviously preserves the life of the explorer, it introduces a temporal and geographic bias that may influence interpretations, as we shall elaborate further in Section 3.2.

Published maps of cave locations often have a geographic bias as well. For instance, the first author for many years jokingly heard from other cavers in Kentucky that most caves are located within one mile of a State road, a Baptist church, or a cemetery. (Interestingly, this often proves to be the case due to the availability of places to park a vehicle while hiking.) Of course, this is a post-European-settlement viewpoint; it is likely that Native Americans knew the location of these and many other caves. This same scenario has played out throughout the world during the past century as American and European cavers have ‘discovered’ caves that were already well known to the aboriginal people of those lands. Regardless, scientists using data from cavers need to remember that: “predictions based upon data from one location may not be valid elsewhere” (Schumm 1991, p. 58).

Geographic bias is more than just the location on the surface of the Earth; it also includes length and depth.7 While modern remote sensing and geophysical techniques may help identify the location and perhaps the magnitude of the cave ‘size’, the caver or scientist cannot use modern technology to find out what things are like ‘inside’ the cave.8 Therefore, expeditions of cavers continually seek to locate new longer or deeper caves and extend known caves toward new records. However, these efforts can entail significant risks. Very deep caves generally require years of experience and the use of specialized equipment. Knowledge of rope and climbing techniques are needed in the deep and dry caves of mountainous regions. Expertise in the field of SCUBA technology and mixed, compressed gases is necessary for exploring deep, underwater caves. Thus, exploration bias leads disproportionately toward the parts of caves that are easier to explore. However, recent, high-profile expeditions are contributing to an emerging record of caves in previously inaccessible locations.9

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7. Data filters in the databases from state cave surveys exemplify this geographic bias. For example, the Tennessee Cave Survey does not include in their dataset any cave less than 10 m in length or depth. Thus, a large segment of cave that fills with water or ends in collapse after 9 m is not logged even though it may represent a small piece of a much larger system and be important data for the scientist.

8. This statement may become somewhat dated in the next few years. Already, cavers and scientists are working on remotely operated tools to explore caves, most particularly in underwater caves. Bill Stone, with his Digital Wall Mapper used in Wakulla Springs, Florida in 1999, his DEPTHX probe mission in Zacaton, Mexico in 2004–2007, and his present-day ENDURANCE probe mission under an ice-sheet in Antarctica, is the world authority on this technology. Examples of his work are published in National Geographic.

9. Currently, at the time of this essay, the deepest known cave in world is Krubera in the Ukraine at 2,190m. For an exciting narrative describing the monumental efforts needed to reach these depths see Klimchouk (2008).
Scientists who may be unfamiliar with a particular cave or group of caves compile the records collected by the caver and face the daunting task of interpreting the data using their own ‘forestructures’ during the decoding. The resulting interpretation is therefore a function of the preconceptions, goals, and tools of the scientists. Speleogenetic questions such as ‘why is the cave there?’, or hydrologic questions like ‘how does water flow through the cave?’, call for very different methods to look at the same data. The tools used to answer these questions—such as the statistical techniques used for the analysis of spatial or temporal data—are generally less subjective and more quantitative than those used by the caver. The results are also subject to greater scrutiny and review.

For the above reasons, the advancement of cave and karst science faces a dilemma. How do the disparate efforts of exploration (cavers) and interpretation (scientists) attain a mutually beneficial synergy? Without exploration, scientists lack the observational data needed to reach their goals of learning how Nature works. Without science, explorers have less guidance in learning how to read Nature’s signs in ways that help them reach their goals of finding more caves and protecting those natural resources. A direct solution is to become both caver and scientist—the ‘cave scientist’—the path the first author has sought to follow.  

Through the history of cave and karst science, many individuals have made important contributions by following a path combining science and exploration. As demonstrated in Section 3, however, such a path carries the risk of parochialism arising from a limited number of geographic locations or periods of observation. Even an extensively traveled ‘cave scientist’ cannot escape the dependence on observations by cavers who may be the only ones to have seen a particular field location or have documented a particular event. In the end, a great

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10. Our essay originated as the response by the first author to a take-home portion of his PhD qualifying exam in 2005. The question was posed by the second author, who had responsibility in the examination to cover the history and philosophy of geology. The question was: ‘What insight, if any, does Frodeman’s analysis of hermeneutics and geology have for your view of the history of karst science?’
responsibility for ‘cave scientists’ is to communicate the results of their research through publications not only to other scientists but also to other cavers, to advance their ‘forestructures’.

3. COMMUNICATION, ‘FORESTRUCTURES’, AND THE EVOLUTION OF SPELEOGENETIC THEORIES

Before 1800, Western theories of cave formation were based upon a worldview constrained by the short timescale of religious belief. The 4004 BC date for the age of the Earth appearing in the Bible as a marginal note by Bishop James Ussher forced naturalists or natural philosophers to interpret natural processes at rates incompatible with present-day processes. Scientists called upon ‘catastrophes’ to account for the origin of the caves they observed. Some theories associated caves with tectonic processes.\(^{11,12}\) However, many such as Alexander Catcott in his Treatise on the Deluge argued that retreating water from the Flood formed caves through erosion (Catcott 1761).

Uniformitarianism emerged as the great organizer of geological thinking in the early 1800s (Albritton 1980) with the publication of Hutton’s ‘Theory of the Earth’ (1788), Playfair’s Illustrations (1802), and Lyell’s Principles of Geology (1830–1833). Lyell’s Principles contained clear statements concerning the role of limestone dissolution in speleogenesis:

> The subtraction of many of the elements of rocks by the solvent power of carbonic acid ... must be one of the most powerful sources of those internal changes ... so often observed in strata of every age. ... It rarely happens, except in limestone rocks, that the carbonic acid can dissolve all the constituent parts of the mass; and for this reason, probably, calcareous rocks are almost the only ones in which great caverns and long winding passages are found (Lyell 1990, p. 217).

Other scientists who studied caves disagreed. One example was Armand Flamache who strongly objected to the dissolution of limestone as a formative mechanism and instead preferred mechanical erosion. Flamache (1895) held that water in caves would quickly become saturated with calcium carbonate. Edouard Dupont (1894), on the other hand, listed several morphological reasons to think that mechanical erosion did not form caves. Included among them were the dual observations that caves do not form in rocks softer than limestone, and many caves do not contain, nor do they show evidence of ever having contained, free-flowing streams—both of which claims are refuted by modern data.

The prominent French speleologist Edouard Alfred Martel adopted a compromise position:

> No theory about the origin of caves is universal: those that have been put forward have generally claimed to be too inclusive; almost all of them are partially correct; the whole truth lies sometimes in the application of a particular one in a particular case (Martel 1896, pp. 53–54).

Martel’s comment of 1896 reminds us of geologist T. C. Chamberlin’s (1890) famous paper on multiple working hypotheses published earlier in that decade:

\(^{11}\) An account of several early theories of speleogenesis can be found in Shaw (2000, pp. 21–29). Also see Shaw (1992). The latter of these two publications represents Shaw’s doctoral thesis from 1975 and is the seminal compilation of historical records regarding karst exploration and study. His work inspired portions of the present paper.

\(^{12}\) G. L. Buffon provided an example of such a theory in his Des époques de la nature (Buffon 1962). In this work, he invoked the episodic collapse of great arches in underground caverns to explain marine fossils located in rocks well above present sea level.
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But to my judgment neither the one nor the other, nor the third, constitutes an adequate explanation of the phenomena. All these must be taken together, and possibly they must be supplemented by other agencies. The problem, therefore, is the determination not only of the participation, but of the measure and the extent, of each of these agencies in the production of the result (Chamberlin 1890, p. 94).

Both Martel and Chamberlin suggest what Schumm calls a ‘composite hypothesis’ (Schumm 1991, pp. 13–14).

Martel also made an indirect point of the inherent regionalism or parochialism in cave and karst science. Although he did not use the terminology of the hermeneutic circle, Martel’s point was that our experiences and communication with others establish ‘forestructures’ that influence our observations. His efforts at building international lines of communication greatly assisted karst research. His own underground experiences were vast. The combination of research and experience provided him a broader perspective than many of his contemporaries (see Figure 3).  

Figure 3. Wood carving of Edouard Alfred Martel, portraying his first descent of Gaping Gill in the Yorkshire Dales in England in 1895.

Disparate theories of speleogenesis and groundwater flow in karst developed during Martel’s lifetime. These theories were often based on geographically or time-limited experiences—parochial as opposed to globally integrated observations. The merits of these theories were argued during the ‘water-table’ debate during the first quarter of the twentieth century in Europe and the second quarter of the twentieth century in America. The speleological issues in the water-table debate have been explicated in the textbooks by White (1988) and Ford and Williams (2007). According to Watson and White (1985, pp. 111–115), the thirteen years of 1930–1942 were the classical period of karst research in America.

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13. Shaw (1992, p. 29) gives an account regarding the impact of Martel upon the science of karst: “Martel was more than just a cave explorer, cave researcher and writer. He also consciously caused the study of caves to spread into countries outside the European core where it was already flourishing. He was a leader who inspired and encouraged people to investigate the cave and karst problems of their own lands. . . . There can be no doubt of the extent of Martel’s personal links with Speleology in other countries. Of his 26 annual campaigns of exploration, 19 went outside of France in what are now 20 different countries”.

14. For a discussion of the meaning of ‘water-table’, see Appendix 1.
3.1. Geographic parochialism and the water-table debate

By the end of the nineteenth century Martel and others had reasonably established that caves formed by a combination of dissolution and mechanical erosion. According to Chamberlin and Salisbury’s 1904 textbook, *Geology*:

In their [caverns’] production, solution may be abetted by the mechanical action of the water passing through the openings which the solution has developed (Chamberlin and Salisbury 1904, Vol. 1, p. 227).\(^\text{15}\)

The water passing through the openings is, of course, subsurface water. The question thus became: how do caves and caverns fit into the system of circulating subsurface waters, including groundwater?

A first important step toward an answer came from detailed studies of the karst of the Dinaric Alps by two students of Albrecht Penck, the renowned geomorphologist at the University of Vienna. In 1903, Alfred Grund suggested that groundwater in karst could be subdivided into an upper zone of active circulation that rises and falls in response to wet–dry season cycles and a lower zone of stagnant groundwater. Water in the upper *Karstwasser* zone discharges at springs and generally has a short residence-time. The lower *Grundwasser* zone represents the base level to which landscape evolution adjusts (Grund 1903). Grund’s view of karst explained the phenomena he observed. His studies, however, were restricted to the Dinaric karst and were based on limited data.

Objections to Grund’s view were summed up by Katzer (1909). Using more sites and a longer time-frame for observations, Katzer concluded that springs at higher elevations do not always cease flowing before those at lower elevations. He also noted that springs do not all respond in the same way to storm events; some, for example, do not respond at all. He therefore rejected the notion of a regional, integrated *Karstwasser* system. Instead, he argued, each spring represents the outlet for an independent network of conduits. Martel (1921) supported this viewpoint. In fact, he proposed that caves developed chiefly in the vadose (unsaturated) zone, a theory based in large part upon his existing ‘foresstructures’—which were developed from his experiences in the caves he had explored in the mountainous regions of Europe. According to White:

To Martel . . . the existence of underground rivers was self-evident, as was their tremendous erosive power as they thundered down deep pits from the high plateaus to springs in the valleys below. The water simply picked the easiest path through the joints, fractures, and bedding plane partings (White 1988, p. 266).

Martel’s conclusion was that there is no water-table in karst aquifers.\(^\text{16}\)

Jovan Cvijić, the second of the two aforementioned students of Penck, further elaborated these viewpoints in 1918. He too rejected the presence of a regional *Karstwasser*, but he did support the notion of a regional groundwater system. According to Cvijić, groundwater circulates within this regional system, yet the water-table is discontinuous. It is discontinuous, he thought, because spring systems are independent. Thus Cvijić envisioned three

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\(^{15}\) Within this textbook, we find the following statements. “They [caves] were probably developed when the surface relief was slight, and surface drainage therefore poor. Regions where caves were developed under these conditions may subsequently acquire relief, so that caves are not now confined to flat regions”. Most observations of caves, the authors note, are from Kentucky and Indiana, where topographic relief is minimal. Martel would probably have taken exception to these statements, since many of the French caves he studied were vertical in alpine settings with exceptional relief.

\(^{16}\) For a discussion of this statement, see Appendix 1.
hydrographic zones within the karst groundwater system: the zone of permanent saturation, the zone of periodic saturation, and the dry zone (Cvijić 1918).

In contrast to these European theories, which in part developed directly from first-hand experience in caves, the 1930 benchmark paper by William Morris Davis, ‘Origin of limestone caverns’ (1930), was essentially an armchair synthesis by the leading theorist and communicator of physical geography in the US in the first part of the twentieth century. This paper presented a two-cycle theory for speleogenesis. In the first cycle, the cave is dissolved deep within the phreatic (permanently saturated) zone; in the second, the groundwater drains from the cave, and sediments aggrade within it. These two cycles meshed with Davis’s worldview of cycles of erosion.

Even in the face of strong criticism, Davis’s two-cycle speleogenesis has influenced karst science into modern times, at least to the extent that it is even now thought worth specifically rejecting. According to Watson and White:

Davis’ theory . . . is so influential, it is generally wrong . . . and an example of both Davis’ great synthesizing power and how limited data considered under the influence of a general model can give rise to a specific theory . . . false to much of the actual data (Watson and White 1985, p. 113).

How did Davis arrive at such a misguided theory of speleogenesis? Much of the answer lies in the fact that he lacked first-hand knowledge of the topic. In his own words: “the facts on which this essay is based are . . . derived almost entirely from the work of others” (Davis 1930, p. 484). Davis visited only a few of the sites he used as evidence, relying upon maps of varying quality. Additionally, he took examples from diverse regions that had been formed by different geologic processes and in different geologic settings.

Aside from Davis’s great reputation, one of the reasons his paper was so influential undoubtedly was his ability to draw information from a variety of fields that increased the weight of his arguments. Of particular interest are the discussion and diagrams of groundwater flow that he brought into speleogenetic theory adapted from the flow nets within the influential work by Henry Darcy published in 1856, and later the mathematical groundwater considerations of the agriculturalist Franklin Hiram King (1913). His work with the U. S. Geological Survey, in particular, supported the concept of deep circulation of groundwater and therefore gave credence to Davis’s notion that cave formation occurs well below the water-table.

Two years after the publication of Davis’s ‘Origin of limestone caverns’ Allyn Swinnerton published a paper of the same title in the same journal (Swinnerton 1932). Swinnerton, like Cvijić in the Dinaric Alps, emphasized the role of the zone of periodic saturation. He argued that the sub-horizontal levels in many ‘tiered’ caves cut across geologic structures and therefore may be a reflection of seasonal changes of the water-table. Swinnerton’s paper suggested that frictional resistance would guarantee that the greatest volume of water would flow along the path of least resistance, which would generally be the shortest path length. Groundwater flow would thus occur near the surface of the water-table and therefore control the development of cave systems near the water-table.

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17. Davis’s first exposure to karst occurred in 1899 when he visited Penck and his students and spent time in the Dinaric Alps. It is likely that the concepts developed by Grund had an impact upon Davis. It is equally likely that Davis and his theories of geomorphology influenced Penck and his students for both Cvijić and Grund developed karst landform cycles. See Ford and Williams (2007, pp. 391–395) for a summary.

18. Following Davis’s exposure to the karst of the Dinaric Alps in 1899, he expended little effort in personally studying karst. Rather, he collected publications by others and synthesized them in his 1930 paper as yet more examples of his two-stage erosional cycle. Moreover, Davis only focused on a few site examples, specifically Mammoth Cave, Carlsbad Caverns, the Ozark Plateau of Southern Missouri, the Mitchell Plain of Southern Indiana, and the Shenandoah Valley of Virginia. Of these five examples, only Mammoth Cave and the Mitchell Plain share common geomorphic settings.
Thus Davis and Swinnerton appear to have reached divergent theories. Whereas Davis promoted deep circulation of water and cavern formation well below the water-table, Swinnerton advocated that “the vadose-water hypothesis, with emphasis on the controlling factor of the water-table, is an adequate explanation for these caverns” (Swinnerton 1932, p. 667). Davis’s reasoning was largely deductive. In terms of Frodeman’s hermeneutics, he gained meaning for the parts from their relationship to the whole—in this case a worldview of his cycles or erosion. For him, fundamental principles governed the physical processes. The mathematics of groundwater flow in a porous media as demonstrated by Darcy, King, and others in the field of hydrogeology provided the context from which one could deduce where caves should form. In contrast, Swinnerton’s reasoning was principally inductive. In terms of Frodeman’s hermeneutics, he gained meaning for the whole from its relationship to the parts—in his case direct field evidence from sites such as Mammoth Cave. In his own words: “[f]urther observation is clearly essential to the solution of the general problem” (Swinnerton 1932, p. 692). However convincing, Swinnerton’s concepts suffered from a lack of mathematical rigor. According to White, his work was “squelched . . . [by] searing criticism” (White 2000, p. 40) from M. King Hubbert’s now-classic 1940 paper, ‘Theory of ground-water flow’, an impressive work of deductive reasoning based upon first principles that laid the foundation for the future of hydrogeology and at the same time drove a wedge between hydrogeology and cave and karst science (Watson and White 1985).

We conclude this discussion on the water-table debate with one last contribution to the literature that bears witness to this controversy and seeks a middle ground. In 1941, Rhoades and Sinacori published ‘Pattern of ground-water flow and solution’ in the Journal of Geology. There we find the following:

The deep underground flow postulated by Davis is strictly compatible with hydraulic theory. . . . However, Swinnerton advanced compelling arguments that many, if not most, caves have been formed at shallow depths. A reconciliation of the two views is possible on the assumption that the deep flow of Davis . . . diminishes in importance . . . as hydrologic adjustment of an area progresses, shallow ground-water flow becoming increasingly important as the degree of hydraulic adjustment increases and operating more effectively toward cave formation in all but the initial, unadjusted stages of the hydrologic cycle (Rhoades and Sinacori 1941, p. 786).

In other words, the two theories were not mutually exclusive. In this view, Davis’s processes dominate early speleogenesis and Swinnerton’s govern mature and late-stage cavern formation. To this day, many cave scientists agree with this concept.

Thus by 1941, a year that also marked the inception of the National Speleological Society in the US by a dedicated group of cavers, caver scientists, and cave scientists, the three broad classes of speleogenetic theories had been presented (White 1988, pp. 265–271; Ford and Williams 2007, pp. 222–223). One claimed that caves form deep within the phreatic zone. The second held that caves form near the water-table. The third maintained that caves form in vadose (unsaturated) environments (see Figure 4). All these theories were cast in terms of groundwater flow-paths and geographic cycles. They were based largely upon parochial

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19. It is impossible to overstate the importance of this elegant 159-page paper, which Hubbert wrote following his analysis of the subject in preparation for his lectures on groundwater in his full-year course on geology for mining and civil engineering students at Columbia University. The paper not only spelled out for the first time the correct field equations of steady-state flow and the proper context of Darcy’s Law. It made the crucial distinction between pressure and potential. It defined hydraulic conductivity. It laid the foundation for quantitative hydrogeology, using mathematical concepts within the reach of confident students in first-year physics and calculus.

20. Palmer (2007) provides a historical commentary of the influence of the National Speleological Society on the direction of cave and karst science.
observations. According to J Harlan Bretz21 “none [were] based on a really extensive acquaintance with caves” (Bretz 1942, p. 676).22

During the three decades that ensued, that basis changed incrementally as cavers extended and ‘connected together’ knowledge of known caves, traveled to new and distant locations to explore new caves, and shared ideas through increased globalization. A resulting awareness of caves by scientists, particularly hydrogeologists and geomorphologists, increased through publications, such as those by Bretz (1942), Malott (1949), Sweeting (1950), Warwick (1953), Kaye (1957), Thrailkill (1968), and Stringfield and Le Grand (1969), amongst many others. Of particular note, and where we draw this section to a close, occurred when Derek Ford (Ford 1971) and later Ralph Ewers (Ford and Ewers 1978) published the ‘four-state model’ for the development of limestone caverns.

The four-state model broke from the restraint of geographically limited observations and the ‘forestructure’ of theoretical geographic cycles, attributing the relationship between cave passages and the water-table to the geographic relief, the dip of strata, and the frequency of fractures. High relief, steep dips, and low fracture frequencies promote cave development deep below the water-table (States 1 and 2 of Figure 4b). Low relief, shallow dips, and high fracture frequencies lead to cave development along the water-table (States 3 and 4 of Figure 4b). This model featured exactly what Davis did not perceive: the concept of geologic control in the phenomenon of cave evolution. This concept could only come from awareness of variations from around the world.

21. J Harlan Bretz, so well known now for the Channeled Scablands debates of the 1920s when “cataclysmic flooding re-emerged as an important geomorphic process” (Baker 1998, p. 172; see also Baker 2008), was prominent in American caves and karst studies in the 1940s and ’50s, with a much-cited Journal of Geology paper on vadose versus phreatic features (Bretz 1942), an interpretation of the origin of Carlshad Caverns (Bretz 1949) and State survey bulletins on the known caves of Missouri (Bretz 1956) and Illinois (Bretz 1961). His interpretation of Carlshad, based on a ‘forestructure’ of dissolution by CO2-charged meteoric waters such as in Illinois and Missouri, has been discarded now, and Carlshad and other caves of the Guadalupe Mountains are one of the type examples of H2SO4 speleogenesis (Hill 1987).

22. Years later, in 1956, Bretz was invited by Heinz Lowenstam, an early leader in the field of paleoecology and at that time at Caltech, to help revise the Pleistocene stratigraphy of Bermuda as published by Robert W. Sayles (1931). The team consisting of Lowenstam, Bretz, and Robert Ruhe of Iowa State University worked out of the Bermuda Biological Station (BBS). Bretz, already a man of 73 years, apparently walked about the island visiting easy to access caves and outcrops. What followed was a publication in 1960 in the Bulletin of the Geological Society of America with the title ‘Bermuda: A partially drowned, late mature, Pleistocene karst’ (Bretz 1960). What particularly struck Steven Jay Gould (who did field work for his PhD [Gould 1969] out of the BBS soon after) about Bretz was the fertility of his mind. He looked at the Bermuda landscape and came up with an interpretation and a story, and that was the way he wrote his paper: a ramble informed by Lowenstam’s concepts from previous field seasons; basically field notes with a title added! (Gould, oral communication to HLV, 1968). The caves were a peripheral issue. However, Bretz did call Bermuda a karst. So he saw the topography and relief of Bermuda to be largely erosional and to have karst features. What is particularly striking, and specifically relevant to this essay, is that Bretz came up with an interpretation that was familiar. He used the same sort of model that he was familiar with from his early experiences with karst in Missouri—a model similar to that promoted by Davis. He saw the rolling topography of Bermuda’s eolian limestone as karst and not just the product of deposition. It was basically his ‘forestructure’; and he was not entirely wrong (Vacher 1978, pp. 219–223; contra Land, Mackenzie and Gould 1967, p. 999).
3.2. Overcoming temporal parochialism

Whereas the path to the four-state model shows the effect of geographic parochialism or sampling limitations with respect to geographic and geologic variations, a concept in speleogenesis that has been revived in modern times by Smart and Christopher (1989) reveals the influence of a temporal parochialism, or sampling limitations with respect to time. Studies within the Alpine karst of the French Alps (Audra 1994) and Switzerland (Jeannin et al. 2000) have provided additional evidence for speleogenesis in the epiphreatic zone: “the zone in a cave system immediately above the phreatic zone affected morphologically and hydrologically by floods too large to absorb at once” (Monroe 1970).

As stated in a PhD dissertation by Philipp Häuselmann in reference to studies in the French Alps:

[M]ost of the well-known, rounded passages that generally are attributed to having been created below the water-table, are in fact produced in the epiphreatic zone. . . . [O]nly in the epiphreatic zone is corrosion sufficiently strong to provide effective solubility (Häuselmann 2002, p. 97).

Regarding his own study in Switzerland, he wrote:

We conclude that the epiphreatic zone is responsible for the latest stage of tube morphogenesis in Alpine caves, whereas the initial formation of the tubes happens in the perennial phreatic zone (Häuselmann 2002, p. 163).
The issue of phreatic versus epiphreatic origin of cave features can be illustrated by the hazards of conditional propositions (if–then statements) as they pertain to the link between cave passages of elliptical cross-section (see Figure 5), conduits below the water-table, and pipe-full flow conditions. There is a predominant generalization in the interpretation of cave morphologies by cavers and scientists: conduits that enlarge below the water-table display elliptical cross-sections indicating pipe-full conditions of flow. There is no arguing with that statement. If a conduit enlarges below the water-table, the conduit is pipe-full. Therefore continuing dissolution around the entire wetted perimeter tends to result in an elliptical cross-section. The problem comes when the converse of the first conditional is stated: if the conduit is elliptical, and thus indicative of pipe-full conditions, then the conduit grew to that form below the water-table. The converse forces us to consider: what is the duration of time that these conduits spend in pipe-full conditions? “Both the period of record, as well as the time span under consideration, can be critical to the understanding of natural phenomenon” (Schumm 1991, p. 46).

![Figure 5. An elliptical passage in Wells Cave, Kentucky, some eighteen meters above the present water-table. Passages of this sort are traditionally interpreted as having developed in phreatic conditions. However, many people now view such passages as having developed in epiphreatic conditions where the passage is only filled with ‘chemically aggressive’ water during high-flow events that comprise a small fraction of the passage history (photograph by John Agnew, 1989).](image)

Epiphreatic speleogenesis acknowledges the above question and emphasizes that pipe-full conditions occur occasionally above the water-table—during floods associated with snow melts and rainy seasons, for example. These high-water and chemically aggressive conditions are regularly called upon to explain morphologic features described in caves such as the ‘bypass passages’ of Ford (1968) or the ‘flood-water mazes’ of Palmer (1975). Hence, growth of a conduit below the water-table is a sufficient, but not necessary, condition for elliptical cross-sections.
The development of passages with elliptical cross-section in both the phreatic and epiphreatic zones is an example of convergence. In his discussion on the problem of convergence in Nature’s geological signs, Schumm explains that:

[Similar results from different processes make interpretation of earth surface features difficult, and therefore, a fragmentary record from the geologic past or limited observations at the present may be an inadequate base upon which to postdict or predict (Schumm 1991, pp. 61–62).]

Palmer and Audra (2003) expand upon the epiphreatic concept, recasting Ford’s four-state model in terms of tectonic adjustment or aggregation for State 1 (see Figure 4b) and conduit development within the epiphreatic zone for States 2, 3, and 4 (Figure 4b). As part of developing this concept they invoke the Messinian low sea level in the Mediterranean during the Miocene as the mechanism for the development of certain caves that are now deeply phreatic, such as Vaucluse Spring in France. They also cite as evidence patterns of vertical loops and lower bypasses (the soutirages of Häuselmann 2002) for air-filled cave passages within Alpine regions of France and Switzerland, where water levels in flood events may span more than a hundred vertical meters. However, analogous to Rhoades and Sinacori (1941), it is important to note that the argument presented here may not necessarily apply to the inception phase of speleogenesis when “there is insufficient effective porosity for [the water-table] to be lower” (Ford and Williams 2007, p. 223).

Limited observations in space and time are pertinent to the modern consideration of epiphreatic speleogenesis in two ways. First, we reflect upon the ‘forestructures’ of cavers and scientists presented in Section 2. If a passage is too small for a caver to explore, it might as well not exist—an anthropocentric conundrum explored by Curl (1986) in the context of caves as natural fractals. Therefore, following the logic of Figure 1, a scientist decoding the data from a caver will be unaware of these smaller passages that may be important routes for subsurface water. This is, in effect, a form of geographic bias that may impede interpretation.

The second effect of limited observations occurs because cave exploration of epiphreatic passages happens during times of low water, as opposed to during floods when the passages are lethally pipe-full of storm or melt water. But during times of cave exploration, epiphreatic passages may be dry (such as the one in Figure 5) or only occupied by small free-flowing streams. This form of temporal bias may skew how we view the function of a particular cave passage. For example, one may call a cave passage ‘relict’ or ‘abandoned’ when in fact it remains an active, if infrequent, part of the flow system and is therefore continuing to enlarge.

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23. Another example of a feature attributed to the phreatic zone that is reinterpreted to have formed also during epiphreatic floods are vertical loops, where flow descends and then rises toward a spring. The epiphreatic development of these features is discussed in detail in Chapter 4 of Häuselmann (2002).

24. Ford and Williams (2007, pp. 226–227) provide counter-examples including the El Abra in Mexico and Castleguard Cave in Canada, which cannot be explained using the model proposed by Palmer and Audra (2003).

25. “[I] do not doubt that many passages are ‘enlarged’ during floods; a small proportion of them are ‘created’ in floods, almost always as secondary segments that appear later in time. The critical question is whether the looping patterns of the great majority are ‘initiated’ (constructed to ‘breakthrough’ dimensions) in permanently phreatic conditions or not. I contend that they are—[and] in most cases there can be no doubt about it” (Derek Ford, personal communication, 2010).

26. Price Valley Cave within Sinking Valley in southeast Kentucky provides an example of an ideal water-table cave (State 4 in the four-state model) that may be interpreted as an epiphreatic cave. Passages within this cave are elliptical even though large portions are mostly air-filled. Survey data suggest that collapse features may impound water and result in the phreatic components that segment this cave from others. Indirect reports reveal the influence of floods on this cave. Aside from the trunks of trees wedged into fissures in the ceiling, Simpson (2004) describes a well drilled into Price Valley Cave that “spouted water 20 feet into the air during a 1981 flood pulse” (Simpson 2004, p. 105). The cave passage lies approximately fifty feet below the surface at this location.
Even with these challenges, recent geochemical and hydrologic data suggest that post-inception speleogenesis, even in non-alpine Kentucky, is dominated by the infrequent storm events. For example, while investigating the temporally varying dissolution kinetics of cave waters in the main river sections of the Mammoth Cave karst aquifer, Groves and Meiman (2001) found that waters in the cave stream were capable of dissolving limestone during only 31% of the year, when floodwaters rise to fill to the ceiling passages that are otherwise free-flowing streams. These and other forms of evidence lead some to suggest that various dissolution features and passage morphologies previously attributed to the phreatic zone in many caves are in fact the result of water-level fluctuations in the epiphreatic zone (Palmer 2001).

Within the context of cave and karst science, it appears that the epiphreatic speleogenesis has brought us full circle, back to Cvijić’s periodically saturated zone from Section 3.1. While certainly applicable to many settings, we must remain fully aware of Martel’s message:

No theory about the origin of caves is universal: those that have been put forward have generally claimed to be too inclusive; almost all of them are partially correct; the whole truth lies sometimes in the application of a particular one in a particular case (Martel 1896, pp. 53–54).

Recent discussions of epiphreatic speleogenesis have, for some, drawn some distinctions with the original formulation of the four-state model (Jeannin et al. 2000). On the other hand, computer modeling (Dreybrot et al. 2005 and references therein) supports many aspects of this speleogenetic model. Regardless, the appearance of Ford’s theory in the 1970s was a milestone in the history of cave and karst science because it broke the hold of Davis’s armchair paradigm and drew from cavers, both amateur and scientist, knowledge of accumulating hydrogeologic diversity. Additionally, it appeared at the end of a long hiatus in prominent karst research that followed an unfortunate problem of communication between cave scientists and hydrogeologists (Watson and White 1985). In the end, we may come to the conclusion that the observations from new, increasingly accessible locations in our globalized society will serve to enhance, rather than contradict, this conceptual framework.

4. CONCLUDING REMARKS

Vacher and Rowe (1997) analyzed the evolving thought about Bermuda’s Pleistocene sea-level history in terms of the hermeneutic concept of ‘forestructures’ as brought to the attention of geologists by Frodeman (1995). That analysis led them to argue that “Bermuda’s Pleistocene sea-level history needs to be examined without applying foreknowledge of how high sea-level must have been from coeval deposits at other places, and other extra-Bermuda considerations” (Vacher and Rowe, 1997, p. 70). They concluded with the recommendation that “there be

27. Closing the Bretz thread in these footnotes, it is appropriate to point out that Bretz’s Spokane floods and Channeled Scablands have led to a new understanding of uniformitarianism versus catastrophism (e.g., Baker 1998) that is relevant to the flooded-passages of epiphreatic speleogenesis and one’s inability to observe those places in those times. From a view in the early nineteenth century characterized by “No causes should be invoked in our geological reasoning unless they have real existence (i.e. we have directly observed them) and that they be adequate to produce the purported effect” (Laudan 1987, in Baker 1998, p. 175), we have the great term “actualistic catastrophe” (Hooykaas 1970, in Baker 1998, p. 176). “The term ‘catastrophic’ only applies to the intensity and duration of a particular geological process. It does not necessarily have anything to do with whether or not such a process is manifest today (actualism) or even with the well-known methodological claim that simpler explanations are to be preferred to more complex ones” (Baker 1998, p. 180). Putting scale and frequency of the phenomena aside, there is a parallelism between the expectable flooded passages above the water-table during times of great storms and snow melt, and the “giant glacial outburst floods [that] are now recognized as characteristic of the terminal phases for the immense ice sheets that covered much of North America and Eurasia in the ice age (Baker 1998, p. 172).
more descriptions and analysis of actual exposures . . . than continued argumentation about how Bermuda proves out one or another geological world view” (p. 85).

In this paper, we have applied the perspective of hermeneutic ‘forestructures’ to a much broader landscape, the evolution of thinking of caves in relation to the water-table, and have come to a basically similar result but with the additional insight of the role of communication amongst disparate segments of the community. In particular, studying caves and karst involves fieldwork that is complex, strenuous, and time consuming. Large fractions of the known data have been collected by amateurs and explorers rather than scientists, who may be well versed in caves and karst, but may have virtually no practical experience in that field of study. The hermeneutic circle of Martin Heidegger and later Robert Frodeman tells us to expect that understanding caves and karst is an iterative process, fundamentally guided by preconceptions, goals, and tools. So cavers and scientists, who depend on one the other, may have vastly different ‘forestructures’, with the result that problems of communication will inevitably occur where these groups make contact, as in the records of cavers’ explorations and scientists’ research publications.

Difficulties in communication plague cave and karst science even to the present. Researchers have been, and still are, inclined to think within the limits of their own experience and in consequence they cast their understanding of karst into regional as opposed to global theories. It was the hard work of individuals such as E. A. Martel, who strove to gain very extensive empirical experience and promote communication between scientists around the world, which helped cave and karst science endure.

We have also seen how the synthesizing power of a respected geomorphologist drove the direction of cave and karst science for many decades. By his own admission, William Morris Davis had little knowledge of caves and karst. With limited data of questionable quality, he designed a theory that persisted in the face of considerable criticism and conflicting data, conveniently explaining caves and karst in terms of his cyclic worldview of physical geography.

Finally, we have investigated parochialism in theories of speleogenesis not only from the perspective of datasets that lack a sufficient spatial extent and density, but also limited temporal extent and density. As one example of how karst scientists can move past spatial parochialism, we have reviewed a well-respected theory in karst science that relates simple geologic parameters to the position of caves relative to the water-table—the ‘four-state model’ of Ford (1971) and Ford and Ewers (1978). With regards to ‘temporal parochialism’, we have discussed epiphreatic speleogenesis driven by high water levels in floods, and how convergence of phenomena may produce a similar end result—cave passages of elliptical cross section—assumed a priori by some to indicate phreatic speleogenesis, or cave development below the water-table. Recent data demonstrates that in many karst aquifers, including alpine systems and at least some in Kentucky, the enlargement of cave passages tends to proceed in a punctuated manner, driven by floods.

5. APPENDIX

THE CONCEPT OF ‘WATER-TABLE’ IN KARST

Water-table is a crucial concept in this essay, as it is in the history of cave and karst science generally. In basic terms, the water-table is defined as the top of the zone of saturation. All the pores within the rocks are filled (saturated) with water in the saturated zone. The gauge pressure is zero at the water table (where fluid pressure is equal to atmospheric pressure). Below the water-table is the phreas or phreatic zone where pressure increases linearly with depth. Above the water-table is the unsaturated (or vadose) zone. Water in the phreatic zone is called groundwater. Water in the unsaturated zone is called vadose water, which includes soil
water in the upper part of the unsaturated zone. Collectively, groundwater and vadose water are termed *subsurface water*.

Much of the history of cave and karst science has involved a debate over the extent to which caves originate and grow in the phreatic zone or in the vadose zone; how particular cave features formed in the respective zones can be recognized; and even whether there is such a thing as a continuous water-table in a region of caves and karst. For example, in this manuscript, we have stated in Section 3.1 that E. A. Martel’s caving experience in the mountainous regions of Europe during the late 1800s and early 1900s led him to the conclusion that caves form in the vadose zone and he therefore concluded that no water-table exists in karst.

How, one might ask, can there be no water-table? The answer hinges on one’s view of the hydraulic properties of the rocks between and above caves. If the rocks surrounding a cave are porous and permeable, then water fills the porosity, including the caves, to a height within the subsurface determined by the supply of water flowing through it. A water-filled cave is simply a large pore within the saturated porosity of the subsurface. If, on the other hand, the rocks surrounding the cave are non-porous to the extent that they do not hold water, as is nearly the case with some dense, crystalline limestones and marbles, a water-filled cave can be surrounded by dry rock. When one drills in such a place, one does not encounter water unless the drill-hole intersects the cave. Then, water rises in the well to some height above the cave determined by the hydraulic head of the water in the cave passage. Technically, this level is known as the piezometric surface, and at that height, the water at the water level in the well is at atmospheric pressure.

According to the modern view, there is always some porosity (limestone porosity ranges between less than 1% to more than 40% of the total rock volume), and so subsurface water can always form a water-table; the motion of water in that rock, however, concerns the magnitude of the permeability—how easily the water transmits through the pore spaces. Are the porous rocks sufficiently permeable to allow the level of the water outside the caves to rise and fall with the changing conditions of supply within an observable time frame?

In our examples from Kentucky, where the limestones are relatively old and cemented, the permeability of the rock is very low and so changes in water pressure in a water-filled cave, such as during flood events, do not penetrate easily into the surrounding limestone. As a result, these flood waters rise quickly into overlying, less active or relict passages within the karst aquifer. The resulting epiphreatic zone as described in Section 3.2 may therefore become vertically extensive—for example up to 400 m thick, as noted by explorers of Krubera Cave under the Aribeka Massif in the Ukraine. In contrast, the relatively young limestones of Florida are as much as 100,000 times as permeable as those of Kentucky. Water transmits readily between the cave and the rock. The relative differences between the karst hydrology of very permeable and nearly impermeable limestones have been a subject of considerable investigation. See Florea and Vacher (2006) for a review of the subject.

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