

The Great Debate in Mid-Twentieth-Century American Geography: Fred K. Schaefer vs. Richard Hartshorne

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Introduction

Since geography's establishment as a university-based discipline in Western Europe during the mid-to-late nineteenth century its methodology was primarily descriptive. Geography relied on the gathering and recording of raw empirical detail, displayed as factually laden essay-style prose, assorted typologies, tables of numbers and perhaps most importantly maps. There was very little of what we would now call theory, explanation or analysis. The heyday of geography's descriptive methodology was during the 1920s and 1930s. In an era when the Internet was a distant future, compilation and curation of relevant facts on a subject was a valid academic endeavor and arduous work. And in many places in the world geographers commanded respect for their ability to synthesize knowledge about distant places. Nevertheless, there grew an increasing sense of an intellectual gap between descriptive geography and the methods of other disciplines. This was certainly the case for the physical sciences that were rigorously theoretical, quantitative, and often experimental, but also social sciences like economics, political science, sociology, and psychology. In the social sciences, change had been especially afoot from the 1930s, and once the Second World War ended, those disciplines became increasingly like the physical sciences, deploying formal theory, pursuing generalization, carrying out measurement using quantitative data and strictly defined statistical techniques for testing and verification (Mirowski, 2001; Abbot and Sparrow, 2007). Carl Schorske (1997, 295) called this move by those social sciences, "the new rigorism", a "passage ... from range to rigor, from loose engagement with a multifaceted reality historically perceived to the creation of sharp analytical tools that could promise certainty where description and speculative explication had prevailed before".

But not so much in geography, despite its claim that it was the discipline *par excellence* to bridge natural and social worlds. The discipline seemed rather lightweight and naïve, continuing to peddle "mere" description, compiling dull gazetteer lists of facts. Neil Smith (1989) likened the state of at least American geography until the early 1950s to a museum-like existence; in effect, it was preserved under glass, unchanging, stuck in a past era. The methodological debate that we review in this chapter set in the early post-Second World War period changed all that. It was an iconoclastic moment when the museum display cases were smashed open, and the present came

storming in. The change that the debate wrought was fundamental. For those participating, it did not involve just tweaking the existing methodological position around its edges, but invoked vast wholesale transformation. Louis Althusser (1972, p. 85) used the (geographical) metaphor of discovering a new continent to denote a fundamental alteration in knowledge acquisition. The debate we review here was about discovering a new continent by a young generation of geographers, opening territory they thought was never explored before, producing possibilities never previously imagined.

Lasting between 1953 and 1959, the debate was between Fred K. Schaefer (1904–1953) at the University of Iowa and Richard Hartshorne (1899–1991) at the University of Wisconsin 180 miles (290 kms) up the road. For these two protagonists, the dispute was about the very soul of geography. It turned on fundamental questions around the nature of geographical practice, the correct method, and the definition of disciplinary progress. Bill Bunge (1928–2013) (1968, p. 12), an acolyte of Schaefer, but early on a student of Hartshorne, thought the controversy was *the* historic battle of the discipline, likening the clash to that “between Michelson and Newton, or Hegel and Feuerbach”.

For those like Bunge who framed Schaefer versus Hartshorne as geography’s “great debate”, the central issue was about the appropriate geographical method. Should it stay with its historically descriptive method, known as the idiographic approach, that focused on the assembly and careful arrangement of unique geographical facts, Hartshorne’s view? Or, instead, should it be like the physical sciences and some social sciences and seek laws expressed mathematically and as rigorously defined abstractions, a nomothetic approach, and Schaefer’s view?¹

In reviewing Anglo- American geography’s mid- twentieth century *great methodological debate*, we divide the chapter into three sections. First, we discuss the state of Anglo- American geography before the debate broke out in the early 1950s as well as introducing its *dramatis personae*, Schaefer and Hartshorne. Second, we provide an account of the dispute, starting with Schaefer (1953), followed by Hartshorne’s (1954, 1955, 1958, 1959) responses. Finally, we discuss the aftermath and disciplinary consequences.

Context and *Dramatis Personae*

A key methodological dualism in geography is between “systematic” and “regional” geography first articulated by Varenus in 1650 (Hartshorne, 1939, p. 41). If geography is about understanding variation on the earth’s surface, should the geographer prioritize general mechanisms causing geographic difference – systematic geography – or begin by detailed documenting of areal variation – regional geography?

Systematic geography was originally defined by analogue to “systemic science” as the search for “general laws” (Hartshorne, 1959, p. 109). As the discipline specialized, systematic geography was subsequently understood as thematic specialization such as economic, transportation, or political geography (Ackerman 1945), although the older causal connotation remained resonant (Schaefer, 1953). Both Hartshorne (1939, p. 456– 459) and Schaefer (1953) acknowledged that

¹ The distinction between idiographic and nomothetic was first made in the late nineteenth century by two German philosophers, Wilhelm Windelband and Heinrich Rickert (Staiti 2013). They divided disciplines into two kinds: the idiographic was concerned with the unique (history was their exemplar); and the nomothetic concerned with making generalisations, the ultimate of which was a scientific law (their examples were chemistry and physics).

both systematic and regional perspectives mattered for geographical practice. They differed, though, in their assessment about which approach should be the ultimate disciplinary goal and justify geography as a “science”.

During the second quarter of the twentieth- century, American geography was dominated by regional geography. The earlier search for causal geographical laws had been discredited around the 1930s, associated with a crude environmental determinism linked to racist Western supremacy theories (Martin 2015). The prime methodological goal for Hartshorne’s generation became the preservation of geography as a science while purging a malignant environmental determinism. To do so they changed the object of study of geography from the causal relationship between the environment and humans to the synthetic study of the region. But what kind of scientific object was the region, and methodologically how should it be studied? These were key questions for Hartshorne and his peers (Martin 2015, Chapter 15).

Their problem was that the synthetic “science of regions” did not always produce appealing studies. John Leighly (1937, p. 127) lamented that regional geography was often “a vision of the whole surface of the earth plastered with topographic descriptions – like the luggage of a round-the- world tourist with hotel stickers ... [It] terrified even the most tolerant reader of regional descriptive literature”. Instead, Leighly (1937, p. 131) proposed that regional geography be an “art” in which the “skill and intellectual integrity” of the geographer-artist created a synthesis of regional features.

Hartshorne disagreed with Leighly that geography’s methodological problem could be resolved by an appeal to art. Instead, he argued resolution would best come from studying recent developments in German geography. In fact, American geography had been largely erected on German foundations, but direct intellectual exchange between geographers from the two countries dried up after the First World War (Martin 2015). Hartshorne, who spoke German and met many German geographers during a year of fieldwork in Germany in 1931 (Martin 2015, p. 890), thought Germanic foundations remained useful in addressing geography’s key methodological problem. That this usefulness was not already recognized, he believed, was a result of bad translations of German works and inept interpretations. After voicing his dissatisfaction to Derwent Whittlesey, the editor of the *Annals of the Association of American Geographers*, Hartshorne was invited, “to write a “statement ... it can be brief” (Hartshorne 1979, p. 63).

Hartshorne got down to work. He believed once he finished, there would be no more misunderstandings. But his statement got quickly out of hand. In April 1938, it was 61 manuscript pages (Hartshorne 1979, 70). By July 1938, on the eve of Hartshorne’s academic leave to Vienna where he planned to study boundary issues in the mid- Danube region, it had grown to 194 pages (Hartshorne 1979, 71). The Nazi *Anschluss* that occurred in March 1938 put paid to Hartshorne’s plans for fieldwork in that country. Instead, he hit the library stacks at the University of Vienna, giving him access to many German-language sources unavailable in the US (Martin 1995, p. 906). When the manuscript was finally completed in April, 1939, in Meilen, Switzerland, where Hartshorne had gone for safety fearing a German war with Poland (and for good reason), it weighed in at over 600 pages (Hartshorne 1979, p. 73). This hefty manuscript became *The Nature of Geography: A Critical Survey of Current Thought in the Light of the Past*, whose publication required two full issues of the *Annals of the American Association of Geographers* (Hartshorne 1939). The volume meticulously explicated, rigorously justified, and genealogically fixed the discipline like no other English language volume before it.

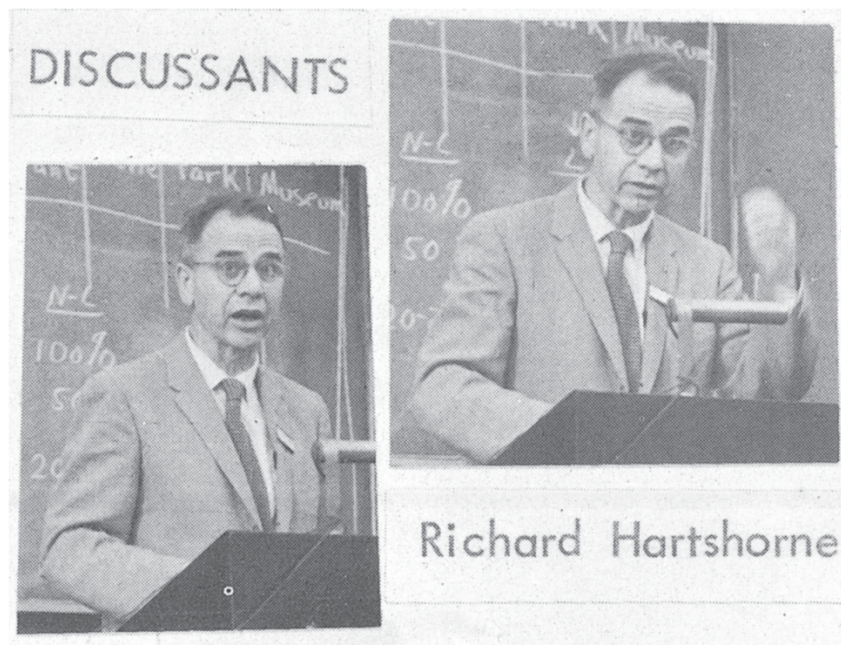


Figure 1.1 Richard Hartshorne defends the regionalist position at the 1960 Lund seminar in urban Geography, 16 August 1960 (Photos: Chauncy D. Harris)

Nature conceived geography as a science, although Hartshorne recognized that it was a different kind of science compared to the systematic sciences that he variously also called the “exact sciences,” “natural sciences,” or “physical sciences” (Hartshorne 1939, p. 367). Hartshorne leaned on the German definition of science, *wissenschaft*. It implied a broad systematic pursuit of knowledge, learning and scholarship, but not the imposition of a singular methodology or set of ends. The Anglophone conception was narrower, conceiving science as concerned with generalization and the ultimate generalization, a law. For Hartshorne, under the broader German *wissenschaft* definition, geography was a science but it did not necessarily pursue generalizations and laws. As he elaborated in *Nature*, the science of geography was defined by the objective understanding of complex geographical phenomena made possible by precise description, meticulous citation practice, and expertise in relevant methods including fieldwork, cartography and statistics.

Given that definition, Hartshorne (1939, p. 468) believed that the science of geography was best realized by regional geography; that is, by the description of regional or areal difference. Systematic geography could help in identifying factors that contributed to areal differentiation, but as a pursuit it was secondary to regional geography.

Regions therefore became the building blocks of the discipline, with all geographical information organized through them. Note, though, that for Hartshorne the regional division of the world was not a “naively given fact” (Hartshorne 1939, p. 275). Rather, the researcher imposed regional demarcation. As Hartshorne (1939, p. 275) put it:

Regional entities [...] are ... in the full sense mental constructions; they are entities only in our thoughts, even though we find them to be constructions that provide some sort of intelligent basis for organizing our knowledge of reality.

Specifically, Hartshorne defined a region as a complex combination of hard facts and causal relations objectively described (Entrikin, 1981). He called those combinations an “element complex”. Importantly, their combinatorial character made each region unique, not found anywhere else in the world (Sack, 1974, p. 441). To use Hartshorne’s illustration, while several regions might share one common element, say, producing grain – for example, the Po plain, the Middle Danube plain, and the American Corn Belt – this did not then make those regions identical. This is because none of them shared in exactly the same combination all the other objective geographical elements found in those specific grain producing regional complexes (Hartshorne, 1939, p. 392). Accordingly, each region “occurs but once on the earth” (Hartshorne, 1939, p. 393). Therefore, “regional geography ... is ... concerned with the description and interpretation of unique cases...” (Hartshorne, 1939, p. 449). While the region is the core idea in Hartshorne’s approach, he nonetheless recognizes that systematic geography might still be useful in determining the components and their relationship within them (Hartshorne, 1939, p. 457).

A critically important methodological corollary stemmed from Hartshorne’s claim of regional uniqueness. As discussed, unlike the German *wissenschaft*, the anglophone tradition equated science with the formulation of general laws. Such laws, however, cannot be expressed for combinatorial entities like Hartshorne’s region. For a scientific law to be stated, the entities on which the law bears must be homogenous. Because regions are not a uniform class of phenomena – each is unique – then no law can ever be derived.

Because for Hartshorne regions are never the same, the making of law- like statements that define Anglophone systematic science cannot apply; it has no purchase. As a result, as Hartshorne (1939, p. 446) noted, “we arrive, therefore, at a conclusion similar to that which Kroeber has stated for history: ‘the uniqueness of all historical phenomena... No laws or near laws are discovered.’ The same conclusion applies to the particular combination of phenomena at a particular place.” Thus, for regional geography, unlike systematic geography, we cannot explain, or predict, or knowingly intervene but only describe: “Regional geography, we conclude, is literally what its title expresses: ... a descriptive science” (Hartshorne 1939, p. 449).

Although Hartshorne (1939, p. 430) surmised that a generic classification of regions might one day progress “to the statement of general principles [i.e., laws]”, he also warned that “any principles we attempt to develop can have no more validity than the ‘objects’ [i.e. regions] we have constructed as their foundation”.

The publication of Hartshorne’s book represented a crossing of the Rubicon for English language geography. Never before had there been a book published like it with the ambition both to define the discipline historically and to lay out a meticulously justified methodology. Hartshorne’s star necessarily rose. In 1940 he moved from the University of Minnesota to become full Professor at the University of Wisconsin, Madison. The next year he was appointed Chief of the Geography Division at the Research and Analysis Branch of America’s military intelligence organization, the Office of Strategic Services (OSS) (Barnes and Farish 2006). In 1949 he became President of the Association of American Geographers, the professional organization of the discipline within the US. Consequently, by 1953 at the start of the Schaefer- Hartshorne debate, he was likely America’s most well-known geographer. That said, Hartshorne’s methodological position enunciated in *Nature* was not uniformly accepted or praised. Some of the younger geographers, including those he hired at OSS, such as Edward Ackerman (1911–1973), thought Hartshorne’s regional geography made geographers a jack of all trades and master of none (Ackerman, 1945).

Hartshorne's fame was in stark contrast to the man who debated him, Fred K. Schaefer. Very few had heard of him. Hartshorne (2004a, p. 277) later said he was "an essentially unknown professor of geography". Schaefer was trained as a metal worker in Berlin immediately after the end of World War I. In 1925 he went back to school as a mature student and in 1931 he graduated from the University of Berlin with a Diploma primarily in economics. His politics were left-wing. When the Nazis came to power in 1933 that was enough for him to be blacklisted, then arrested and sent briefly to a concentration camp. The writing was on the wall. Later that same year he obtained a permit to travel to Switzerland for a skiing trip. He never returned. A political refugee in England, 1933–1938, he worked primarily for various left-wing organizations collecting data and carrying out statistical analyses. In 1938 he left for America, moving to the Scattergood Hostel for European refugees run by a Quaker society, the American Friends Service Committee, in Iowa City, Iowa. After a short stint as a house painter, in 1939 he began to teach part time at the University of Iowa, Iowa City, and because of his training in economics he lectured at the College of Business. Harold McCarty (1901–1987), a Professor at the College of Business, and the future founding chair of the Geography Department, invited him to teach courses on Eastern Europe. In 1943 Schaefer was hired full time in Business, and appointed Assistant Professor. Fluent in English, Russian and German, interested in geopolitics, and having taken courses in political and economic geography in Berlin, in 1946 Schaefer was asked by McCarty to become a member of the brand-new Iowa Department of Geography.

While Schaefer may have been a novice geographer, it did not deter him from being David, trying to slay the disciplinary methodological Goliath, Richard Hartshorne. Schaefer's slingshot was a version of logical positivism developed by members of the Vienna Circle during the 1920s (see Sigmund, 2017, for a situated introduction). Logical positivism claimed that there were only two kinds of meaningful knowledge. The first were empirical truths verified by methods of the systematic sciences. The second were truths that were correct because of the very meaning of the terms in which they were expressed such as logic or mathematics. Logical positivists believed that unless claims to knowledge met either of these two criteria, knowledge was unreliable and spurious. Further, for logical positivists, the highest form of empirical knowledge was a universal law. It took the logical form: if cause A, then for all time and all places the same effect, B. For example, Newton's law of gravity says *if* a pair of planetary masses *then* for eternity and everywhere the gravitational force between them is proportional to their respective sizes divided by the square of the distance that separates them.

It was logical positivism that Schaefer critically brought to bear on Hartshorne's *Nature*. In doing so, he got inside help from a colleague in the University of Iowa's Philosophy and Psychology Department, Gustav Bergmann (1905–1987) (Heald, 1992). Bergmann was a member of the Vienna Circle, originally a mathematician, later a philosopher. Smoothing the relationship between the two men was also that they were a similar age (Schaefer was a year older), were native German speakers, were political refugees from the Nazis – Schaefer because of his left-wing political views, Bergmann because he was Jewish – were employed by the University of Iowa in the same year, 1939, and perhaps most importantly, were unreservedly champions of systematic science.

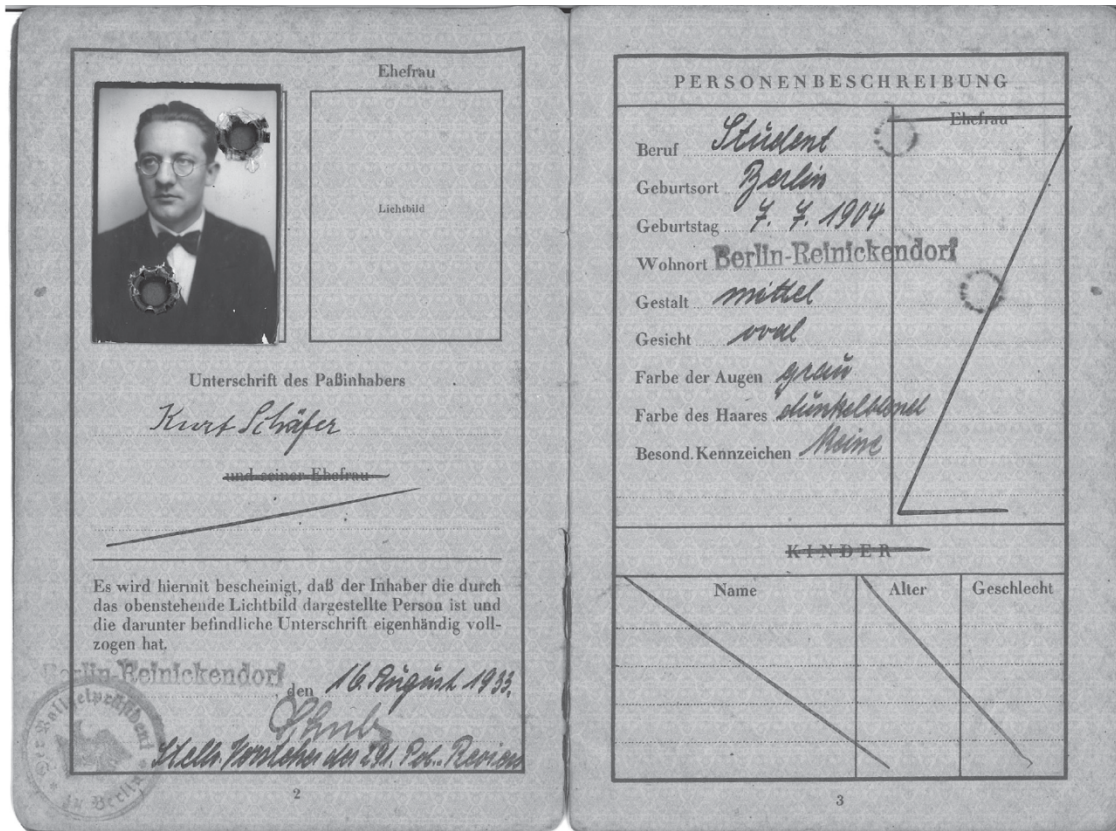


Figure 1.2 Kurt Schaefer's 1933 ID he brought with him as a refugee (Collection: American Geographical Society, AGS- NY AC1 - Box 338 - Folder 7)

Going to this last point, for Schaefer the fundamental problem with Hartshorne's approach to geographical knowledge, as understood by logical positivism, was its unscientific character. And if it was not scientific, necessarily it was unreliable and spurious. For Schaefer, Hartshorne had denied the possibility of scientific geographical knowledge when he denied geographical laws. In doing so, he consigned the subject to a study of the unique and the exceptional, an idiographic discipline. In contrast, Schaefer aimed to make the subject a systematic science, capable of formulating its own laws, a nomothetic discipline. For Schaefer, the scientific task of geographers was to discover laws that explained and predicted the spatial distribution of phenomena. They would complement the laws in economics, sociology and other social sciences (Schaefer, 1953, p. 248). From Schaefer's perspective, Hartshorne's (1939, p. 551) claim that geography was scientific because of its unique "point of view, a method of study" was rejected. If regional geography could not generate laws, it was simply not a science. Claiming to be a special discipline was an appeal to "exceptionalism". For this reason, regional geography could not be the scientific core of a discipline. It was at best only a data vault to inform systematic geographical inquiry (Schaefer, 1953).

The Debate

Strictly speaking the Schaefer-Hartshorne debate was not a debate. By the time Schaefer's (1953) "Exceptionalism in geography: a methodological examination" was published in the *Annals of the Association of American Geographers* in September, 1953, its author had been dead for three months. Consequently, Schaefer was in no position to hold up his end in any argument. He suffered

a fatal heart attack while attending a matinee at an Iowa City movie theatre in early June of that year. While Schaefer may not have gotten to defend himself against the series of ferocious rebuttals that Hartshorne subsequently published in the *Annals* in 1954, 1955, 1958 and in another monograph in 1959, the consensus is that he carried the day anyway, at least for a while. Clyde Kohn (1972, p. i), likely one of the original referees of Schaefer's paper at the *Annals*, reflected in 1972:²

Schaefer's article must be cited as one of the more important contributions to geographic methodology in the history of our discipline. The "revolution" which followed in the late 1950s, and the continued search since then for laws containing spatial variables, demonstrate the vitality and challenge of the ideas set forth by Schaefer in 1953.

Schaefer initially met Hartshorne the first year he was a geography professor at Iowa. They were on a panel together at a regional economics conference in Chicago talking about the Soviet Union. Hartshorne remembered only pleasantries over a cup of coffee, with Schaefer saying that he was interested in his "work in methodology which he would like sometime to discuss ... at length."³ That opportunity came four years later when Hartshorne was invited by McCarty to the Iowa Department of Geography to participate in among other things Schaefer's graduate seminar on methodology where he was asked to speak about *The Nature of Geography*. Schaefer acted as a facilitator, with Hartshorne kept on his toes by sharp but always polite questions from the well-informed graduate students⁴. Schaefer wrote to Hartshorne after the event congratulating him on his "fortitude" and "splendid response to the questioning."⁵ Hartshorne later reflected, "we all seemed to enjoy ourselves."⁶

Hartshorne's next encounter with Schaefer was less enjoyable. In October, 1953, he picked up from his mail box at Science Hall at the University of Wisconsin, Madison, a copy of the latest edition of the *Annals* to read Schaefer's paper that was just published. Schaefer submitted his paper on December 4th, 1952. According to Bunge (1968, p. 20), before Schaefer sent off the paper, he had walked into Harold McCarty's office, and with his hands "trembling" put the paper on his desk and said, "This is my existence in geography". Although this anecdote may be apocryphal, it expresses at least for some the elemental power of Schaefer's paper and its significance for the discipline.

It was only once Hartshorne began reading the paper that he first learned of Schaefer's death. It did not soften his reaction, however. He was incandescent with rage, but also nonplussed. The paper violated all the rules on methodological debate he had so meticulously set out (Hartshorne, 1948). There were so many things that he did not understand including the word "exceptionalism"

² There is no definitive proof that he submitted a review. Unlike the other two reviewers, Edward Ullman, and Stephen Jones, Kohn's review if it existed, has not survived (Martin 1989).

³ "Summary," August, 1969, p. 1, Richard Hartshorne Papers (**RHP**), Box 192, Schaefer and the origins of exceptionalism, file E, Library of the American Geographical Society, University of Wisconsin, Milwaukee.

⁴ The essay of one of Schaefer's students, miss Martha Corry, made such an impression on Hartshorne that her paper was taken back to Madison to be discussed and commented by Hartshorne's own graduate students, including David Lowenthal, who was studying at the history department. Correspondence from 1950 shows that much of her critique echoes the concerns that Schaefer would publish three years later. Subsequent communication after the debate broke suggests that Hartshorne even contemplated that Schaefer had plagiarized his student's essay. See **RHP**, Box 6, Folder 8, Schaefer's seminar at Iowa, 1950-1954.

⁵ Fred K. Schaefer to Richard Hartshorne, May 17, 1950, **RHP**, Box 192, Schaefer and the origins of exceptionalism, file B.

⁶ "Summary," August, 1969, p. 1, **RHP**, Box 192, Schaefer and the origins of exceptionalism, file E

in Schaefer's very title. He tried to look it up in the *Encyclopaedia for the Social Sciences* but "there were four or five different meanings, so that wasn't very helpful" (Hartshorne, 2004b [1986], p. 294). Schaefer, though, was clear in his paper about its definition. It was the view that because of their subject matter some disciplines were unable to formulate scientific laws about the material they studied. Of course, this was Hartshorne's view about regional geography.

In the standard model of explanation and prediction in systematic sciences, the existence and identification of scientific laws are crucial. Once a law is established it is used either to explain or predict. Explanation and prediction are flip sides of one another, both the consequence of having established a scientific law. But because Hartshorne denied for regional geography the possibility of scientific laws, there could be neither explanation nor prediction. It was against such exceptionalism that Schaefer's paper railed.

Specifically, Schaefer's critique of Hartshorne followed two strategies. The first was to counter Hartshorne's interpretation of the German historical writings that justified the definition of the region as unique. If Schaefer could show that those German authors on whom Hartshorne relied did not define the region as unique, he could argue that there was no reason to abide by exceptionalism. Regional geography could then join with other systematic sciences and seek laws, as well as explain and predict and not merely describe.

The second was to make explicit the kind of regional geography that was possible once the discipline's "exceptionalism [was] disposed of" (Schaefer, 1953, p. 242). It would be a regional geography unified with, not separate from, the systematic sciences, law-seeking and concerned with explanation and prediction. Here Schaefer (1953, p. 239) imagined the kinds of laws that geographers might discover:

Spatial relations among two or more selected classes of phenomena must be studied all over the earth's surface in order to obtain a generalization or law. Assume, for instance, that two phenomena are found to occur frequently at the same place. A hypothesis may then be formed to the effect that whenever members of the one class are found in a place, members of the other class will be found there also, under conditions specified by the hypothesis.

The prose is stilted, but Schaefer is setting out here the structure of a geographical law conforming to the classic configuration within logical positivism, if A, then B. What makes a law geographical is that the instances making up the homogenous classes A and B are indexed by location. In Schaefer's example, every case of A occurs at the same geographical location as every case of B. That is, there is something about the geographical location that ensures, if an instance of A, then an instance of B. It is the indexing the instances of A and B by location that makes the law a geographical law.

Schaefer also says that the type of laws geographers would most likely draw up are "morphological", that is, "containing no reference to time and change" (Schaefer, 1953, p. 243). They would take the form, if spatial pattern A, then spatial pattern B. There is no temporal process in this formulation. It involves only a spatial relation. Schaefer thought geographers might also refer to "process laws" (i.e., those involving time), but morphological laws highlighted the discipline's special interest and expertise.

Hartshorne was having none of it, however. A week after Hartshorne read Schaefer's paper he wrote to the editor of the *Annals*, Henry M. Kendall, cataloguing Schaefer's "major errors and

mis-statements.”⁷ A week later he was even madder. Publishing Schaefer’s paper, Hartshorne charged, had “create[d] a mess, for me, and for American geography...” The paper was “a palpable fraud, consisting of falsehoods, distortions and obvious omissions.”⁸ Hartshorne never got over it. In 1955 Hartshorne was still writing to the *Annals*’ editor, by then, Walter Kollmorgen: “In whatever sense it is possible for a learned journal to commit a crime ... The *Annals* has committed a crime unparalleled in its history” (quoted in Martin, 1989, p. 76). Even at age 89, two years before he died, Hartshorne continued to fume, in this case writing corrective letters to both Derek Gregory and Fred Lukermann for giving credence in their writings to Schaefer’s paper when it was so obviously demonstrably wrong.⁹

Hartshorne wrote several formal replies to Schaefer. The 1954 reply was a two- page letter outlining his main criticisms and anticipating the full- blown critique to follow the next year in a second paper. The letter was a blistering attack on Schaefer’s scholarship, or more precisely, the absence of scholarship. It pulled no punches. The *Annals* editorial assistant, sister of the Editor, Walter Kollmorgen, was forced to “excise the color words” from the letter before it was deemed acceptable for publication.¹⁰

The 1955 reply was 40 printed pages long, with over 100 footnotes. On many pages the footnotes occupied more of the page space than the main text. In the reply, Hartshorne concerned himself exclusively with defending himself from Schaefer’s critique of his historical scholarship, while at the same meting out corrosive criticisms of Schaefer’s own scholarship. Hartshorne was extraordinarily well prepared to undertake both tasks. He spent that 1938–1939 sabbatical year at the University of Vienna Library reading all there was to read in German on geographical methodology. Further, he often read those German texts with the University of Vienna’s Professor of Geography, Johan Sölch, literally by his side. With Sölch, Hartshorne made exact translations of key texts, or at least as exact as was possible given it was German academic prose (Hartshorne, 1979).

Drawing on his extensive knowledge of German geography accumulated in the preparation of writing *Nature*, Hartshorne made mincemeat of Schaefer’s argument that he had misunderstood German geographical scholarship. In contrast, Hartshorne with relish as the great corrector systematically, line- by- line, word- by- word, demonstrated Schaefer’s own shoddy textual interpretations that were filled with misunderstandings, misquotations, and mistaken citations; in short, it was Schaefer’s scholarship that was egregious, sloppy and slapdash. None of Hartshorne’s textual corrections have been ever challenged, even by some of Schaefer’s most fanatical supporters, and they could be crazily fanatical.

But it did not matter. Schaefer’s shocking historical scholarship was beside the point. American geographers who took up Schaefer’s cause – Bill Bunge, but other “space cadets” who will be discussed in the next section – were not concerned one whit with a set of long-dead German geographers. Indeed, in the immediate post- World War II environment, associating yourself with German thought was a liability and especially in geography that had a lot of dirty laundry (Michel,

⁷ Richard Hartshorne to Henry M. Kendall, 29 October, 1953, **RHP**, Box 192, Responses – Origins of exceptionalism, file G.

⁸ Richard Hartshorne to Henry M. Kendall, 6th November, 1953, **RHP**, Box 192, Responses – Origins of exceptionalism, file G.

⁹ See Richard Hartshorne to Fred Lukerman, July 26, 1989, **RHP**, Box 194, Correspondence – Fred Lukerman, File S; and Richard Hartshorne to Derek Gregory, October 19, 1989, **RHP**, Box 195, Correspondence/ D-E, File D.

¹⁰ Richard Hartshorne to Harold McCarty, September 4, 1969, **RHP**, Box 6, folder 7, personal views, 1952-1969.

2016). Moreover, the imperative to professionalization was moving the discipline away from regional geography, viewed as amateurish (Ackerman, 1945).

As Bunge wrote to Hartshorne in 1959: “I do not care about the historical scholarship. I consider it irrelevant” (quoted in Martin, 1989, p. 79). The critically important point was that Schaefer’s paper opened up geography to the methods of systematic science, allowing explanation, prediction, and the search for laws as well as corollary practices such as measurement, quantification and theorization. Bunge was saying, in contrast to Hartshorne, that there was nothing about the kind of material that geographers studied that prevented them from drawing on the methods of systematic sciences.

All this criticism was not entirely fair. Hartshorne tried to adapt during the 1950s and clarified and restated himself in other publications, including an abridged and revamped version of the argument developed in *Nature*, *Perspective on the Nature of Geography* (Hartshorne, 1959). He courted and encouraged the mathematically oriented geographers in Schaefer’s old department to deepen their methods in a way that was compatible with Hartshorne’s methodology.¹¹ Moreover, at the end of his 1959 reformulation Hartshorne (1959, p. 182) admitted that “as in any science, [geography] seeks to secure that approach to certainty and universality of knowledge that is made possible by the construction of generic concepts and laws of interrelations among factors”.

Nonetheless, he could not help holding on to his earlier position:

the manifold variety of different and incommensurable factors involved in many features of our object of study, the complex world of the earth surface permits interpretation only of a part of our findings by that desired method (Hartshorne 1959, 182)

But Hartshorne’s 1959 qualification to his argument no longer mattered. The modern era of “sputnik” had begun, filled with young people experimenting with new technology and possibilities (Van Meeteren, 2019). During this period epitomized by divided generations, there was no one more “old guard” imaginable than Richard Hartshorne promoting long- dead German scholars. Young scholars preferred learning FORTRAN than German as a second language. In that world, math skills mattered more than access to a Viennese library. As Davies (1966, p. 127) put it: “acceptance of geography as a science can only be made if it places itself within the current methodological conception of a science, and uses its techniques, instead of harking back to the science of other eras”. Almost at the end of his life, Hartshorne seemed to finally get it. In a 1986 interview he realized that he “hadn’t met his [Schaefer’s] logical thesis” (Hartshorne, 2004b [1986], p. 278). But that was the thesis that mattered, that propelled the aftermath.

The Aftermath

The force of Schaefer’s thesis convinced at least a number of younger American geographers during the 1950s to abandon the methodological injunctions of *Nature* and to do geography differently; to do it as systematic science. All of those geographers at least until the late 1960s were male. In part, this reflected the historically discursive character of geography established as a “manly science” as well as more general structural barriers that obstructed and discriminated against women from entering Mary McCarthy’s “groves of academe” (Monk, 2004). In the United States it was further propelled during the early post- war years by passage of the GI Bill that gave

¹¹ Richard Hartshorne to Harold McCarty, April 30, 1956, **RHP**, Box 5, folder 8, Correspondence Miscellaneous, A-Z, 1930-1992.

large numbers of especially young men who had been enlisted in the military cheap loans to attend university including graduate school (Abbot and Sparrow, 2007, p. 292– 293). American universities, including Departments of Geography, were flooded with young male students, while the limited number of talented women that graduated often left academic research to pursue teaching, government and administrative careers (Monk, 2004).

One of the young male geographers who served as a navigator for USAF bombers in the Pacific Theatre, and central to what came after the publication of Schaefer's paper, was William Garrison. He had completed a PhD in geography on the GI Bill at Northwestern University during the late 1940s drawing on Hartshorne's regionalist method. As he put it in a memoir, his dissertation research had involved just "a lot of walking around, ... classification and description" (Garrison, 2002, p. 103). He was so dissatisfied with the result that he subsequently stole his dissertation from Northwestern's library to prevent anyone ever reading it. His first appointment in 1950 was at the Geography Department at the University of Washington, Seattle. There he began to undertake a different kind of geography, rigorous, systematic, logical, quantitative and scientific. Schaefer's paper profoundly resounded with him. In 1955, he wrote exuberantly about Schaefer's paper to a colleague at the Department, Edward Ullman, another of Schaefer's positive reviewers (Martin, 1989), who had similar intellectual leanings, but who also had a longstanding relationship with Hartshorne from the OSS:

I was and still am excited by Schaefer. Now you may present me with formal proofs (1) that all German geographers are deaf, dumb, and unable to write and (2) that Schaefer was cruel to little children, and I would still be excited by Schaefer. Excited simply because Schaefer seemed to know in some crude way of the world of science of which geography is a part (quoted in Martin 1989, 77).

For Garrison, Schaefer was the brand- new exciting future of the discipline, Hartshorne its moribund past. Something had gone seriously awry in the intellectual core of geography he believed. As evidence, Harvard had closed down its geography department in 1948, its university president, the chemist, James Conant, declaring that geography was "not a university discipline" (quoted in Smith, 1987, p. 159). In his paper, Schaefer used the terms "isolationist," "complacent" (p. 226), "apologetic" (p. 227) and "somewhat lacking" (p. 227) to describe the discipline. He thought it was not practised as a serious subject, confined by Hartshorne to "mere description" (p. 227). But it did not need to be that way. That was what so excited Garrison about Schaefer's paper. It showed that geography could link to contemporary philosophical discussions about science that extended from logical positivism. And even more importantly, it showed that as a scientific discipline of spatial relations, spatial science, geography could join methodologically with other social sciences that had made similar moves to connect themselves with science such as economics, psychology, sociology, and political science (Schorske, 1997). All of this was possible Garrison believed. "There is nothing so powerful as an idea whose time has come, and the time had come for Schaefer by the decade of the '50s", he said.¹²

Schaefer of course could not take up the mantle because of his premature demise. Garrison could, though, and did at the University of Washington. In *annus mirabilis* 1955, Garrison along to a lesser extent with Edward Ullman and the Department's cartographer, John Sherman, began to assemble a group of young male graduate students, the "space cadets", who were to pioneer the new scientific geography that Schaefer anticipated. They included Brian Berry, Ronald Boyce,

¹² William L. Garrison, no title, no date, **RHP**, Box 192, Responses – origins of exceptionalism, File J.

Richard Morrill, and John Nystuen, who joined an earlier student of Garrison's, Duane Marble. Within two years Art Getis, Waldo Tobler, and Bill Bunge, a refugee from the University of Wisconsin, Madison, where Hartshorne had flunked him in his doctoral comprehensive exams, also joined that group¹³

Collectively they committed to geography as science, to Schaefer's vision of the discipline. Richard Morrill (1984, p. 59) remembers: "we were introduced to [Schaefer's article] quite soon – I think in that first year '55- '56 by Garrison." But Morrill also says, "Hartshorne was what we were against". To that larger end of doing scientific geography, they took courses in mathematics and statistics. In 1955, Garrison gave the first ever statistics course (Geog 426: Quantitative Methods in Geography—a "baptism of fire"; Morrill, 1984, p. 60) in an American geography department. They also taught themselves how to operate a computer. This was no mean feat. The first computer ever on the University of Washington campus arrived in 1955, an IBM 650, and was located in the attic of the Chemistry Building. It came with no formal programming language, and no hard disk for memory storage. Using the computer was a bootstrap operation, learning by doing, and usually for the space cadets that happened in the very early hours of the morning when no one else on campus wanted to use it. And finally, and maybe most importantly, they tried to practice an exact, formal scientific theory, with the ultimate goal of achieving Schaefer's aspiration of geographical (morphological) laws.

In 1959 it seemingly all came together in a volume Garrison and his students wrote, a "revolutionary book" according to Morrill (1984, p. 61), one of its co- authors, *Studies of Highway Development and Geographic Change* (Garrison et al., 1959). It was a remarkable publication, crammed with calculations, data matrices, statistical techniques, costs curves and demand schedules, and conventional maps overlaid with numbers, arrows, starburst lines, and balancing equations. The real revolution, however, was the changed conception of the region. It was no longer conceived as a Hartshornian element complex, but as a scientific theoretical object after Schaefer, capable of explanation, prediction and law- like statements.

Conclusion

As the Washington students (and indeed Garrison himself) left Seattle for new jobs, they took with them the new conception of geography as a science that later came to permeate and irrevocably alter American geographical thought. Schaefer won the debate, not by proving Hartshorne was wrong in his historical sources or interpretations, but by profoundly changing disciplinary thought and practices (Davies, 1966). Geographers increasingly came to live in Schaefer's world, not Hartshorne's. The Hartshorne- Schaefer debate, as interpreted by Schaefer's followers, travelled, influencing other national geographical traditions, stimulating similar transformations elsewhere in the world. Eventually, it contributed to re-writing geography's history in Germany itself (Harvey and Wardenga 1998). The victory tour lasted until Schaefer's world met its own limits, when the

¹³ None of the space cadets were women. Women were actively discouraged from doing quantitative work with its connotation of masculine rationality. In 1967, when the geographer Susan Hanson entered graduate school at Northwestern University, one of the holy sites of quantitative geography, she was discouraged by the Chair of the Department, Ed Espenshade. As Hanson (2002) put it, Espenshade could not understand 'why you would be in graduate school if you were female and already had a child. He just couldn't understand it. ... When he spoke, he only asked how the family were, but never asked about scholarly work. Realistically as a woman in grad school at that time, one did not expect anything different! We knew very well that we were entering male turf.'

search for spatial laws came up emptyhanded (Barnes, 2004). One might think that would have vindicated Hartshorne, but it did not. Although there have been recurring calls to renew regional geography (Gregory, 1978; Johnston et al., 1990), there have been few calls to renew Hartshorne. He remained trapped in historiography, viewed as a past figurehead of a dreary regional description. Ironically, Hartshorne is now remembered less as a champion of the region than as an opponent of Schaefer's geographical scientific revolution. His masterpiece is seldom interpreted beyond Schaefer's scathing verdict, and with knowledge of the discipline's German roots squarely beyond the rear-view mirror. Such can be one's fate in methodological debates within geography.

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