Trading Zones or Boundary Objects:

Understanding Incomplete Translations of Technical Expertise

Nicholas Chrisman
chrisman@u.washington.edu
Department of Geography Box 353550, University of Washington
Seattle WA 98195-3550 USA

ABSTRACT

Sharing technology with a larger community of users involves a process of explaining the technology, what it does, and how to use it. In simplest form, technology simply diffuses, unchanged from its original conception. While diffusion offers a neat model for certain processes, few technologies are unchanged as they develop and become implemented in particular situations. From the many alternative explanations of this process, the concepts of boundary objects and trading zones both offer explicit room for the incomplete nature of translation. This paper explores this process using examples drawn from the use of geographic information systems (GIS) technology, particularly as it has been applied to wetland mapping in the United States. Much of the treatment of GIS implementation consciously acknowledges the potential for incomplete translation, but also involves strong forces attempting to enforce a single solution through standards and other political mechanisms. Despite these overt attempts, the lack of complete translation is not difficult to document. In the case of GIS representations, the digital artifacts from various perspectives can be compared, and differences tied to specific parts of the landscape. The paper applies both the trading zone and the boundary object interpretation to the issue of wetland mapping, finding particular circumstances better handled by one or the other. Overall, the creation of a common interlanguage (a pidgin), as required in the trading zone approach, is harder to find. At best, the users of GIS seem to "agree to disagree", a situation better explained by boundary objects.

How do diverse groups share information?

It is more than slightly presumptuous to address such a long-standing issue in a paper of modest dimensions, yet the question "How do diverse groups share information?" must be taken as the starting point. This question might sound overly broad, and I have no intention of providing a complete history of the philosophy of knowledge, nor do I intend to poach in the disciplinary expertise of those much better trained for such pursuits. By contrast, I would like to offer an empirical example to contribute to some recent threads of research.

This topic is motivated by Galison's *Image and Logic* (1997), detailed history of the instrumentation of physics in the twentieth century. The theoretical frame for Galison's work builds directly upon Kuhn's (1970) development of the "paradigm" as an organizing principle for scientific communication.

Foundations of Science

When the literature consists of a cascade of theoretical refinements, it makes somewhat more sense to begin at the beginning and introduce the critique as it was developed. For many centuries, the dominant approach to science has highlighted the cumulative accretion of knowledge. Hacking (1981) sets this as one of the nine points that formed the "image of science" that Kuhn transformed.

1. *Realism*. Science is an attempt to find out about one real world. Truths about the world are true regardless of what people think, and there is a unique best description of any chosen aspect of the world.
2. *Demarcation*. There is a pretty sharp distinction between scientific theories and other kinds of belief.
3. Science is *cumulative*. Although false starts are common enough, science by and large builds on what is already known. Even Einstein is a generalization of Newton.
4. *Observation-theory distinction*. There is a fairly sharp contrast between reports of observations and statements of theory.
5. *Foundations*. Observations and experiment provide foundations for and justification of hypotheses and theories.
6. Theories have a *deductive structure* and tests of theories proceed by deducing observation-reports from theoretical postulates.
7. Scientific concepts are rather *precise* and the terms used in science have fixed meanings.
8. There is a *context of justification* and a *context of discovery*. We should distinguish (a) the psychological or social circumstances in which a discovery is made from (b) the logical basis for justifying belief in the facts that have been discovered.
9. *The unity of science*. There should be just one science about the one real world. Less profound sciences are reducible to more profound ones. Sociology is reducible to psychology, psychology to biology, biology to chemistry, and chemistry to physics. (Hacking 1981, p. 1-2)

Hacking admits that this is a pastiche of opinions, and no one person held all of them in such clear form, however, he also points out that these do inform the popular conception of science. Some of these nine points have a strong bearing on the issue of sharing geographic information. The first would be the naive realism of a single knowable world. Under this premise, there is a true rendition of a geographic feature, and it is just our job to "discover" it. This links to number seven, which asserts that meaning can be determined accurately. These beliefs are self-reenforcing and become the guides to public policy and administrative procedure, even when there is substantial evidence to contradict them.

One manifestation of cumulative development (number three) is the quotation "If I see far, it is because I stand on the shoulders of giants" which has been ascribed to just about every prominent scientist from Einstein to Newton. A part of the ethos of science is, of course, that the scientist themselves should not claim all the credit, so probably each one of these prominent scientists actually uttered the statement. In the first half of the twentieth century, the logical positivist approach made much of cumulative development (Carnap 1966, for example). If knowledge is to be cumulative, it must be shared, and it must be shareable. Among other elements, the sharing process requires a geography and a temporal coherence. The geographic element is required to move knowledge between different knowledge workers, implying some kind of central place hierarchy of knowledge dissemination and a consequent division of labor. The temporal process is perhaps more critical. Knowledge produced at one time does not become stale, it can be used later, if appropriately preserved. From this brief description, it should be apparent that these pre-Kuhnian assumptions about knowledge sharing are quite unreasonable. No knowledge infrastructure has been able to surmount the vagaries of time and space. Yet, many organizations seem to hold onto these articles of faith as the principles of their operations.

The Kuhnian Revolution

While there is certainly some kind of long term cumulation of scientific knowledge, there are also "revolutions" in the practice of science when some new approaches seem to wipe the slate clean and redirect efforts in a new set of questions, using a new set of tools. Kuhn (1970) argued that these revolutions were not aberrations, but a necessary element. According to Kuhn, a dominant "paradigm" which controls the way that a group of scientists see the world. Work referenced to one paradigm can be incommensurable to work produced in another. The term "paradigm" has come to permeate science and many other fields (including a thread of books for management seminars and marital reconciliation). In geography, for example, there was a period of declaring a new revolution every two or three years, a rate not contemplated in the story of long periods of "normal science" punctuated by episodes of revolution. Nobody wanted to be associated with "normal science" when the real payoff would come from replacing the dominant paradigm. Perhaps once the concept of paradigm becomes recognized, it is no longer possible to hold a revolution in quite the same unself-conscious manner.

Another consequence of Kuhn's work is that diffusion takes on a different character. In place of the spread of an idea through a passive matrix, a paradigm operates in a dialectic with some prior opposing framework. First it must convince, then it may have to wait for the unconverted to simply die off. However, the central concept of incommensurability means that groups not sharing in the paradigm cannot simply absorb "facts". In its initial form, the concept of paradigm was fairly sweeping. The realm of theory took on a new role in influencing what was observable. Sharing of information was basically constrained by the framework of a set of paradigms, but within them, it seems that the old rules (one to nine above, roughly) could be expected to apply in some local sense.

Beyond paradigms

Galison (1997) sets out to demonstrate that the paradigms of theoretical physics did not dominate the subdisciplinary world of instrumentation. Since Kuhn had made his argument using theoretical physics as his central example, Galison's book provides a challenge to the simplification of paradigms. In its place, Galison offers a linguistic analogue, a so-called "trading zone" where participants from different cultures communicate using a stripped-down interlanguage much as traders created pidgins or creoles that operated on the interface between social groups. This paper will not presume to criticize the case made by Galison, but seeks to see if it can be applied to the communication between the distinct disciplines involved in the use of geographic information technology.

The use of a linguistic analogue is not new in the study of science and technology. The nineteenth century made much of mathematics as a "universal language" to unify all scientific endeavor. Latour, in a series of books about science (Latour and Woolgar 1986, Latour 1987, 1988, as a starting point), developed the term "translation" to refer to the actions performed by scientists in making the connections between microbe and disease, laboratory and the world. Rather than just recognizing incommensurability, Latour directed his efforts to how the process was carried out. While Latour's work is often read (and even originally written in English sometimes), it must be remembered that the French word for "translation" also means "treason", and it is a part of the Actor Network folklore to make this connection (Law, 1997). Translation never converts meaning flawlessly, either in regular usage or in specialized analysis of science. In this community of scholars, the term "boundary object" has been developed to handle one form of incomplete translation (Star and Greisemer 1989, Fujimura 1992). Galison (1997, p. 47) in a footnote takes notice of boundary objects as "most congenial" with his notion of trading zones, but more or less brushes aside this line of work and does not investigate alternative explanations in his empirical work. His reason to use trading zones seems to be that the interaction is not limited to "objects" but includes processes and activities.

Mediation of diverse interests occurs in the construction of artifacts; negotiations are necessary to create coherent operations. Star, Greisemer and Fujimura develop the relationships between multiple actors and artifacts through what they call *boundary objects*. Boundary objects mediate between different groups; they don’t provide a common understanding or consensus between participants. They don't create a common language or a perfect translation. Instead, boundary objects serve a dual function: at the same time they serve to distinguish differences, they also supply common points of reference (Harvey 1997a, Harvey and Chrisman 1998).

Institutions and disciplines play a crucial role in formulating boundary objects that allow for stable translations between different perspectives on the same phenomenon.

These two concepts (trading zone and boundary object) are indeed rather compatible, since both seek to refine the basic idea of translations. The key difference comes in what they expect to see in the middle of the process. Galison's trading zone of scientific cooperation is instantiated in Safdie's proposed architecture for the Superconducting Super Collider in Texas (Galison 1997, p. 831), as opposed to the "positivist" disciplinary divisions at Brandeis (p. 786). The activity in the trading zone requires a new pidgin to emerge; an interlanguage like the realm of computer simulations. By contrast, the traces of a boundary object are not so architectural, rather they imply a negotiation process and an agreement to understand that the other group may not see things as you do. There is no new language here, just an awareness that the other side has different meanings to attach to the already common words; the other side measures the same things differently. Before a boundary object is recognized, each side might think that their terminology was universally understood. Once the negotiation is complete, there are new steps to make adjustments for the differences in meaning.

This paper seeks to contribute to the study of technology and science by providing a modest example of how information is actually shared, interpreted inside these alternative frameworks. It will also provide some assistance to the field of geographic information technology in clarifying which form of knowledge sharing we should really expect to see.

Integration: fundamental to geographic information technology

Geographic information systems were specifically directed, from their inception, to integrating information from different sources. Integration implies sharing. Definitions of "GIS" (Maguire 1991, Chrisman 1999) have used this role as a distinctive characteristic, or even a litmus test. Yet the actual steps by which integration occurs are not quite as transparent as they are supposed to be (Harvey 1997b). Nyerges (1989) sets out two procedures for integration. The geometric approach uses collocation as the primary criterion and relies upon an overlay calculation to connect themes. This geometric technique was certainly a founding element of information systems from the Canadian Geographic Information System (Tomlinson 1967) to the current expansion of GIS (Chrisman and Niemann 1985, for example), but the limitations of overlay have been clear to most research scholars (Chrisman 1987a, for example). Nyerges sought to introduce a second approach to integration based on "meaning" by which distinct schemas for information were matched on structural characteristics such as name of the variable, cardinality, and other representational details. This process is placed in a formal linguistic setting seeking "equivalences" between elements of two database descriptions (a process that requires a fairly strict form of translation, though that word is not employed). The attempt to formalize the semantics of geographic information have proven to be rather more complex than originally imagined. There does not seem to be any magic automated procedure to recognize what the critical difference might be that renders two representations incommensurable.

Schema integration places great store in the definitions of terms and the usage of words. In practical situations, it seems much more frequent for multiple participants to use common words to mean quite different things. Integration will not find perfect equivalence, but rather some kind of associations and partial matching. The example of wetlands mapping in the United States offers a chance to examine how different maps can be when they are meant to be mapping a phenomenon labelled with the same words.

Wetland mapping: a case of failure in sharing

For a long string of reasons, wetlands have become a major public policy issue in the United States, and a cause of nearly universal support. While they were once seen as a miasma of decay and foulness, wetlands have reversed their public image in a way that even Aldo Leopold might find amazing. The pessimism about progress in the *Sand Country Almanac* (Leopold 1949) must be somewhat tempered fifty years later by some version of a land ethic, at least as far as wetlands are concerned. Both Presidents Bush and Clinton have fully endorsed a policy of "no net loss", and (by at least some measures) the total area of wetlands (though perhaps not their ecological functioning) might have stabilized (Helmich and Melanson 1995, Tolman 1995, Wilen 1995). By making the national policy revolve around "no net loss", the measurement of loss or gain becomes a major issue in itself.

Some previous studies have noted the radically different approaches inherent in various forms of environmental surveys. The regulatory and tax views of wetlands in one town in Wisconsin showed almost no association with each other (Sullivan, Chrisman and Niemann 1985, Chrisman 1997, p. 229). In this situation, the tax assessor estimates the quantity of wetlands for each parcel, while a regulatory agency mapped the wetlands. Only 19 of 119 parcels had an area measurement within twenty percent agreement, and 54 parcels were not considered a wetland at all by the one or the other.

	number of parcels
Wastelands assessed, Wetlands not mapped	16
Wastelands assessed, only Farmed Wetlands mapped	3
Wastelands exceed Wetlands by more than 20%	36
Wastelands match Wetlands within 20%	19
Wetlands exceed Wastelands by more than 20%	7
Wetlands mapped, Wastelands not assessed	38
Total non-exempt parcels with Wetlands or Wastelands	119

Table 1: Comparison of Wetland Inventory to Wasteland assessment
Source: Sullivan, Chrisman and Niemann, 1985 Table 1

In the Wisconsin case, the two agencies were quite unaware of each other's existence and the inconsistency between the tax incentive and the regulation might have been hard to ascertain without a GIS toolkit. It is true that the "wasteland" category used by the assessor could include other kinds of non-productive land, but in this particular township wetlands were the exclusive reason for using the category. This case is not an example of sharing, because no one expected the categories to be comparable at all.

The US federal government provides a particularly detailed study of the nature of the divergence in wetland mapping results for agencies attempting to respond to the federal policy of no net loss. In 1995, the Wetlands Subcommittee of the Federal Geographic Data Committee published a report on a comparative study of wetlands inventory activities in Wicomico County, Maryland (Shapiro 1995). In methodical detail, this report sets forth a story of substantial disagreement in the areas determined to be 'wetlands' by a number of mapping projects conducted by federal and state agencies. The group collected eight sources that included estimates of the amount of wetlands in this county, produced by six agencies. Two of the agencies produced both full-coverage maps and sampling frames (either points in the case of the NRCS NRI or small plots in the case of FWS-SAT). Because these approaches were so radically different, the comparison concentrated on the full-coverage mapping, but the reports on national trends in land-use change are often generated from these sampling techniques due to their more frequent repetitions and their emphasis on greater consistency. Two of the agencies (US Geological Survey and Environmental Protection Agency) withdrew their sources from the comparison prior to the analysis, on the grounds that their material was not generated for wetlands mapping specifically, and the scale or date made them inherently less compatible. This claim is understandable in the case of USGS GIRAS data since it was based on 1973 photographs, and compiled for rather generalized purposes at a scale of 1:250,000 not the 1:24,000 used by most of the others. Still, it would have been interesting to see if the GIRAS source with all these limitations was actually any less useful than the others. The overall mission - to detect net loss - requires temporal comparisons, and it seems futile to sit and wait until the correct data sources become available. In circumstances of changing technology, there may never be a chance to perform a proper change analysis (Chrisman 1998). In the case of EPA, they withdrew their material from the test because they used the NWI material as a source in their compilation process. Again, it would have been useful to see if this data source, designed for ecological assessment, was compatible with other sources. Clearly, these two agencies felt they had something to lose in the testing process.

In comparing the four most compatible sources [Fish and Wildlife Service (FWS) National Wetland Inventory (NWI), Maryland (MD) Water Resources Agency (WRA), National Oceanic and Atmospheric Administration (NOAA) Coastal Change Analysis Program (CCAP), Natural Resources Conservation Service (NRCS) Wetland Inventory (WI)], only 8% of the area determined to be wetland in at least one of the sources was similarly classified by all four (Table 2).

	0 data sets	1 data set	2 data sets	3 data sets	4 data sets
FWS-NWI	91796	653	2099	4789	5444
MD-WRA	91796	1531	3838	6292	5444
NOAA-CCAP	91796	7798	11366	6001	5444
NRCS-WI	91796	27140	13205	5649	5444
Total (acres)	91796	37122	15254	7577	5444
Totals (%)	58.4%	23.6%	9.7%	4.8%	3.5%

Table 2: Agreement between wetland mapping sources in acres
based on (Shapiro 1995 Table 4, p.38) [The column "0 data sets" records area never classified as wetlands; the last column was classified in all four sources.]

The goal of wetland mapping is to detect change, and to interpret a loss or gain of wetlands. These four sources were not obtained on exactly the same dates; NWI was the oldest with 1981-82 photos, while CCAP and WRA used 1988-89 sources. NRCS used a variety of sources dating from 1970 through to 1987-89. However, there is no simple alignment of the differences. The majority of the area in wetlands was only judged to be in wetlands by a single source. Some of these differences might be written off as the result of the application of different mapping technology. The CCAP project used satellite imagery in 30 meter raster cells; NRCS attached wetland attributes to soil series polygons; while NWI and WRA adopted more traditional photointerpretation. These two sources used the identical Cowardin classification system (along with a similar mapping technology), and consistent results were not obtained. The Fish and Wildlife Service mapped 10,641 acres of Palustrine wetlands, while the Maryland Water Resource Administration mapped 14,581 acres, but they only agreed on 7,214 acres. 10,504 acres were mapped as Upland (not any kind of wetland) by one source and Palustrine by the other (Table 3 (Shapiro 1995, p. 49) Visit http://www.usgs.gov/wetlands/task3a.html#spatial for the map overlays).

	Palustrine	Lacustrine	Riverine	Estuarine	Upland	Total
Palustrine	7214	33	8	15	7311	14581
Lacustrine	32	458	0	0	58	548
Riverine	178	5	567	41	61	852
Estuarine	24	0	1	1044	48	1117
Upland	3193	49	29	74	136705	140050
Total	10641	545	605	1174	144183	157148

Table 3: Comparison of wetlands mapped by NWI and MD WRA
(Rows: MD WRA; Columns FWS NWI; all figures in acres; shading for agreement)

This kind of disagreement signals many difficulties in administering programs to preserve wetlands, among other purposes. It also points to significant difficulty in the social control of GIS technology. Due to the expense, GIS data is not the result of replicated experimental conditions. Agencies are usually trusted to obtain their own data and to perform quality control. Professional expertise and disciplinary training plays a significant role in rendering the results useful to others. GIS sources are highly localized and contingent on a maze of technical details. In addition, these maps demonstrate the power of government agencies to create a landscape in a certain image. Wetlands maps restrict human activity, the change taxes and allocate agricultural subsidies. The nature of mapping technology intervenes in these policy spheres.

This incoherence between different agency views suggests that the strategy of the subcommittee cannot succeed. Consistency cannot be created by sharing all the details of the technology. Each map product in the Wicomico study had been constructed by a particular set of people, at a particular time. Even with the best intentions of replicating a shared agenda, the results were different. The managerial approach to geographic information attempts to reduce these kinds of differences by promulgating standards and sharing technical tools. However, this is only one force at work. Each of the agencies mapping Wicomico County had sound technical arguments for conducting their work as they did.

A section of the Subcommittee report contains detailed results from some highly ambiguous accuracy tests. It seems to be very difficult to establish where wetlands boundaries fall in the field. Some characteristics might be present at a point, not present 100 feet away, then present again 100 feet further on along a transect. These locations did not show any clear pattern in relation to the boundaries drawn by the four sources. Despite valiant attempts to validate their sources in the field, the recourse to realism did not provide a unifying standard for comparison. It is not easy to declare one source "wrong" while the others are "right".

Call for standards

The Wetlands Subcommittee, a group of professional employees of the federal government, used their report to call for the creation of standards – a common technique to regulate an information society. By 1999, this group had succeeded in adopting the Cowardin taxonomy for the identification of wetlands (Cowardin and others 1979). This standard has been the subject of some opposition by other agencies with specific stakes in the wetlands arena (Wetlands Subcommittee 1995). None of the comments took note of the failure of NWI and WRA to obtain reasonable similar results with similar methods and the Cowardin manual. The main issue for the Corps of Engineers is that Cowardin recognizes a wetland if one or more of three properties (hydrophytic vegetation, hydric soils, saturation) are present. The regulation enforced by the Corps (Section 404 of the Clean Water Act) requires all three attributes to be present. Ironically for the mapping programs (FWS, NRCS, EPA), the mandate (Chrisman 1987b, for the usage of this term) for wetland regulation requires on-site inspection, not a mapping source. There was a period of "manual wars" in 1987, 1989 and 1991 during the Bush administration (Swarth 1998). At the start, four agencies (FWS, EPA, NRCS and the Corps) had their own separate manuals (the FWS manual was the 1979 Cowardin report). The 1989 draft "Federal Manual for Identifying and Delineating Jurisdictional Wetlands" was opposed by development interests as too loose. The 1991 version, strongly influenced by Dan Quayle and the development interests, was much more restrictive about what could be called a wetland. When the Clinton administration took office, the situation was returned to the status of the original manuals. The scientific argument (as opposed to the economic consequences) boils down to the use of one criterion from three or all three in combination. In somewhat oracular prose, the National Research Council's report on the subject seems to provide solace to both sides:

> A wetland is an ecosystem that depends on constant or recurrent, shallow inundation or saturation at or near the surface of the substrate. The minimum essential characteristics of a wetland are recurrent, sustained inundation or saturation at or near the surface and the presence of physical, chemical and biological features reflective of recurrent, sustained inundation or saturation. Common diagnostic features of wetlands are hydric soils and hydrophytic vegetation. These features will be present except where specific physiochemical, biotic, or anthropogenic factors have removed them or prevented their development. (Lewis 1995, p. xx)

Despite the emphasis on water in this definition, the remote sensing tools (airphotograph or satellite) used in most mapping focus on the vegetation component. The aspect of habitat comes largely from the biological elements on the landscape, though these are sustained by the hydrological processes. Similarly, the hydric soil element used by NRCS is also judged to be secondary, caused by the inundation. The Corps, in their commentary opposing the Cowardin standard, supported their "Hydrogeomorphic" classification scheme because it relates to the "functions" of wetlands, not the consequences. There is no sign here of a trading zone where the participants develop a common basic language. Each group holds out for their version of the manual, and works through the bureaucratic jungle to implement their viewpoint as some kind of statutory authority or standard.

The Cowardin classification manual, proposed as a standard, is a cumbersome scientific document. It provides a whole series of codes for various phenomena, but does not apply them as exclusive categories in the typical taxonomy (such as the land use codes used for USGS GIRAS which were simply 34 discrete categories). There are elements of hierarchical taxonomy at the "system" level used in Table 3, descending down three more levels through subsystem to "class" and "subclass". For example, a cattail stand is a Palustrine Emergent Persistent wetland (PEM1); a coral reef is Marine Subtidal Reef Coral (M1RF1), and so on. However, a polygon mapped according to these rules does not have to be wholly in a single category. It is quite common to append various subclasses, and even classes together to show all the various types of wetland located somewhere in the polygon. It is quite common to have a PEM1/OW if there is open water in the middle of the cattail stand. Similarly, there is a list of "modifying terms" outside the hierarchical scheme. These cover water regime, water chemistry, soil and "special modifiers" (like partly drained, farmed, artificial, etc.). These modifiers can be inserted at will with the class mixtures. In practical terms, the Cowardin codes do not come in a fixed list. There are a combinatoric maze. Each new map sheet may contain a code never seen before. Is this an interlanguage? It seems a lot more like the "fractional code" developed in the early days of landuse inventories (Finch 1933) to record all manner of attributes in the days before GIS regularized the association between object and attribute.

Outline of results:
From this review of wetland mapping in the United States, the control of time seems to play a central role. As Rachel and Woolgar (1995) saw in their observation of a software business, the definition of what is "technical" is usually a claim about time. In their case, the future was associated with advance, progress and the technical. In the cases where remote sensing is used for wetland mapping, this seems to occur as well. However, some of the other temporal claims point backwards. The use of photointerpretation for NWI is largely justified to keep the project consistent. When NWI was started in the 1970s, satellite remote sensing tools were unproven.

The references to time are not just about the technology of making the maps. In the case of the NRCS, the use of soil maps tries to peel back the layers of human disturbance to determine which areas had been wetlands in the past. Over sufficient time, hydrology, vegetation and soils all come into ecological equilibrium. Thus areas with those kinds of soils probably were wetlands before humans started to manipulate the vegetation and the hydrology. However, is the regulatory function to restore some past landscape or simply to preserve the currently viable wetlands? This ecological view of time (thousands of years) runs directly at odds with the bureaucratic view of history.

Even the Corps of Engineers is making a temporal claim when they invoke their legislative mandate. The rules they enforce were established by a prior Congress, thus it takes precedence in the administrative logic. Other agencies of the federal structure take pride in the date of their establishment. The nautical charting and geodesy group has had a dozen names of the the years, but they remember the 1803 creation of the Bureau of the Coast much more than the twenty year gap in which the agency didn't exist. Census Bureau points with pride to its role in the Constitution and the 1790 census. Thus the Corps of Engineers is saying: 'We have a direct legal sanction for our rule; you do not. If you want to change how we do things, go talk to Congress'. Much of this is an argument from bureaucratic strength, but it is also a promise of lengthy involvement in a process whose conclusion is not always controlled by scientific logic. Porter (1995) provides a detailed study of the Corps of Engineers in an earlier period in which the Corps developed the numerical procedure of cost-benefit analysis to buttress their requests to Congress for appropriations for dams and flood control. While the other agencies were scientifically oriented, the Corps has much stronger political links than military ones.

Each claim about time invokes another kind of time. In the technical sphere, being up to date is of great value, but this has little relevance to the ecological claim or to the bureaucratic claim. Some historians claim that the past is a different country. In this case, each agency attaches themselves to a different past or future (Chrisman 1993, for more on multiple time threads).

In addition to time, there are other threads of interpretation that explain the differences. In the application of various technologies, each group makes choices about which elements can be trusted. In the remote sensing interpretations, Oak Ridge inferred that all pixels that had similar mixtures of color (on the four Landsat bands) were in the same land cover as the training sites. By contrast, NRCS trusts the inference of the complex chain of logic that goes into making a soil map (Chrisman 1986, for example). NWI trusts the visual recognition of trained interpreters working on color infrared airphotographs, but this faith was challenged by finding the discrepancies between the Maryland WRA and their own. The FGDC report includes a lengthy footnote explaining that NWI recompiled their inventory for four of the six quadrangles in Wicomico County. This note claims that the original NWI was too restrictive, though Table 3 shows error in both directions. For whatever reason, FWS circled the wagons and created a new result more similar to the WRA results (using their photographs and field surveys). This is hardly a grand victory for the Cowardin standard.

Trading Zone or Boundary Object?

At the start, this paper asked a question about the processes of sharing information. These wetlands maps are certainly multidisciplinary. There is certainly a need for sharing, but there seems to be very little of it in practice. Each group attempts to make their view of the world enshrined as the standard so that the others will have to adjust to it. At one time the rhetoric is scientific, at times it is political, but at all times the message of bureaucratic preservation shines right through.

In Galison's terms we should expect to find some kind of trading zone where each group sheds its special jargon to work together in a simplified pidgin. Beside the computer representations, there is little sign of such an interlanguage. By contrast, there is a lot of evidence for negotiation, and various forms of diplomacy by other means. The overall term "wetland" is accepted by all, and it is understood that each group does not really implement this common term in the same way. The rules of the wetland game require complex interpretations to figure out what the other group really means. The wetland mapping system does not serve all its clients very well because there is so much hidden disagreement.

CONCLUSION

The story of wetlands mapping in Wicomico County establishes some bounds on the philosophy of geographic information. It seems clear that there is no "true" wetland out there simply waiting to be measured. The FGDC report amply demonstrates that the Kuhnian view that all measurement is theory-laden. The failure of standards and procedures to obtain a comparable result also demonstrates that instrumentalism (or operationalism) will not solve the problem either. We are left with a constructivist alternative. We must understand a lot more than traditional metadata to be able to understand how to integrate sources from radically different origins. This kind of work is not hard or exotic; it has been a common element of human societies for millennia. Some linguistic metaphors capture some useful tools to address the problem, but pidgins and trading zones are simply extensions of a translation metaphor. The boundary object concept adds the understanding of awareness that you don't understand the other party completely. The agreement to disagree is central to making society and technology cooperate to common goals.

ACKNOWLEDGEMENTS

Supported by a grant from National Science Foundation Grant SBR 98-10075.

REFERENCES

Carnap, R., 1966, *Philosophical foundations of physics; an introduction to the philosophy of science* (New York: Basic Books).

Chrisman, N. R., 1986, Quality report for Dane County soil survey digital files, In *Report 7*, edited by H. Moellering (Columbus OH: NCDCDS) , pp. 78-88.

Chrisman, N. R., 1987a, Accuracy of map overlays: a reassessment. *Landscape and Urban Planning*, **14**, 427-439.

Chrisman, N. R., 1987b, Design of information systems based on social and cultural goals. *Photogrammetric Engineering and Remote Sensing*, **53**, 1367-1370.

Chrisman, N. R., 1993, Beyond spatio-temporal data models: A model of GIS as a technology embedded in historical context. In *Proceedings Auto-Carto 11* pp. 23-32.

Chrisman, N. R., 1997, *Exploring Geographic Information Systems* (New York: John Wiley).

Chrisman, N. R., 1998, Beyond the snapshot: Changing the approach to change, error and process, In *Spatial and Temporal Reasoning in GIS*, edited by M. Egenhofer and R. Golledge (Oxford: Oxford University Press) .

Chrisman, N. R., 1999, What does GIS mean? *Transactions in GIS*, **4**, 175-186.

Chrisman, N. R. and Niemann, B. J., Jr., 1985, Alternative routes to a multipurpose cadastre: merging institutional and technical reasoning. In *Proceedings AUTO–CARTO 7* pp. 84-94.

Cowardin, L. M., Carter, V., Golet, F. C. and LaRoe, E.T., 1979, Classification of wetlands and deep-water habitats of the United States. *FWS/OBS 79/31* (Washington DC: Fish and Wildlife Service).

Finch, V. C., 1933, Montfort: a study in landscape types in southwestern Wisconsin. *Geographical Soceity of Chicago Bulletin*, **9**.

Fujimura, J. H., 1992, Crafting Science: Standardized Packages, Boundary Objects, and "Translation", In *Science as Practice and Culture*, edited by A. Pickering (Chicago: University of Chicago Press) , pp. 168-211.

Galison, P., 1997, *Image and Logic* (Chicago: Chicago University Press).

Hacking, I., 1981, Introduction, In *Scientific Revolutions*, edited by I. Hacking (Oxford: Oxford University Press) , pp. 1-5.

Harvey, F., 1997a, Agreeing to disagree: the social construction of geographic information technology. In *Proceedings GIS/LIS '97* **1**, pp. 808-815.

Harvey, F., 1997b, From geographic wholism to geographic information system. *Professional Geographer*, **49**, 77-85.

Harvey, F. and Chrisman, N. R., 1998, Boundary objects and the social construction of GIS technology. *Environment and Planning A*, **30**, 1683-1694.

Helmich, R. and Melanson, J., 1995, Wetlands lost, wetlands gained. *National Wetlands Newsletter* **17**(3), 1, 23-25.

Kuhn, T. S., 1970, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press), second edition.

Latour, B., 1987, *Science in Action* (Cambridge MA: Harvard University Press).

Latour, B., 1988, *The Pasteurization of France* (Cambridge MA: Harvard University Press).

Latour, B. and Woolgar, S., 1986, *Laboratory Life: The Construction of Scientific Facts* (Princeton NJ: Princeton University Press), 2nd edition.

Law, J., 1997, Traduction/Trahison: Notes On ANT. (Lancaster UK, Sociology University of Lancaster) http://www.lancaster.ac.uk/sociology/stslaw2.html

Leopold, A., 1949, *Sand Counry Almanac* (New York: Scribners).

Lewis, W. and others, 1995, *Wetlands: Characteristics and Boundaries* (Washington DC: National Academy Press).

Maguire, D. J., 1991, An overview and definition of GIS, In *Geographical Information Systems: Overview, Principles and Applications*, **1**, edited by D. J. Maguire, M. F. Goodchild and D. W. Rhind (Essex: Longmans) , pp. 9-20.

Nyerges, T. L., 1989, Schema integration analysis for the development of GIS databases. *International Journal of Geographical Information Systems*, **3**(2), 153-183.

Porter, T. M., 1995, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton NJ: Princeton University Press).

Rachel, J. and Woolgar, S., 1995, The discursive structure of the socio-technical divide: the example of information systems development. *The Sociological Review*, **43**, 251-273.

Shapiro, C., 1995, Coordination and integration of wetland data for status and trends and inventory estimates: Progress Report. *Techncial Report 2* (Reston VA: Federal Geographic Data Committee, Wetlands Subcommittee).

http://www.usgs.gov/wetlands/
Star, S. L. and Greisemer, J. R., 1989, Institutional Ecology, "Translations," and boundary objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology. *Social Studies of Science,* **19**, 387-420.
Sullivan, J. G., Chrisman, N. R. and Niemann, B. J., Jr., 1985, Wastelands versus Wetlands in Westport Township, Wisconsin. In *Proceedings URISA* **1**, pp. 73-85.
Swarth, C., 1998, Wetlands: controversy and confusion. *Volunteer Monitor (Environmental Protection Agency)* **10**(1), 6-7.
Tolman, J., 1995, Achieving no net loss. *National Wetlands Newsletter* **17**(3), 5-7.
Tomlinson, R. F., 1967, An introduction to the Geographic Information System of the Canada Land Inventory. (Ottawa: Department of Forestry and Rural Development).
Wetlands Subcommittee, 1995, Response to comments received related to the proposed establishment of the Cowardin Classification System as the Wetlands Standard for FGDC. Federal Geographic Data Committee.
Wilen, B. O., 1995, The folly of the numbers game. *National Wetlands Newsletter* **17**(3), 8-10.