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Six GIScience Ideas That Must Die

Krzysztof Janowicz[1,2], Rui Zhu [2], Judith Verstegen [3], Grant McKenzie [4], Bruno Martins [5], and Ling Cai [2]

[1]Department of Geography and Regional Research, University of Vienna, Austria
[2]Center for Spatial Studies, UC Santa Barbara, Santa Barbara, CA, USA
[3]Laboratory of Geo-information Science and Remote Sensing, Wageningen University, Gelderland, Netherlands
[4]Department of Geography, McGill University, Montreal, Quebec, Canada
[5]Instituto Superior Técnico and INESC-ID, Universidade de Lisboa, Lisboa, Portugal

Correspondence: Krzysztof Janowicz (janowicz@ucsb.edu)

Abstract. In 2015, John Brockman edited a volume of chapters contributed by leading thinkers from various domains discussing common scientific ideas hindering further scientific progress. While starting with the provocative slogan of *This Idea Must Die*, the book's chapters and their authors (for most parts) do not argue that those existing – often foundational scientific theories from various domains – are false, but instead that their widespread, and often unquestioned, utilization has started to hinder the evolution of new theories. Through this work, we would like to foster a similar discussion in our community, by suggesting six ideas in GIScience/geoinformatics that may benefit from retiring to make room for new perspectives. Our suggestions are somewhat controversial, and readers are encouraged to keep an open mind.

Keywords. Modeling Space and Place, Volunteered Geographic Information, Data Science and Machine Learning, GIScience Application Domains

1 Introduction

According to the Planck principle, *science progresses one funeral at a time*.[1] While this is often interpreted as a statement about people, it may as well be a metonymy for scientific ideas and theories put forward and defended by those people. So what makes such ideas overstay their welcome? For instance, statistical *significance testing* is a commonly used example of an extensively used and beneficial method, that is considered less favorable now due to several adverse effects of its practical application in science (Carver, 1978, 1993; Johnson, 1999; Brockman, 2015). The key arguments made are not that significance testing is wrong, but that effect size is often not considered, that experiments are tailored to yield low p-values, and that overuse of one method starts to suppress alternatives and even holds back scientific results that do not fit one specific style of research design. Similarly, the notion of stationarity (in time and space), while a very useful simplification for modeling, is unlikely to be the norm but rather an exception as far as processes on the Earth's surface, and even more of human behavior, are actually concerned (Brockman, 2015; Verstegen et al., 2016; Zhu et al., 2019).

To generalize from these examples, we aregue that scientific ideas and theories may become a burden for future progress for some of the following reasons:

- A method (or scientific paradigm) becomes so dominant that it leaves no room for alternative methods, or impacts what type of work is published in the first place. The aforementioned statistical significance testing is such an example (Brockman, 2015).
- A method (or dataset) becomes widely used to a degree where research is tailored specifically towards yielding the best evaluation results under this method or dataset, essentially leading to over-fitting. Freebase (FB15k) and ImageNet may be such dataset examples for knowledge graph (Bordes et al., 2013) and computer vision research (Deng et al., 2009), respectively. Similarly, for several years, Recurrent Neural Networks used to be a must-have for dealing with sequential data (such as time series), and studies often did not consider alternative methods that are perhaps simpler and less prone to over-fitting.
- A key notion becomes redefined and broadened over time to a degree where its usage does not clarify but leads to confusion. *Feature* and its widely different usage within and across domains is such an example within GIScience and computer science research.

[1]Based on Planck (1950).

Concept is another example, particularly in the domain of Cognitive Science.

- Software and workflows supporting a certain technology/method may be so broadly available and adapted that new (and perhaps superior) methods do not find wide application. For instance, analytical methods provided by ArcGIS toolboxes, such as the Spatial Statistics toolbox and the Geoprocessing toolbox, mainly implement traditional pairwise and distance-based methods (e.g., Moran's I), leading to an under-representation of higher-order spatial interactions and directional effects (Zhu et al., 2017, 2019). Similar arguments can be made about clustering techniques beyond DBSCAN.[2]

- Long-held assumptions about what defines a research field may hinder interdisciplinarity or a broadening of scope. For instance, we will question whether modern GIScience is still a bridge-building discipline required to connect domain scientists with computational methods.

In this work, we would like to initiate a discussion about these issues. More specifically, we suggest six ideas in GIScience/geoinformatics that may benefit from retiring to make room for new perspectives.

Concretely, in Section 2, we first question the fundamental dominance of geometry-first representation and analysis of geographic information. Next, we discuss three popular research topics and concepts that have dominated GIScience research for years: Place (Section 3), Volunteered Geographic Information (Section 4), and purely data-driven GeoAI (Section 5). In Sections 6 and 7, we question the assumption that GIScience is a bridge-building discipline and, therefore, interdisciplinary by definition. Finally, we discuss the risk of constantly creating geo-specific sub-fields of emerging trends.

Ultimately, the goal of our work is not to argue that an idea must truly *die*, but to spark a discussion among our community that allows us to revisit some key assumptions that we have left unquestioned for years and even decades.

2 Geometry First

Together with time and theme, location has been regarded as an imperative dimension composing the atomic representation of geographic information (Goodchild et al., 2007). In fact, it is the concept of location that makes geographic representation, as well as its following analysis, *special* when compared to other non-spatial representations (Anselin, 1989).

Location is intrinsically a *relation* rather than a *property* (Kuhn, 2012), so a reference system must be used to locate an entity (Bittner, 1997). Humans use multiple ways to describe and communicate locations, including referring to addresses, directions, topology (in terms of relations to other locations), place names, natural language descriptions, or absolute coordinates, among other possibilities. All these approaches have either an intrinsic or extrinsic frame of reference (Clementini, 2013).

Despite such a diverse range of representations, the presence of a concrete geometry (be it point, line, or polygon-based) together with a formal reference system is a necessary first step for data loading, entry, and analysis in GIS. For instance, modern GIS still lack the ability to load geographic data based on topological or directional information alone, e.g., the fact that two geographic features are adjacent. Moreover, geometry-first spatial analysis and modeling has received almost all attention aside from some initial research on place-based GIS (Gao et al., 2013). For example, classics such as Kriging (Cressie, 1990), Getis–Ord Gi (Getis and Ord, 2010), and Geary's C (Geary, 1954), are all derived from geometric relations (distance and/or direction) between geographic entities. Such an observation can be attributed to the historical root of GIS and GIScience in thematic cartography (McHarg et al., 1969; Clarke, 1999), whose focus was on projection so as to visualize geographic entities and phenomena in a plane. Put differently, GIS starts with a geometric description of the world and then typically asks questions about:

- the location of features;
- the patterns multiple features form, for instance (based on) their distribution;
- How (non-spatial) attributes vary by location;
- and, how (non-spatial) attributes vary by inter-feature distance.

There are, of course, many reasons to do so, e.g., when studying the absolute distribution of crime spots in urban spaces or estimating the presence of ore deposits.

While *qualitative* and *approximate* means of representing and measuring location (Frank, 1992; Freksa, 1991; Egenhofer and Mark, 1995) have been well studied and formalized in fields related to (spatial) cognition, Artificial Intelligence (AI), decision-making, economics, and so on, they often play a secondary role in typical GIS workflows, e.g., when defining topological constraints on geodatabases. Put differently, a GIS workflow can consist of loading a vector layer of concrete urban neighborhoods (i.e., their geometries) and then asking whether adjacent neighborhoods show similar crime patterns. However, asking if offenders typically commit crimes within their own neighborhood or across neighborhoods, without specifying where on the Earth's surface these neighborhoods are located, is not possible. While this may seem a small issue, it substantially hinders the utilization of GIS for simulations and forecasts that do not rely on absolute geometry.

[2] Although one could argue here that the delta is not sufficient to justify the implementation of other algorithms.

The ongoing COVID-19 pandemic is a prime example for the aforementioned limitation. Questions such as the efficiency of masking, stay-at-home orders, effects of population density, place-type based restrictions (e.g., wrt. dining), and so on, cannot be modeled in a classical GIS just based on non-spatial attributes and topology, but always requiring an absolute reference frame in addition. Simply put, we currently cannot ask about the spread of COVID as it relates to the everyday trajectories and visited place types, without the absolute location (Shaw and Sui, 2020) of these trajectories and the places people visited, despite them being irrelevant for the task at hand.

In addition, with an ever-growing availability of social sensing (Liu et al., 2015; Janowicz et al., 2019), heterogeneous data across multiple media can now be easily collected, which record almost all aspects of human interaction with the surrounding geographic space. Not surprisingly, a large portion of such socially-sensed data is in fact qualitative (e.g., textual descriptions of a city configuration). The lack of effective tools to process such qualitative geographic information, consequently, poses a challenge for us to tackle as a community.

Yet another example comes from the digital geohumanities (Grossner et al., 2017) where researchers are interested in studying, visualizing, and documenting historic itineraries or trade routes, in most cases without being able to provide geometries for them.

Summing up, while geometry certainly matters greatly for GIS and GIScience, *geometry-first* has restricted the usage of many of our methods and tools, and has also slowed down progress in research on place-based GIS (Gao et al., 2013; Papadakis and Blaschke, 2017). [3]

3 Humanistic Concepts of Place are Sacrosanct

Place is among the key notions that define the field of (human) geography. Historically, it has been explored within the purview of humanistic and cognitive geographers, as well as critical theorists. Seminal work by authors such as Yi-Fu Tuan (1977; 1975) and Tim Cresswell (2014) are often the first to be referenced in any discussion about place (e.g., Blaschke et al. (2018), Zhang et al. (2020), and Brown et al. (2020)). The GIScience community studies the representation of place and, hence, it looks upon this body of work with a combination of reverence and apprehension. Humanistic geographers (Tuan, 2017) tend to write on the topic of place in a language that is descriptive, yet most often not formal in a way that would facilitate the computational representation of the concept of place.

As a discipline of quantitative scientists, we appear to have concluded, either through interpretation of these writings or by being explicitly told as much, that place is an exceptionally complex concept that cannot be formally represented in a computer with sufficient accuracy and/or detail. In the past, GI scientists have viewed this as a challenge, which has led to a plethora of publications aimed at computationally modeling place in one way or another. Geographers and information scientists have been approaching the topic of place from a quantitative perspective for at least the past decade (Tang and Painho, 2021; Goodchild and Li, 2011; Scheider and Janowicz, 2014), and arguably longer (Harrison and Dourish, 1996; Winter et al., 2009). Paradoxically, at the same time, we have continued to hold onto the notion that *place* is unquantifiable (Golledge and Stimson, 1997), and that holistically modeling place computationally is an insurmountable task.

Here, we argue that this impossibility does not truly hold, and that a large body of work now exists in which researchers have successfully modeled place in order to accomplish a range of tasks, e.g., human movement simulation, place recommendation, or similarity assessment (see Purves et al. (2019) for an overview).

The reality is that *place* is an abstract concept that means different things to different people. However, this does not mean that one cannot select their definition of place, state their assumptions, and go about developing and testing empirical, theoretical, and/or simulated models of place. In order to push forward and truly embed place-based research within spatial data science, we must move past the purely humanistic definitions of place, and take ownership of the concept within our field.

This is even more important for modern, interdisciplinary research. If we, as a community, refuse to make formal place representations (Casati et al., 1999) available to all, and tell them apart from purely location-based approaches, others will either not be able to utilize our work or borrow from our neighboring disciplines that readily provide definitions and models of place (even though we often consider those to be reductionist). Put differently, no science can rest on the assumption that its key notions are too complex to be understood by other domains and too multifaceted to be captured computationally.

4 All User-Generated Content is VGI

In late 2007, Goodchild (2007) published a paper in which he coined the term *Volunteered Geographic Information (VGI)*. While the concept of VGI had existed in GIScience research prior to this work, applying the label of VGI increased interest in the topic, and led to the organization of a community of researchers interested in exploring and using data contributed by citizen sensors (Silvertown, 2009).

In his work, Goodchild explicitly stated that VGI is "...a special case of the more general Web phenomenon of user-generated content..." The growth of research in this area, combined with the dramatic increase in social media plat-

[3] Interestingly, one of the anonymous reviewers pointed out that the argument could also go the other way around and it is the place-based researchers that should develop suitable tools for their needs, instead of blaming GIS for their design.

forms, mobile devices, the internet of things, and context-aware technologies, has led to VGI now being used to describe a large subset of user-generated content, including (involuntary) data traces.

We argue here that much of the content being analyzed today within GIScience research is erroneously being labeled as VGI (Horita et al., 2013; Bakillah et al., 2014). In order to understand how and why this has changed, we need to pull apart the term *volunteered geographic information*, as well as the context in which the term is employed. Take, for example, the now common place activity of "checking in" to a place through a social media application, or reviewing a restaurant on a platform like Yelp[4] or Foursquare[5]. These are arguably not voluntarily contributed for the sake of analyzing geographic information, but instead simply user-generated content that includes geographic attributes. The distinction is important. We make the argument that 1) volunteered geographic information is content that is actively *volunteered* by a user, and 2) the primary purpose of sharing VGI data is to share geographic content. In fact, most geotagged contributions to social media applications today are not VGI, in that the geographic information is secondary to the primary purpose of sharing the data, namely posting a photo or a video, personal fitness tracking, etc. (McKenzie and Janowicz, 2014). Second, many online platforms today, while seemingly voluntary, have been socially engineered to remove choices and promote particular behaviour (Hou et al., 2019). To a teenager, the pressure to engage through online social platforms is conceivably not voluntary. Businesses, government agencies, and even schools are increasingly moving to for-profit online platforms, requiring citizens to not-so-voluntarily contribute data to these systems. Similarly, past GIScience research has utilized social media postings, e.g., from Twitter or Flickr, together with their explicit or inferred location information for inferences, e.g., about health, walkability, crime, sentiment, and so on. Clearly, such messages are not VGI, and the users of these platforms are not aware of the fact that their contents end up in datasets and papers, even if in aggregated form. The same argument can be made for data traces more broadly (e.g., tracking people using Wifi and IP-based techniques).

To be clear, VGI remains an important form of data, with a thriving community of researchers that are using these data and exploring the motivation for such voluntary contributions (Yan et al., 2020; Ballatore and Zipf, 2015; Ballatore et al., 2013). It is important, however, that as shepherds of the term VGI, we do not fall victim to painting all manner of user-generated content with this brush. This does us, and the broader scientific community, a disservice. Our suggestion is that in working with user-generated content, we make sure to be explicit as to when our data meets the criteria of 1) being volunteered, and 2) with geographic content being the primary attribute. This is particularly important in a time of increasing awareness for privacy and misinformation.

5 Purely Data Driven GeoAI

While modern data-driven methods and Artificial Intelligence (AI) have opened up countless interesting opportunities within GIScience, they have also led to the emergence of a mindset that can perhaps be described as *AI solutionism* (Morozov, 2013). This corresponds to the idea that, given enough data, machine learning algorithms can directly solve almost all problems. We argue that there are many pitfalls associated to this idea. Instead of supporting progress within GIScience, it actually diminishes the important scientific contributions that have been advanced by our community, and contributes to setting unrealistic expectations about what can be accomplished and about the future of *insight*, more broadly.

While the intersection of AI and GIScience has a long history (Smith, 1984; Couclelis, 1986; Openshaw and Openshaw, 1997), recent developments associated to deep learning have led to a wave of enthusiasm and increased interest, initially in tasks related to remote sensing and earth observation, but quickly also embracing other problems. Communities around the topic of Geospatial Artificial Intelligence (GeoAI) were quickly established (Janowicz et al., 2020), and researchers have, in fact, put forward interesting contributions relating to why spatial is special in AI, or relating to problems that we can now address better through the use of AI rather than more traditional approaches (Mac Aodha et al., 2019; Yan et al., 2019; Wang and Li, 2021; Mai et al., 2020).

Nonetheless, it is important to keep in mind the limitations within state-of-the-art data-driven methods, many of which will only be resolved through new discoveries. The unreasonable effectiveness of deep learning (Sejnowski, 2020), and other important concerns (such as interpretability and explainability (Xing and Sieber, 2021)) regarding these methods, are shared with varying degrees of intensity by most leaders in the field of AI (Samek et al., 2019; Arrieta et al., 2020).

According to deep learning skeptics such as Gary Marcus (2018), these methods are greedy, brittle, opaque, and shallow. In brief, deep learning is greedy because it frequently demands huge sets of training data for supervised training, at the same time performing sub-optimally at the *long tail*. They are brittle because when a neural network is given a *transfer test* (i.e., when the network is confronted with scenarios that differ from the examples used in training, such as remote sensing imagery from different areas or collected with different environmental conditions), they cannot contextualize the situation and frequently yield results with low accuracy. They are opaque because, unlike traditional approaches following formal rules and computations defined explicitly, the parameters of neural networks can only be interpreted in terms

[4]https://www.yelp.com/dataset
[5]https://developer.foursquare.com/

of their weights. Consequently, these methods are black boxes, whose outputs cannot be properly explained, raising doubts about their reliability and biases. Finally, they are shallow because they are programmed with little innate domain knowledge. Neural networks are limited to encoding correlations in the data, instead of causation or ontological relationships (which have been studied in GIScience extensively (Frank, 1997; Kuhn, 2005)), and they are not well suited for high-level reasoning or planning with data (e.g., causal reasoning).

Syncretism can contribute to addressing these limitations. In particular, GIScience contributions related to modeling spatial dependence or issues involving scales, can bring forth benefits if injected as additional information within machine learning methods. Practitioners have shown that such domain theory-informed methods (e.g., taking into account domain theory as constraints, e.g. in the form of model regularization strategies, at training) can accelerate model training or improve accuracy, especially when supervised data is scarce. However, a widely ignored fact is that space is not only dependent but also heterogeneous. Most existing ML models (referred to as *global models*) only consider spatial dependence across space, but neglect local variations. Consequently, such models cannot perform well locally, or be transferred to study similar problems in other regions.

Rather than directly applying deep learning methods advanced in areas like computer vision, with minor adjustments, GIScience should carefully consider how to best combine its key principles together with these methods in terms of various geographical problems. For instance, with regard to the aforementioned global models, we advocate developing *local models*, so as to take into consideration local variations and increase the generability of ML models when applied to geographical problems. Moreover, instead of respecting model size and complexity just for their sake, the field should embrace the idea that not all spatial analysis will require heavy-duty deep learning models. In fact, using more lightweight models (e.g., ensembles of decision trees, especially if combined with appropriate methods for introducing spatial features) can often reduce some of the aforementioned bottlenecks.

6 Building Bridges

GIScience is an application-independent discipline which develops methods and models for handling (geo)spatial and temporal data. These models and methods are then employed in application domains to find solutions to domain-specific problems. In such exercises, the GI scientist has traditionally fulfilled a bridge function, bringing the methods to the application domain. While the GI scientists are naturally familiar with one end of the bridge, e.g., the methods, it is nearly impossible for them to be completely familiar with the other end, i.e., every possible application domain. Yet, detailed domain knowledge is essential to a variety of activities:

1. Sensibly apply methods, e.g. to select parameters like the movement speed of particular agents within spatial simulation models. Put differently, to connect these methods to the (geographic) context of the application (Dodge, 2016).

2. Interpret and explain results or solutions, e.g., to understand the reason that a certain factor received high importance in a statistical model (Gahegan, 2020).

3. Develop models that challenge existing theories, which may be considered as the most useful aspect of modelling (Oreskes et al., 1994), or even to discover new theories.

These examples illustrate that GI scientists need expertise from application domains. Still, how much expertise do we need, and what degree of familiarity with computational methods can we expect from the subject matter experts we are supposed to support with our research?

In practice, the idea that GIScience is a bridge-building discipline may have overstayed its welcome. On the one hand, GI scientists publish domain research, e.g., about biodiversity, health, the effectiveness of state mandated COVID measures, and so on, without explicit external domain experts on their teams. On the other hand, these experts are utilizing our advances, and other computational methods, and are increasingly developing such methods themselves given the wide range of existing software libraries to support them.

This puts us back into a situation that we tried to overcome two decades ago with the creation of GIScience on top of GIS, namely being no longer perceived as mere toolbuilders. One suggestion would be to focus more of our work on studying the *geographic* information universe as such (Adams et al., 2014; Janowicz et al., 2014; Miller, 2017). Another solution may be to become even more domain-independent, and broaden our work towards *spatial* data science. For instance, currently, a rapidly growing body of work on spatially-explicit machine learning models has been widely used in various applications and across many domains, ranging from health (Kamel Boulos et al., 2019) to ecology (Meyer and Pebesma, 2022). Finally, a third approach may be to integrate ourselves closer with domain experts and to build expertise in concrete domain topics, thereby abandoning the bridge position (and particularly avoiding 'one-sided' bridges).

Put more provocatively, GIScience should develop domain-independent methods, but not engage in domain-agnostic applications.

7 Spatial is so Special that it is Best Served with Highly Specialized Research Communities

Even within highly specialized research areas one is likely to find numerous sub-specialties. In different fields of study, we have seen an increasing trend towards considering that spatial data calls for special techniques, for instance reflected on specialized workshops (e.g., GeoKG & GeoAI 2021 in GIScience[6], or GeoAI in SIGSPATIAL[7]), conferences, journals, and special interest groups within scientific societies and professional organizations. Within GIScience, increased attention is also being given to the specificities of different application areas. Topics like geospatial humanities, geospatial artificial intelligence, geographic information retrieval, geographic knowledge graphs, or geospatial big data, are but a few recent examples of these trends.

While understandable and not without merit (e.g., the importance of appropriately modeling spatial dependence, spatial heterogeneity, or scale, is being highlighted within different types of problems), we argue that an excessive geo-labeling (referred to as *GeoX*) can contribute to creating micro-communities, which may be difficult to develop and sustain and may lead to undesired insularity problems. The aforementioned trends can have profound impacts on the ability to broadly generalize research findings, and on the degree to which research can be thought to inform the practice and theoretical development of other individuals outside the hyper-specialized areas.

While space is indeed special, GIScience should strive to have broadly shared research agendas, commonly applied methodologies, stable and agreed upon dissemination channels, and shared paradigms. The idea of avoiding the proliferation of micro-communities is also not incompatible with further involving domain experts in GIScience research. Ideally, domain experts should be involved as collaborators within the mainstream research taking place in GIScience. Their contributions should be visible at the main GIScience fora, rather than being delegated to hyper-specialized workshops.

Put differently, while creating our GeoX sub-fields can be important for community building, training, raising awareness, and securing research funding, it is important to ensure outreach of our ideas into their respective upstream communities. Otherwise our impact will remain small and instead of innovating, we will be relegated to applying lessons learned from other fields.

8 Conclusions

Starting with the observation that scientific theories must occasionally make room for new ones, we suggested six popular ideas in GIScience that may benefit from retiring (or at least rethinking) to make room for new ideas and perspectives. Among these ideas are prominent notions such as place and more specifically the idea that place cannot be successfully formalized, as well as the unquestioned usage of VGI as a term for data traces more broadly. In order to spark a discussion within the community, we illustrated how and why these six ideas may hinder progress. For instance, we questioned the default workflow of GIS, whereby every feature needs to have an associated geometry defined within an absolute reference system. We realize that this list is not conclusive and that other authors may have added some and discarded other ideas. Just as with Brockman's original book, we do not aim at consensus, but are interested in seeing arguments brought forward in support of retaining or retiring those six and many other ideas, in order to ultimately understand which core concepts GIScience should carry forward during the next decade.

[6]https://ling-cai.github.io/GIScience-GeoKG/

[7]https://geoai.ornl.gov/acmsigspatial-geoai/2021-2/

9 Data and Software Availability

This paper has no directly associated data or software. Relevant data/software are described in the referenced papers.

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