A deflationary approach to fundamental principles in GIScience

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1. Background

While the practice of mapping has ancient, even prehistoric, roots, the past fifty years have seen increased emphasis on the sciences of geographic information. A key element is moving from the fixation with the specific constraints of the mapped product (the graphic artifact) to re-centering attention on databases as the resources for constructing all potential maps, and the GIS as the basis for all models of process of change. There is an early indication of this approach enunciated in a short (three-page) article by John Sherman and Waldo Tobler in Professional Geographer (Sherman and Tobler 1957; Chrisman 1997a). This was followed by a work on the promise of digital technology (Tobler 1959). Perhaps one of the clearest articulations in the rethinking of cartography was Board's (1967) "Maps as Models".

The first textbook for teaching GIS adopted the title *Principles of Geographical Information Systems for Land Resources Assessment* (Burrough 1986), thereby adopting the idea of a set of basic principles to apply to the

developing GIS scene. A clear case of this move is evident in Andrew Frank's (1987) treatment of overlay processing, in which he argues for a "conceptual approach" (17) rather than one based on implementations. Frank's final sentence set the agenda quite clearly: "The separation into a few relatively simple concepts can benefit the design of the user interface and would result in systems which are easier to learn and easier to use" (Frank 1987, 29-30). Viewed from twenty-four years later, this tendency persists, despite the evident lack of impact on the feature-laden user interfaces now in use. In more recent years, this viewpoint is so commonplace that it would be fruitless to cite all instances. Goodchild and Sui, in various articles, have offered reviews of the subject built on the metaphor of the Tower of Babel and the urge for a common unifying language (Goodchild 1992; Sui 1999; Sui and Goodchild 2001). This chapter was written in parallel with Frank (2012) as a joint reflection on the issues of fundamental principles.

2. Are there fundamental concepts in GIScience?

This essay derives from a long engagement with the subject, and a long engagement in debating different approaches with colleagues over forty years. It takes some explaining to establish what we have been debating. At first glance, of course there are fundamental principles in GIScience. If there is any science in what we do, we all must understand some of the basic elements to be able to work together. But, such a statement, in its sweeping form, raises the crucial issues. Who are the "we", and how do we so universally share some common understandings?

In 1987, right after Andrew Frank's presentation at AUTO-CARTO 8 cited above, I presented under the title "Fundamental Principles of GIS" (an essay later published under a more descriptive [and less grandiose] title

(Chrisman, 1987)). That year signaled twenty years since the first publications using the GIS term, providing an occasion for reflection just as much as the fifty-year signpost that occasions this event. My 1987 essay started with a concern to address "fundamental" principles, particularly related to the choice of data model--an issue that was still current at the time. By the end of the first column of text, I had turned the issue from the fundamentals of mathematics to the "deep issues of why we collect and process geographic information" (Chrisman 1987, 1367). The 1987 text made reference to a paper from ten years earlier in which I had argued that two geographic data structures (raster and vector) were so dissimilar that they were incommensurate (Chrisman 1978). The paper then tried to dethrone the classic communications model that posits messages passing unfiltered through channels (a theme that returned in Poore and Chrisman 2006).

In many respects, I shared the viewpoint of Andrew Frank (rearticulated in Frank 2012) that we needed to divorce ourselves from specific implementations to discuss fundamental issues on some conceptual plane. My argument in 1987 turned toward the design of systems based on social goals, a direction I have attempted to refine over the decades (Chrisman 2002; Harvey and Chrisman 2004). Where Frank (1987) supported a "normalized" view of databases, the social analysis in my 1987 essay required a more diffuse decentralized engagement between actors with roles of custodians, validated by mandates and other social goals (specifically equity over cost-savings). By 2012, Frank (2012) argues that our GIScience is overly influenced by a "database" static view, and he proposes a "process" viewpoint. This essay will explore an alternative approach to fundamental principles, leading to a different approach toward process as well.

The 1987 engagement was just one episode in the debate, but one that problematized the "universality" of pronouncements about principles. The theme did not die

out. In a co-authored editorial, Peter Burrough and Andrew Frank (1995) posed the rhetorical question: "are current GIS truly generic?" Their complex argument covered much of philosophy and cognition, with detours to cite Monty Python. The complex argument arrived at a multi-dimensional scaling (three dimensions to compress five "aspects" located by some ill-defined subjective 0 to 1 scale measurements) in which to situate the various viewpoints of schools of thought in the GIS arena (Burrough and Frank 1995, 113). The two authors concluded that the distinct paradigms are so different that a single generic solution is hard to imagine; the viewpoint of each co-author must be taken into consideration, as always.

Andrew Frank (2001) introduced a series of "tiers" in what he termed ontology, but that he accepts extends to include issues of a more epistemological flavor. In fact, Frank introduces a different approach to the philosophy of science at each tier, from a radical realism at the base level to a subjective social constructivism at the cognitive level. At each tier in his framework, Frank changes the definition of "fundamental", an approach that is problematic and ends up with not much more than positional statements from the viewpoint of various actors. In this paper, I will try to provide an argument for abandoning this concept of tiers completely.

In a similar vein, I revisited my 1987 article in a paper presented at GISRUK in 2001 (Chrisman 2002). I criticized my earlier work as overly structuralist, in that it placed too much emphasis on historically embedded social and institutional entities, not leaving much room for agency to modify the structures. I had become uncomfortable with the implicit "superorganic" entities that seemed to live on their own "tier" without reference to actual people. This is a theme of increased interest recently. The 2001 paper includes a much richer connection to the field of Science and Technology Studies (STS), a field that had been acknowledged in a minor way through the use of terms like "paradigm" (often without the citation to Kuhn (1970). Beyond the over-used term "paradigm", the paper focused on the issue of technological determinism and how the user is configured to fit the technology, and not necessarily the other way around. The paper ends with an emphasis on the division of knowledge, something that provides an alternative to Frank's tiers (Frank 2001, 2012). Software developers are often trying to work inside an existing definition of who-knows-what, rather than working in a cognitive paradigm from Artificial Intelligence. Consequently, the easy division between Society and GIS becomes more and more difficult to defend or detect.

This review has concentrated on the works of Andrew Frank and myself not because we are the best at articulating the question, but more as a sample to be connected to other work. Frank's tiers can be seen in even more elaborate development in Raper's (2000) progression through layers of philosophy of science. Similarly, the questioning about the truth statements about the science of GIScience relates to the questioning of media (Sui 1999; Sui and Goodchild 2001; Harvey and Chrisman 2004).

In the discourse of the GIS field, the issue of "fundamental principles" has remained something to debate and use as a springboard for other issues of importance. It will not go away. Perhaps the most useful question was posed by Tobler (1976) in terms of the half-life of the concepts in his course on analytical cartography. (Frank 2012) uses this as the introduction to his article.) Tobler expressed the desire that half the concepts would still be valid twenty years later. In my personal experiment, I found that Tobler was modest in his claim; about sixty percent of his course content was valuable twenty-six years later (Chrisman 2001). Of course, the rate of decay in the various isotopes of the course was uneven. Some elements were more directly linked to the primitive equipment of the time; others dealt with already long-standing mathematical findings. The off-the-cuff prediction of a wrist-watch

positioning system was one of the elements validated through the course of technological events.

These empirical findings provide a start to deflate claims about abstract universals in GIScience. Claims for the enduring persistence of fundamental principles are in fact subject to empirical validation; it just takes time. Examination of course curricula may prove the clearest demonstration, since the time available for a course is essentially a constant. While the body of knowledge may expand, the instructor must prune back the growing bush to keep the course coherent. The process of leaving something out, forgetting an element judged important in the past, is a crucial act in the progress of science. Bowker (2005) and others in STS have begun to study these memory practices in a more general way. Frank (2012) refers to Popper on falsification of theories and what is excluded. This focus can be extended to what we forget to explain to our students about the phylogeny of our body of knowledge.

3. The deflationary turn

One problem with fundamental principles is that they can be asserted in a way that denies debate. If they are strictly fundamental, they must also be universal, for all time, and self-evident. This naïve Universalist claim was particularly apparent in an earlier era of "social physics" with statements about the rank-size rule and the "law of least effort" (Zipf 1949), but it persists in many forums at least in a tacit form. Frank (2012) has a kind of nostalgic attraction to timeless science, a foundation that is immune from debate. He places mathematics in this category, though there is plenty of evidence to suggest that the history of mathematics is far from unilinear (Boyer and Merzbach 2011). Nearly 2000 years ago, Sixtus Empiricus posed a paradox for the dominant Stoic school of the era (Barrow 1992, 277) that is similar to the anti-essentialist argument that I propose in this essay. Plato positions mathematical truth on some higher plane, remote from human frailties, and therein lies the inconsistency (how then do we gain access to this higher plane of existence?). A more recent lesson from the history of mathematics is that Hilbert's formalist agenda must confront Gödel's demonstration that complex systems necessarily include statements that cannot be proven inside the closed world of formalism (for an application to the agenda of Artificial Intelligence, see Penrose 1989). The odd part here is that both Gödel and Penrose are ardent Platonists. For me, the Platonism behind the formalist agenda introduces *immaterial entities* immune from observation. I do not see the need to build a structure that depends on so much metaphysics. As an alternative, some would argue that mathematics is not "discovered" in some abstract timeless truth, but "constructed" in a contingent sequence that depends on the personalities involved, their time and place (Barrow 1992, 265ff). However, the history of mathematics is not the issue here.

Frank (2012, 2) argues for a "limited" universalism based on the limitations of what can be established by the games played with symbols without reference to the world. Frank (2012, 3) also invokes Popper on the incremental advances of science, but then moves quickly to assert that mathematical truth is "nearly timeless" along with a reconfirmation of formalism as a means to truth. Limited or not, Frank remains tied to a universalist stance. Universalism is not about determinism, but about how certain assertions apply to all places and times. Frank and many of his colleagues have sought solace in mathematical formalism in the hopes that their work will stand some test of time. In most cases, this means a deliberate separation from what they see as social issues. Frank talks about "grounding" as a means of attaching formalism to the world, with some hope of a one-to-one correspondence that requires a lot of work to maintain. I find this stance hard to explain.

As an alternative, I wish to bring in an approach from the philosophy of science that addresses the inconsistencies that Frank catalogs at the different tiers of ontology, and explains the grounding process more as a two-way feedback. Rather than isolating the "social" component to the higher levels, far away from the inescapable physical fact, I offer a coherent approach that seeks to avoid the division into tiers.

My proposed approach derives from a paper that Sergio Sismondo and I published in *Philosophy of Science* (Sismondo and Chrisman 2001). The paper developed an issue in that field concerning the correspondence between scientific theories and the world, through the observation that much of this literature uses the map as a metaphor to talk about theories. Philosophers of science, much like the folk tale about the blindfolded observation of an elephant, obtain different readings through the map metaphor. Robinson and Petchenik (1976) had observed much the same divergences in the use of the map metaphor. For Sismondo and me, the different interpretations of maps seemed totally consistent. Maps serve different functions in different settings; sometimes they are best seen as realist, other times as instrumental, and in other cases constructed.

By adopting a stance of "deflationary realism", Sismondo argues that the divergent examples all make sense if one keeps the claims of explanation modest. The deflationary approach derives from what Arthur Fine (1986) called a "Natural Ontological Attitude" (NOA). The naturalness comes by abjuring the extra additives imposed by more extreme versions of realism and anti-realism. NOA stands on two supports: anti-essentialist and anti-interpretive, with an injunction to leave all metaphysical baggage behind.

Essentialism is the tendency or orientation to assert the "fundamental" nature of certain elements (like fundamental principles). This tendency exists in all fields, and GIScience is not immune. Frank's observation of the different tiers and his explanation of imperfect knowledge show an engagement with the inconsistencies of essentialism. We cannot stick to one tier or another; our knowledge is never totally reliable. Geographic representations are at times instrumental, at times not. They are often rock-solid "facts", and at other times quite subject to interpretation by different actors. Reduced forms of information may be much more "useful" than the mass of facts behind them.

Frank (2012, 18) uses an illustration of a ten euro note to introduce "social construction". This kind of socially sanctioned value is not surprising in any way. Money, particularly in modern paper or virtual form, is a matter of trust and social institutions, and a long distance from "hard facts". Frank brings in "context" as a form of explanation. Yet, context is a very slippery concept, often used ex-post to tell just-so stories. This is where a deflationary account helps restrain the table thumping. A ten-euro note is worth ten euro exactly because of the supporting infrastructure of banks, international agreements, and social trust. It is hard to extract the value of the euro from all the work behind the scenes to make it appear so inherent and effortless (see Latour 1999 for some other examples). Context is not an entity that can be sensed in the world in any concrete way, rather it is an explanation after the fact of what was important to know. The concept of context, despite its currency in computer science circles, has little explanatory value in this kind of debate. Latour *et al* (2012) recently took the provocative stance that there is no need to postulate "superorganic" entities like anthills, nations, or societies. By adopting data-mining approaches, there is no need to aggregate upwards. This recent work is a part of the anti-essentialist thread adopted by this paper, since it avoids predefining what exists. Latour (2012) also argues that "the whole is less than the sum of the parts", in opposition to Frank's (2012, 8) assertion that complexity emerges from simple atomic objects. I do not have the space here to review or evaluate this confrontation, only the space to suggest that a debate still persists.

Ten-euro notes (and the various interpretations of monads and complexity theory) are not really the subject matter of our GIScience discourse. Let us take an example of much more direct pertinence: the bedrock of our GIS enterprise, our geographic coordinates. These measurements appear to be firmly mathematical in their representation, yet they derive from an architecture of assumptions and treatments that are historically bound and thus contingent rather than fundamental. We can assert, for example, that a geodetic datum is "socially constructed" to acknowledge the careful work of standards committees that sat for years to bring World Geodetic Standards 1984 (WGS) into being. Acknowledging this necessary work does not mean to bring it into disrepute as arguable. The deflationary stance tries to avoid jumping to value statements. As Hacking (1999) observed, the term "socially constructed" is often used in a sense of ridicule or criticism, as we can observe in Harley's (1989) critique of maps as political messages. Here, the deflationary stance allows us to simply record and acknowledge the combination of mathematics and organizational logics that uphold the WGS. They are neither pure mathematics nor corrupt human institutions, but rather a mix of human agency and mathematical models through and through.

Both Andrew Frank and I have supervised PhD students whose work explored the epistemological underpinnings of simple assertions like that coordinates for define property boundaries. Frank (2012) cites his student Buyong (1992), while I would cite my student Karnes (1995) in setting out a pluralistic means to adjudicate the best use of the historical record of measurements at any given time. These PhD dissertations are examples that argue against an "essential" truth in one source over another.

In terms of interpretation, Fine's NOA begins with a basic level of agreement that the best-established claims are sufficiently justified. It is "natural" (or unproblematic) to accept them as "true". NOA resists any attempt to add other accounts: correspondence, pragmatic, instrumentalist, or conventionalist. Thus, Fine has a simple approach to "truth"--statements that we have good evidence to support. Some "table-thumping" realist may insist that electrons are really real, and Fine can calmly agree that we have sufficient evidence; no need to get worked up about it. Sismondo argues that Fine runs into trouble around here because his anti-essentialism conflicts with his anti-interpretivism. No matter the fine points, we can still retain the approach of not granting extravagant value to statements that appear to have sufficient evidence to support them.

Sismondo and Chrisman (2001) bring in various examples of maps to demonstrate where Fine's NOA runs into trouble. Understanding abstract symbol systems such as maps and models requires some interpretation of a metaphysical sort. One example is to consider the use of the Mercator projection for nautical charts. This projection is a perfectly reasonable choice to make certain operations easy (compass bearings lead to straight lines on the chart), at a certain cost (distances vary by latitude). Yet, when some diplomats meet to determine nautical boundaries between countries, they will not find much other than the nautical chart for their deliberations. The geometric midpoint between two coasts may not appear to be a fair solution when drawn on the chart. Many maritime boundaries are thereby located with much less equity than the actors thought they would exhibit (Lathrop 1997). Therefore, the subsequent map user needs to understand the interpretations made in the original user scenario, and not import any preconceived notions about geometry. The affordances of the Mercator projection have to be used inside a well-defined practice, otherwise it is easy to mislead on properties of distance or area. Some measure of interpretation is required.

This philosophical stance implies a very restrained, hence deflationary, level of metaphysics, but the kind that the

expert analyst must learn to make sense of abstractions. In day-to-day work, many practitioners in GIScience adopt a basic level of deflation without using the label.

4. Applying deflationary analysis to Tobler's Law

To demonstrate the utility of the deflationary stance, I will proceed to consider what has been termed "Tobler's First Law of Geography" (Tobler 2004; Miller 2004; Sui 2004). This "Law" states: "Everything is related to everything else, but near things are more related than distant things". (This statement was announced by Tobler (1970) as the "first law of geography", but attributed under his own name only more recently.) As laws go, this one is loosely formulated and without concrete predictions. As Tobler (2004, 309) points out, a rather similar formulation was made by the statistician Fisher in 1935. Hecht and Moxley (2009) have presented what they term evidence to support the assertion, based on an analysis of Wikipedia entries. The deflationary stance finds this all unremarkable. Tobler's Law (or indeed Fisher's Law) might well have substantial evidence to support them, so why not treat them as "true"?

A formalist might feel that a "real" law has to have clearly defined terms, not just "things" and "relationships". And, of course, there are those who insist on laws with mathematical predictions that can be falsified or at least potentially disproved. Tobler's Law may be a last weak restatement of the social physics movement (such as Zipf 1949) that tried to specify the power function for distance decay. From certain studies, we can ascertain if the distance between social interactions actually declines with the inverse square of distance or not. Certain geographers (such as Barnes 1991) have criticized this movement with quite complex arguments that I will not reproduce in this essay. Barnes in his turn makes a case for a different interpretation of the role of science, one based on a different epistemology. The conflict on the geography front of the Science Wars is perhaps as irreducibly futile as elsewhere. With a deflationary stance, the amount of influence of the time and place in which some work was done is just another interpretation to take into account.

This essay will examine a few of the commentaries offered on the subject of Tobler's First Law (TFL) in a forum edited by Sui (2004) in the *Annals of the AAG*. The deflationary stance offers a means to balance the irreconcilable viewpoints and provides a means to address fundamental principles. Most importantly, Barnes (2004) attempts to offer an anti-law proposition based on what he terms "anti-philosophy of science", namely science studies. I do not see any such opposition. The deflationary stance is developed by philosophers of science with substantial grounding in the discourse. Beyond Fine (1986), the deflationary stance relates to Giere's (1999) "Science without Laws" as well as various works of Hacking (1999), Feyerabend (1975) and others. It is quite correct that this viewpoint is not the approach from the mid-twentieth century (such as Carnap 1966). However, various arguments have deep roots, and it is hard to characterize another discipline in a short space. Barnes seeks to reject outright the use of the term law. Tobler (2004, 304-305) responds by citing the fallibility of philosophers, and arguing for greater faith in empirical results, adopting Richard Feynman's more lax views on what may be better termed models than laws. To each of these arguments, the deflationary stance provides a place of balance in a surprisingly taut debate.

Goodchild's (2004) contribution as discussant in the above debate takes a version of the deflationary approach. He argues that the use of the term "law" or principle or theory is not of great importance. What matters more is the utility of the statement in teaching and application. But, deep down, Goodchild retains his training in physics and looks for the "universal, true, and eminently useful" (2004, 303)

in the archetype Boyle's Law. Like Barnes, I have read Shapin and Shafer's (1985) historical account of Boyle's experiments. It provides a clear study of the relation of the scientist to his or her time and place. Yet, there is room for a measure of realism here. However Boyle (or Newton or Tobler) was influenced by his social and historical setting, something portable remains. This "immutable mobile", as Latour (1993) terms it, is invariant as it is translated from one use to another. If Boyle held hierarchical and aristocratic views of society, it certainly had an influence on his methods and how he construed "proof". His Law, however, can be proved by other means to suit whatever taste. It makes sense to accept that gasses expand to fill a space, following a specific mathematical formulation. This part is indeed portable. The deflationary stance withholds the judgment about universality, which is more a result of the test of time than something we can determine here and now.

5. Dealing with process

Frank (2012) argues for a change in paradigm from the view conditioned by a static database towards a more dynamic view of process through time. On the surface, this sounds a bit inconsistent with the view that principles are timeless. Yet, of course, the urge to deal with the dynamics of the world system is of long standing. Frank's treatment of functions, functors, and other constructions serves to demonstrate how elements of a formal logic can be combined to model complex processes. I share Frank's determination to orient the practitioners of GIScience to pay more attention to history, time, and process (Chrisman 1997b; Poore and Chrisman 2006). We do not agree (yet) on the means to this end.

Frank proposes a paradigm of a "spatial time machine" as the start of a list of paradigm shifts that he foresees. The tiered ontology divisions remain a central part of this conception, so our views remain divided. My concerns are not specifically about the role of social science or public administration (as portrayed by Frank on page 20), but on a more philosophical stance of anti-essentialism. I understand the urge to create simplified "objects" that respond to limited, tightly defined rules. I wrote programs myself, and I know how one creates little logical worlds isolated from everything else. I also learned that that formalism doesn't buy me anything in the world outside. If our models fail to represent the world, they are simply logic games of limited value.

Invoking H.G. Wells's *The Time Machine* does not align our science more closely with fundamentals. The thought experiment of dropping a current scientist into another time is perhaps fruitful in demonstrating how hard it might be to make a convincing demonstration to the residents of a different era. Our massive stores of "big data" would be hard to explain by hand-waving to the scribes of an Egyptian pharaoh. Piles of papyrus do not afford much text mining. Similarly, a future scholar may dimly remember the era when we were impressed by terabytes and 140-character messages.

The process of greater interest is the process of forgetting, of how a discipline prunes back all of the acquired details to see certain clear fundamentals that persist over decades and centuries. Our geometrical principles were developed in that Egyptian setting, and most of our property surveying would be explainable to a time machine visitor from that era. Of course, the GPS equipment would appear to be magic, though it embeds the same-old triangles. In these cases, we can be fairly sure that we are close to fundamental principles. The logic is historical and empirical, not requiring any metaphysics.

In his conclusion, Frank (2012) expects that his tiers of physical objects and social-construction can become connected, perhaps through some emergent properties. My naturalist impulse asserts that they were never

disconnected, and that any order imposed is arbitrary and unproven. Latour (1999, 141) extends the work of Whitehead to explain how the relations between humans and non-humans (Frank's objects) is much more symmetrical and undecided. The more recent work by Latour *et al* (2012) on social networks and data mining are particularly pertinent in showing a way around the ontological tiers of separation. Latour contends that "the whole is less than the sum of the parts", in his usual counter-intuitive manner. Basically, he adopts an anti-essentialist (anti-Platonist) argument that social complexity does not belong in the superorganic, but in each person. These arguments are much more convincing to me. It will take time to tell if Frank's views and mine are so dramatically different, or if they have converged in all but their surface formulation.

6. Starting a list of fundamental principles

A chapter such as this one should perhaps not end without providing some guidance on fundamental principles drawing on the deflationary stance, anti-essentialism, and anti-interpretativism. To some extent, adhering to this perspective, it is premature, even illusory, to expect to have a single short list of fundamentals for all time. Like items in the curriculum of a course, the research agenda will change as the pressures on the field change and the world around us shifts. The deflationary approach can orient the development of principles in a manner that avoids the pitfalls of universalism and essential truths. More importantly, this approach can emphasize the practical utility of these principles while bringing a measure of modesty to the debates about principles.

For right now, in closing, I would propose a few samples of applying deflationary approaches coupled with anti-essentialism to the development of principles, rather than attempting to be exhaustive or canonical. The first would insist that value (in an information system) derives strictly from use. This denies the often-inflated claims of some inherent value that resides in the database.

Second, I would suggest leaving the Cartesian world behind and embracing the spherical (or better yet ellipsoidal) world as it is. There are too many false conclusions drawn and stupid measurements made when geographic software, built for projected Cartesian coordinates in a local setting, is applied at the global scale. For example, there are global databases developed with raster technology applied to degrees (so called "square degrees"). There is nothing square about them; they are not homogeneous in size or in neighborhood relationships. However, significant communities of scholars (in highly funded circumstances) have convinced themselves that this data representation makes sense. Equally, we find silly results shown for "distances" calculated to millimetric precision using entirely inappropriate algorithms on the Plat Carée projection (guilty parties not cited out of tact). Each of these flaws comes not from some tiered ontology, but from oversights and laziness on the part of system designers. The fundamental principle is that we need to respect the shape of the Earth and not imagine that our models are so important that they overrule reality.

Overall, these "fundamentals" are perhaps too reactive to errors in others. Unlike Tolber's Laws, they attempt to avoid folly rather than to distill wisdom. So be it. It may take some time to develop a set of positive fundamentals. I will leave that exercise for further research.

7. Conclusion

A generally deflationary stance towards claims of grand universality is a useful step in toning down the rhetoric in the debates over fundamental principles. With this viewpoint, we can agree that there might be some fundamental concepts that work in many settings. These

useful models, such as Tobler's First Law, should form a part of instruction. However, any claim that they are timelessly universal does not seem to be of great value or importance. As a part of this stance, the anti-essentialist element is particularly crucial to deal with the current tendencies on the formalist side of GIScience. The anti-interpretative element will take additional effort to elaborate in a way that supports the pragmatic goals of GIScience in the world. The tentative fundamental principles advanced in this chapter will need some reflection and evaluation before they can be used as a guide for coursework and orienting the research enterprise.

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