Our GIS is too small

Abstract

Geographic Information Systems and Science have been successful over the last 30 years in tackling many geographical problems. But technologies and associated theory can become limiting if they end up defining how we see the world and what we believe are worthy and tractable research problems. This paper explores some of the limitations currently impacting GISystems and GIScience from the perspective of technology and community and contrasts GIScience with other informatics communities and their practices. It explores several themes: (i) GIScience and the informatics revolution, (ii) the lack of a community-run innovation platform for GIScience research, (iii) the computational limitations imposed by desktop computing and the inability to scale up analysis (iv) the continued failure to support the temporal dimension, and especially dynamic processes and models with feedbacks, (v) the challenge of embracing a wider and more heterogeneous view of geographical representation and analysis and (vi) the urgent need to foster an active software development community to redress some of these shortcomings. A brief discussion then summarizes the issues and suggests that GIScience needs to work harder as a community to become more relevant to the broader geographic field and meet a bigger set of representation, analysis and modeling needs.

Introduction

Creating computational systems that both enable and provide a home for GIScience research is a bit like driving a car at the same time as various parts of the car are being proposed, added or modified. Vannevar Bush (1967), the great science visionary and leader who oversaw the creation of the US National Science Foundation in 1945, described the process by which a research field moves forward using the following analogy.

> *"The process by which the boundaries of knowledge are advanced, and the structure of organized science is built, is a complex process indeed. It corresponds fairly well with the exploitation of a difficult quarry for its building materials and the fitting of these into an edifice; but there are very significant differences. First, the material itself is exceedingly varied, hidden and overlaid with relatively worthless rubble, and the process of uncovering new facts and relationships has some of the attributes of prospecting and exploration rather than of mining or quarrying. Second, the whole effort is highly unorganized. There are no direct orders from architect to quarrymaster. Individuals and small bands proceed about their businesses unimpeded and uncontrolled, digging where they will, working over their material, and tucking it into place in the edifice."*
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> Vannevar Bush, *The Builders*[1]

In a field that is still evolving and expanding, it is difficult to recognize a sound overall structure from what has currently emerged and to construct it into a coherent analytical system, such as a GISystem. So we should expect that, from time to time, the structure and related systems that have emerged so far will be shown to be inadequate and will require some reworking.

[1] Although published in a book in 1967, this smaller piece appears to have been written around 1945.

This paper explores GIScience and GISystems from a *research process* perspective, questioning how the two are linked and why it is that so much useful GIScience does not seem to make the transition into a working GISystem.[2] In what follows, when you read the abbreviation GIS, this includes both GIScience and GISystems.

As a GIScientist who has morphed into a Director of eResearch (also known as eScience in Europe and CyberInfrastructure in the USA) I have had the opportunity over the last fifteen years to work with informatics researchers from many different fields. I can attest to the fact that all of these domains also contain a large amount of theory and new science. But by and large their corresponding informatics communities see their role differently: to not only propose new analytical theory and methods but to operationalize and evaluate them within a coherent and functioning ecosystem. However, and to be clear, these are personal observations and I do not claim this to be a scientific study[3].

This brief article reflects on why GIS does not empower more geography research and why the GIScience community is often not seen as a key partner by our geography colleagues. Much has been said already from a *critical* point of view on the shortcomings of GIScience and GISystems (e.g. Pickles, 1995; Schuurman, 2000; Crampton, 2010; Goodchild, 2015; Thatcher et al., 2016). However this article is not a methodological critique of GIScience, but rather a series of reflections on problems with the way we do GIScience and the GISystems we have settled for as a result.

The name *GIScience* has been the preferred designation for our research community since the early 1990s, emboldened by Goodchild's (1992) visionary rallying cry. But the generic term given to the study of the analytical methods, tools and information architecture associated with a research domain is *x-informatics*[4,5]. Hence *bioinformatics* is the study of these topics as they relate to biology and *health informatics* plays the same role for a broad section of health and medical sciences (Tolliver, 2008). There are in fact many other such communities (e.g. *astro-informatics*, *eco-informatics, cheminformatics*; Borne, 2010; Michener & Jones, 2012; Augen, 2002), a consequence of the huge shift towards digital research that has changed the nature of many disciplines over the last twenty-five or so years (e.g. Baker et al, 2008). Just like GIScience, these other x-informatics communities have their own mature journals, conferences and research agendas and they all trace their roots back to the theoretical foundation of their discipline (e.g. Hogeweg, 2011). In this company, GIScience is somewhat of an anomaly for two reasons. Firstly, as noted above it claims to be a science (in preference to computational geography or the more conventional geo-informatics (both are names that have been used to define our field at various points in time) and perhaps this sets a bias towards the discovery and development of science theory? Secondly, by and large the GIScience community does not see its primary role as providing the platform and methods that support geographical enquiry for the rest of the geographical research community, preferring instead to pursue a more independent research quest.

GIScience has indeed proven to be a very effective rallying call, and evidence suggests it successfully opened up new channels of research funding, particularly in the USA, that

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[2] Dawn Wright and colleagues (1997) present a useful overview on the distinctions between GIScience and GISystems.

[3] If emoticons were acceptable in research journals, I would place a smiley here.

[4] To gain a practical sense of the generic aspects of x-informatics, relevant across multiple research fields, this course syllabus, created by Peter Fox at Rensselaer Polytechnic, offers some useful insights: https://tw.rpi.edu/web/Courses/Xinformatics/2016 (Accessed 10th Nov, 2016).

[5] Rahul Ramachandran, Science Informatics, what is in a name? http://www.rramachandran.com/content/science-informatics-%E2%80%93-what-name (accessed, Nov 11th, 2016).

helped to coalesce the field and support much of the early progress. And there is nothing wrong at all in striving for GIScience theory (Goodchild, 2004); new theory is certainly needed. But if it is useful, then it needs to become part of the toolset for other researchers to use. There has, of course, been much success to celebrate too, many geographical problems have become accessible and solvable with GIScience, particularly where the problems involve map operations and some kinds spatial analysis (more on this later). But we can more.

This article outlines five issues with the current state of GIScience and offers a few suggestions to perhaps make us a more effective community, via comparison with other research fields that are also heavily based on informatics. It concludes with a brief discussion.

The innovation platform appears to be missing

> *There are those who are quite content, given a few tools, to dig away unearthing odd blocks, piling them up in the view of fellow workers, and apparently not caring whether they fit anywhere or not. Unfortunately there are also those who watch carefully until some industrious group digs out a particularly ornamental block, whereupon they fit it in place with much gusto and bow to the crowd. Some groups do not dig at all, but spend all their time arguing as to the exact arrangement of a cornice or an abutment. Some spend all their days trying to pull down a block or two that a rival has put in place. Some, indeed, neither dig nor argue, but go among with the crowd, scratch here and there, and enjoy the scenery. Some sit by and give advice, and some just sit.*
>
> Vannevar Bush, *The Builders*

By and large, we the GIScience research community do not own or develop our own codebase(s) and systems. This is a tragedy, and contrasts sharply with bioinformatics and other x-informatics disciplines where community-built, open-source cutting-edge systems are much more common and in many cases are widely used by their respective disciplines as their *research engines*. There are of course some excellent counter-examples of more limited scope, such as the *PySAL* open toolkit (http://pysal.readthedocs.io/en/latest/#), *PostGIS* (http://www.postgis.net/), *GeoTools* (http://www.geotools.org/) and the *GRASS* project (https://grass.osgeo.org/) that could be used as the basis to integrate and validate new methods, perhaps even new data structures and models in the case of *GRASS* and *PostGIS*. But this is not our usual practice.

So why isn't GIScience leading the development of the methods and systems needed by geography more broadly? Much of reported GIScience research to date has not found its way into GISystems; most of it remains trapped in the hallowed pages of our GIScience journals. There is of course merit in ideas for their own sake and in places to share and discuss them. But ultimately I believe that journals should not be their final resting place. They need to take material form, be used, be evaluated, then to be put to work or discarded, depending on their perceived value. Validating new theory and methods is actually a vital stage in the process of science itself (Gauch, 2003; Oreskes, 1994) and it is perhaps an uncomfortable truth that much of our GIScience research is, by this measure, not very scientific!

Contrast this with bio-informatics, where this community has provided many widely-used analysis tools, and even entire workflow systems that represent and facilitate a huge range of analytical activities; for examples *Galaxy*, *Taverna* and *Triana*, see Leipzig (2016) for a more comprehensive review[6]. One could make similar claims for the statistics field via the *R* open-source community and huge collection of tools based on *R* that are now used routinely in many fields[7]. There are also of course many highly successful commercial analytics environments within use in these two fields: but despite these the x-informatics research communities have: (i) *created* and (ii) *maintain* and (iii) *own and govern* the software platforms that successfully support open innovation and experimentation across a broad swath of the research discipline.

A sign of their success is that most of these communities can point to significant, open, toolsets that not only empower many domain research questions but also provide a cohesive and effective platform by which to validate and deploy new research—new theory, new methods and even new data models all have a natural home, can be independently verified, contrasted with established methods and rolled out to the broader research community. In such a functioning ecosystem, evolution can work to push the best of these innovations into newer versions of the informatics platforms. Such a platform also allows researchers around the globe to all contribute via development, testing, comparison and use of the tools. An excellent example of this idea in action is the data and analytics infrastructure developed by the high-energy physics informatics community to support the research carried out on the Large Hadron Collider[8].

The absence of such a platform for research makes progress in GIScience very difficult. Even when the source code is available for new methods, for example, they seldom find their way into an established research platform or toolkit because they are not engineered with this in mind. While this situation is allowed to persist, substantial progress in GIScience will be much harder to achieve, lowering our impact and importance as a necessary partner and enabler of geographical research.

Lack of a shared experimental codebase also makes it very difficult to evaluate new ideas (say new data structures or new qualitative algebras) in any consistent way, so we do not really know which methods are best, or in what situations they perform better than others. As a consequence we do not move forward in the sense that our better ideas do not end up being incorporated into our shared codebase. Or to draw on the opening quotation, we do not know which blocks are sound enough to build upon. This helps to explain why—despite huge research pushes in the areas of time, or spatial information theory, for example—we still do not have a GISystem that implements the stronger ideas in these fields. There are exceptions, where ideas from academia have successfully migrated into commercial GIS. One example that comes readily to mind is the *ColorBrewer* tool for selecting a perceptually-sound colour palette for maps[9] that has found its way into both *ArcGIS* and *R*. This particular idea would have been easy to add to an existing GIS as it would have little impact on adopted data models and workflow, but much GIScience research is more challenging to operationalize. Of course, there are also ideas that do not require implementation, or that cannot yet be implemented[10]—these should still be encouraged yet recognized as such.

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6 See https://omictools.com/workflow-management-systems-category for a succinct summary of the more popular offerings. The problem in BioInformatics is that there are so many different workflow engines and analytics toolkits to choose from!

7 Such as https://www.r-project.org/, https://www.rstudio.com/products/rstudio2/, http://r.analyticflow.com/en/.

8 See for example the virtualised analysis environment available for the Compact Muon Solenoid (CMS) experiment: http://opendata.cern.ch/research/CMS.

9 *ColorBrewer* can be accessed at http://colorbrewer2.org/.

10 The idea for which Vannevar Bush is perhaps most well known is the *Memex*—a mechanical device that was never built for discovering and connecting knowledge. It resembles the modern Internet in some respects.

Please note that this is not a criticism of commercial GIS, and it is no way *ESRI*'s fault that GIScience research does not easily migrate into its codebase. It is down to us. We got lazy. We should not expect a third party to take responsibility for validating and delivering our ideas into a GISystem that is focused on research.

A home-grown innovation platform has some clear advantages: (i) Software interfaces can be created where needed to simplify the addition of certain kinds of functionality, such as spatial analysis methods, cartographic presentation methods and external databases. (ii) It allows the research community to comparatively evaluate different methods if they all reside in the same systems, so we can established if a new method really does outperform an established one, or offers some other advantage. (iii) The community can propose and make deeper changes to the core of the system: analytical workflows, even data models and underlying data structures. (iv) The community can drive the evolution of this platform based on emerging research needs, as a common endeavour.

The problem is, it is in nobody's short-term interests to address this glaring omission. Taking a short-term view, all of us are better off sticking to small, tractable research questions that can easily be published, and avoiding these bigger and more complex issues. And in writing this, I *out* myself as part of the problem too.

The following ideas may help us reset our course a little.

- Insist that data and code be published for all new research that uses them, and in open and accessible formats using ISO and OGC interface specifications where appropriate, and preferably hosted in repositories that are owned by the community (and not owned by the journal publishers).
- Encourage or insist that new ideas be compared with established ones where feasible.
- Work towards developing community research infrastructure to provide an open codebase for GIScience research. This implies getting funding agencies on board, but the NSF-funded *EarthScope*[11] project shows that they can occasionally be persuaded to fund community infrastructure into the long term.
- Begin to value the reuse of research outcomes other than journal articles, track them and give credit for them.
- Ensure that GIScience education gives adequate attention to scientific programming, information science and mathematics, so graduates are equipped to implement and validate their ideas.

In short, we have let the available software set the limits around the research that we, and our colleagues in other fields of geography, can do, and we need to fix this.

Not all problems conveniently fit within the limits of a personal computer

Whereas many science disciplines have translated their modelling problems successfully onto High Performance Computing (HPC) or data intensive platforms to allow them to scale up to bigger datasets, higher precision or more analytical complexity, GISystems and related analysis research seems to still be forever stuck in a single, serial thread! This seems to lead to the view that if a research problem does not fit in a GISystem, because it is too computationally demanding in terms of cores needed, or memory footprint or data needs, then it is not a GIS problem. The most worrying aspect of this is that it has become so accepted that hardly anyone questions it any more. If the problem can be solved on a typical desktop computer, then it belongs to GIS, otherwise it is a problem for some other community. But in taking this line, we have created an artificial barrier between GISystems and very useful analytical functionality. We have perhaps also pushed some of the geographical research

[11] http://www.earthscope.org/

community away who need more performant analysis methods (such as erosion modelling and predator-prey analysis). Many sub-disciplines of geography and environmental science need the analysis and visualisation methods that are in current GIS, but they also need more advanced analysis and modelling tools than GIS can provide (a topic that is taken up in the next section).

Scaling up GIS analysis capabilities is by no means a trivial exercise, and in other fields much time and effort has been spent optimizing HPC codes in fields such as climate forecasting, genome sequencing, earthquake modelling, early universe cosmology and the like. But as a result of this work, there are excellent HPC templates that support many different research problems: for example sparse matrix linear algebra, *N*-body methods and structured grids that all have strong applicability to geographical research. An excellent starting point to understand HPC challenges is provided by Asanovic and colleagues (2006).

It is time that GIS scaled up: the opportunity has been understood for some time (Armstrong, 1995); the HPC templates needed to address bigger and more complex problems are readily available (e.g. https://paralleldwarfs.codeplex.com/). Note that moving GIS analysis onto virtualized servers in the cloud does *not* solve this problem at all, though it may in some circumstances provide more data throughput and a larger memory footprint than a typical desktop machine, which can in turn alleviate a different class of performance bottleneck. But it does not provide any kind of parallel acceleration—to get this we need to rework our algorithms.

There is some good work in this area to build on such as the GRASS project mentioned earlier, that has some parallel capability (e.g. Akhter et al, 2010), and new work emerging from the recent renewed interest in this field (e.g. Puri, 2015).

The world is not static and lifeless

Despite many well-reasoned pleas in the GIScience literature for the better representation of time (e.g. Langran, 1992; Peuquet, 1994, 2003), we have so far failed to incorporate this vital dimension. A useful summary of the challenges and opportunities is provided by O'Sullivan, 2005). A challenging critique of contemporary information systems in general for being 'static and lifeless' is given by Sowa (2003).

The research focus for GIS and time so far seems to revolve around providing 'temporality' for objects. At a very deep level, our ontology has favoured the primacy of objects over processes, so we have chosen to model the map and not the process that give rise to the map (or the process by which we interpret the map). Perhaps a good place to begin is the question: *What would GIS be like if it had been designed around dynamic processes rather than static maps?* Possibly it would better serve geography (as opposed to cartography) because processes are often more scientifically relevant to substantive research questions and objects are in fact just a snapshot of some process (even human-made or *fiat* objects can be represented in this way, as the outcome of a social or political process). If we could represent in GIS the processes that gave rise to objects, or simulate processes acting on objects, then we would have a much more powerful modelling environment. Such processes might be physical (such as water drainage) or socio-political (such as how a voting district was defined), or even personal and interpretational (such as the process of understanding of a map)—which might put us in a better position to address some of the concerns of critical theory too. The current state of supporting a set of coverages representing different times (at best) is a very poor facsimile of a dynamic system.

What GIScience has failed to do, where many other x-informatics domains have succeeded, is to move beyond from the static world of representing objects and into the dynamic world of representing processes—as interactions between actors, over time, with feedbacks that can actually change the underlying model. A simple example of this is landscape erosion and deposition—the process *changes* the model as material is redistributed across the landscape over time. As such it requires that the underlying data model (such as a surface) be defined or changed as a result of ongoing working of the process, not simply that a few snapshots of the process are taken and stored. Whereas there are many good examples of this functionality in fields such as cheminformatics and even ecology, this kind of functionality eludes most GIS. A notable and worthy exception is the PCRaster software from Utrecht (Karssenberg et al, 2009), that actually represents a landscape as the result of physical processes, with equations defining how the landscape changes as a process such as erosion plays out.

Equation-based dependencies between the process and the representation are needed to allow GIS to expand into a range of dynamic processes that have been marginalized or pushed into other disciplines, such as hydrology in the above example.

I believe the aspiration here should be to support the integration of complex, dynamic models: using research progress in systems dynamics and multi-physics (Keys et al, 2013; Karnopp, 2012). As an example, the current state of the art for modelling the functioning of the heart in bio-engineering requires solving different sets of PDEs for fluid flow, electro-potential and biochemistry, at the cellular level. These different physical models obviously effect each other so must be integrated at every step. In an ideal GIS, we may want to integrate models that are physically based with those that are socially driven. But the same underlying technology and mathematics would allow us to create a model of (say) water catchment and use in a subsiding environment, that is subject to a population explosion—as a single, dynamic system with feedbacks. Such models are important now and will be likely more so in the future. They will need to draw heavily from the analytical and representational capabilities of current GIS. So why not support them within GIS?

Too little of geographical research is catered for by GIS

Why is it that GIScience is not leading the development of the tools and methods needed by geographical research fields more broadly? And how do we engage with such a "*multidisciplinary and multiparadigmatic field*"? (Blaschke & Merschdorf, 2014). There have been several serious attempts to broaden GIScience theory to meet a wider swath of geographical needs, in the areas of time and process, critical theory, feminist theory, visualization, qualitative spatial reasoning and others: Reitsma (2013) provides a very useful summary of such efforts. One very interesting critique of GIS from the perspective of archaeology is provided by Hacıguzeller (2012), which has the helpful property that is from the perspective of another discipline. But still the majority of geographical problems cannot be solved in a GIS and the majority of problems that are addressed by GIScience seem to me to be ones that GIScientists have chosen for themselves—the bigger agenda of enabling geographical research appears to be largely absent or in some cases still just an agenda.

It is not only the lack of temporal dynamics and process modelling that restricts the utility of GIS to geographical analysis more generally. Existing GIS are built around two useful but limited representational paradigms: the raster grid and the vector map (usually but not always including point, line, region topology) and offers analysis methods that fit within these paradigms. There are a lot of potentially useful methods in this space, both for analysis and for visualization, and GIScience has done well here to propose, test and improve what we have. But there are entirely missing representational paradigms that are (or could be) very useful and possibly better for some tasks, such as Voronoi Spaces, hexagonal grids, spaces defined by field equations and qualitative spaces, to name just a few. These are important research topics in GIScience and geographical analysis, and have real value in terms of engaging with a broader set of research questions, but are marginalized because they are not available in a mainstream GIS for researchers to build on and turn into useful platforms. As such, we are stuck with the geographical modelling paradigms that were developed first, not necessarily the ones that are best.

The same argument can be made for missing analytical functionality, such as cartographic animation, agent-based models, spatial interaction models and the like. During the quantitative revolution, those involved imagined quantitative geography to be a vast field, with many useful analytical representations that could be explored (Haggett and Chorley, 1967; Macmillan, 1997), or later that emerging fields such as geocomputation will help to restore some of the missing analytical functionality (Gahegan, 1999). What we have settled for in GIS is far narrower than this initial vision and we are impoverished as a result. We need an analytical toolbox at the very least, which allows us to explore spatial analytical problems that fall outside of GIS analysis capability.

It is getting easier for researchers to add in missing analytical functionality to commercial systems (ArcGIS server object extensions for example) and this certainly helps. But adding missing paradigms requires that we have our own codebase. From an even more radical point of view, perhaps we could extend GIScience to embrace different epistemologies and ways of knowing, beyond those used in normal science and towards those used in critical research, or in knowledge discovery?

This points to two important needs, going forward:

- Design systems so that it can accommodate new methods, modelling paradigms and theories, i.e. that can evolve. Designs will need to be very modular and provide well-documented interfaces at multiple level of abstraction.
- Recognize the system(s) in use, i.e. the methods, models and theories we are using, and their shortcomings, and make plans to address them. We are good at the former (recognition and creation of research agendas), but poor at the latter.

Stronger engagement with the open-source community is urgently needed

> "*There are those who labor to make the utility of the structure real, to cause it to give shelter to the multitude, that they may be better protected, and that they may derive health and well-being because of its presence.*"
>
> Vannevar Bush, *The Builders*

The open source movement extends right across the research sector and has done a marvellous job of not lowering the financial barrier of entry into many research domains but also of democratizing the software development process (Warger, 2002). One of the most exciting things to emerge in the GIS universe in the last few years is the Open Source Geospatial Foundation[12] (colloquially known as OSGeo), whose dedicated community is committed to developing and delivering GISystems and technology following open-source principles. The work here is highly laudable and the community is clearly both really passionate about their vision and supportive of the idea that GIS needs to be open for it to progress. They have created some strong, alternative GIS platforms based on open codebases, for example *OSGeo4W*[13] and *QGIS*[14]—ones that are owned by the community and thus can be modified as the community sees fit. It demonstrates that building and maintaining a GIS codebase (or an innovation platform for GIScience) is indeed a viable goal for a community of determined people.

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[12] http://www.osgeo.org/content/foundation/about.html
[13] OSGeo4W: http://trac.osgeo.org/osgeo4w/
[14] QGIS: https://www.qgis.org/en/site/about/index.html,

Perhaps equally importantly, these activities are producing a new generation of programmers, with all the knowledge and skills needed to create and maintain their GIS platforms and systems (Elliot and Scacchi, 2008). As a community, I believe we should be more engaged with OSGeo, and I note that both the ICA and the ISPRS are already collaborating with OSGeo. For our long-term health, we need to join forces with this group, GIScience needs what they can offer.

The missed opportunity—so far—is that open GIS projects are often based around replicating the functionality of existing commercial GIS, and so are usually based on the same restrictive assumptions about the world, the map and the importance of structure over process. But we could work with this community to create an open GIScience research platform as a fast track resolving some of the issues described above, with shared governance.

Discussion

To revisit the opening quote: it is easy to throw rocks, far harder to be constructive and suggest how they might be fashioned instead into a useful structure. The problem is NOT commercial GIS, because they are responsive to the commercial market. The problem is creating and maintaining an effective platform for research, and by researchers.

Issues to address here are:

1. Market Failure: The market has not provided what we need, a research-focussed software platform, nor will it. We need to take this upon ourselves as many other x-informatics communities have before us.
2. Funding Failure: Lack of focus on sustaining research infrastructure, as opposed to the next round of new ideas.
3. Education Failure: Lack of coding and software engineering skills among GIScientists.
4. Cultural Failure: An academic culture that fails to reward those who build or maintain tools and software and encourages a short-sighted and individualistic approach to research.

GIScience sometimes reminds me of a smart college graduate: passionate, articulate, and capable; brought up by loving but rather self-absorbed parents; still living at home, in the basement, playing video games and afraid to take on any real responsibility. If we want GIScience to remain vibrant and relevant then I believe we need to urgently we need to rethink how we meet some of the challenges described above. We need to set our sights higher, think bigger, take a deep breath and imagine what a shared, open, dynamic, geographic analysis platform could do for us, if it integrated all of those complex models and simulations that now live outside of GIScience. Add to that the layers of societal, infrastructural, demographic and health-related data that we DO understand and can analyse well; now that would be an impactful and highly relevant x-informatics, one perhaps worthy of being called a science.

In *The Builders*, Bush (1967) goes on to describe the importance of those who unearth new ideas, cautions about those who take credit when they did not really do the work, or spend their time arguing about less important details. He underscores the need for a functioning community of many roles, all working together, including: visionaries who can see in advance where pieces may fit; those who give meaning and context to the edifice; those who construct the emerging pieces into a working structure and make it useful; those who encourage and facilitate behind the scenes.

Some self-reflection is needed by each of us: what is our contribution and how does it help? In the GIScience literature we seem to have a lot of papers on research agendas, conceptual frameworks, reviews of progress in the field and papers defining what the field is or critiquing it. We also have a large pile of interesting ideas, most of which are still lying on the floor of the quarry in a heap, waiting for somebody to incorporate them into a working system and thereby establish their worth.

It seems appropriate, by way of encouragement, to let Vannevar Bush have the last word.

> *Finally, the edifice itself has a remarkable property, for its form is predestined by the laws of logic and the nature of human reasoning. It is almost as though it had once existed, and its building blocks had then been scattered, hidden, and buried, each with its unique form retained so that it would fit only in its own peculiar position, and with the concomitant limitation that the blocks cannot be found or recognized until the building of the structure has progressed to the point where their position and form reveals itself to the discerning eye of the talented worker in the quarry.*
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> Vannevar Bush, *The Builders*

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Key Messages (requested by the journal submission system)

1. GISystems and Science, like any enabling technology and theory, can become limiting if they end up defining how we see the world and what we believe are worthy and tractable research problems.

2. Like 'R' is to statistics, the GIScience community needs an innovation platform by which to deploy and validate new ideas, and to empower geographical research more broadly.

3. At a very deep level, our ontology has favoured the primacy of objects over processes, so we have chosen to model the map and not the process that give rise to the map.