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Contents

Theory into practice: managing change through genre

by Diane Evans (diane_evans@merck.com)

(This article is based on a paper submitted to fulfill the requirements of the English 6410 course at Utah State University in December 2004.)

In his article, *Writing in An Emerging Organization*, Stephen Doheny-Farina (1986) asked "How do writing processes shape the organizational structure of an emerging organization?" He followed the writing of a business plan and the effect of this plan on the social and business dynamics within an organization that was hoping to establish a foothold in the marketplace. Other researchers have studied writing in mature organizations. For example, in *Writing Power: Communication in an Engineering Center*, Dorothy A. Winsor (2003) examined the power of writing in a for-profit engineering center. She examined the way that various employees used texts as "capital" within the organization, thereby enhancing their own standing in the company and the standing of their department as a whole.

Project quality-improvement systems such as the Capability Maturity Model and Total Quality Management often prescribe documentation paths that should be followed for project success. These techniques can be taught to students (Werth 1994) to prepare them for future employment in mature organizations, but often have limited acceptance among emerging organizations.

How does an organization become mature? Just as a child passes through a period of adolescence before achieving adulthood, an organization must go through a maturation process to pass from emergence to maturity. For the child, the teenage years can be very traumatic—their body seems to change daily, new responsibilities are placed upon them, and they must prepare for the independence that lies just a few years ahead. An organization will pass through a similar process, with organization charts, duties, and responsibilities changing frequently as management experiments with the best way to prepare for the future.

"Just as a child passes through a period of adolescence before achieving adulthood, an organization must go through a maturation process to pass from emergence to maturity."

This article describes a biotech center that has entered this adolescent stage, somewhere between emergence and maturity. As departments, employees, and development activities changed frequently, a project emerged as one way to control this change through the creation of a new genre and facilitating the acceptance of this genre throughout the organization. Genre is often thought of a type of art—such as oil painting or writing mystery novels. Documents having a particular form, content, or technique can be another type of genre. If the purpose of the genre is carefully embedded within a template, the resulting document can lead users through activities that will help to control changes.

The birth of Northwest Biotech

In 1996, a small group of scientists discovered a method for squirting RNA onto a glass slide using an Epson printer head, then analyzing the patterns of gene expression that could be detected in the resulting "printout". They quickly realized the value of this process to medical research, and formed a small gene-expression company, Northwest Biotech, in Seattle. (Northwest Biotech and Mega Drugs are pseudonyms.)

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Editorial: Examining our assumptions: lies, damned lies, and statistics

by Geoff Hart
(ghart@videotron.ca)

Every so often, it's worth our while to take a large step back from what we're doing and ask a few important questions. To me, the most difficult one to answer requires an unusual form of introspection. Ask yourself the following question: To someone watching us "from the outside", without any stake in what we're doing, what assumptions do we appear to be making? Then there are important and equally challenging follow-up questions:

- What do those assumptions say about us?
- How do those assumptions constrain or enrich our thinking?

If I tell you that these questions originate in the field of cultural studies, you might be tempted to stop reading right now, but stick with me a little longer. Among other things, cultural studies research is based on the assumption that our thoughts and actions are strongly shaped by the context (the culture) in which we create, interpret, and build upon those thoughts. Academics with deep roots in cultural studies and related fields such as social construction are frequently mocked by science communicators because some of them—oblivious to the irony of their position—appear to consider themselves somehow above the constraints of social construction and thus free to remain ignorant of their own assumptions when they critique our work. Most have a more balanced view, and if you want to find out more about why I believe we have much to learn from them, check out my recent article on the subject (Hart 2007). In the context of this editorial, the important point is that we science folk also subscribe to certain constraining assumptions that are, perhaps, equally worthy of mockery and re-examination.

Modern science represents the triumph of mathematics and abstract reasoning over straightforward observation. Whereas the naturalists of Darwin's day could still achieve fame and shape the course of their field solely by observing nature and reporting what they saw, the modern scientist will have a hard time presenting their data in a peer-reviewed journal, at a conference, or in any other respectable forum unless they can support their observations with a body of hard data that is both large and robust, supplemented by rigorous statistical testing of the probability of each experimental outcome being real rather than merely the result of unfortunate coincidence. On the whole, this change has been a good thing. Among other things, it has replaced *I think* with *I have reason to think*, and has forced us to develop new ways to test whether what we *think* we have found is what we *really* found.

It's worth interrupting with a brief historical footnote: even in Darwin's time, observation alone was not sufficient. Researchers such as Gregor Mendel, famed as the father of genetics, relied heavily on large quantities of accumulated data on which to base their hypotheses and theo-

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What is *gene expression*? Every cell in your body contains the same DNA. Within each cell, some genes are expressed, or turned on, within that cell, thus making the cell behave in a certain way. The science of gene expression focuses on discovering which genes are turned on (or *expressed*) within a cell, and how to turn these genes on and off. If you turn off the growth genes in a tumor, you could potentially cure cancer.

Northwest Biotech created software for quickly analyzing gene expression data, and soon became well-known within the biotech community. Five years after the founding of Northwest Biotech, the company was purchased outright by Mega Drugs, a century-old pharmaceutical company. The employees found themselves stuck between two worlds: the start-up personality of the biotech company, and the maturity of Mega Drugs.

As a GxP-compliant company (http://en.wikipedia.org/wiki/GxP), Mega Drugs has many procedures in place to help it comply with Good Manufacturing Practice, Good Laboratory Practice, and Good Clinical Practice regulations. Northwest, as a research and development company, had never felt a need to comply with these regulations. Since the study of gene expression is a relatively new science, even the Food and Drug Administration has no guidelines for those involved in studying gene expression.

Northwest Biotech employs four distinct types of developers: scientists, software developers, automation developers, and equipment operators. Each of these groups conducts research and development in a manner unique to their needs.

Development at Northwest Biotech

At the time of the buyout, Northwest had recently begun developing a Laboratory Information Management System (LIMS) to track experiments as they passed through the lab. Because the company's Reverse Transcription/In Vitro Transcription (RT/IVT) process had already been scientifically established, the development of LIMS involved creating software and adding automation (robots) to the lab. Each form of development proceeded independently for each group, with a few weeks at the end of the 2-year development process reserved for integrating the systems.

Upon release of the LIMS system into production, the software development team became available for other projects. One of the first projects assigned to team members was to develop a new LIMS for a new scientific process, that of extracting RNA from tissues. The previous development method—working independently, then integrating components at the end—began to fail because the science was being developed concurrently with the software. Soon the groups realized that a new procedure should be established to improve communication between the development groups.

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Managing change

Faber (2002) posits that any change has five aspects: identity, communication, narratives and images, discordance, and reconstitution. Suppose you want to change the type of car you drive. You would follow these five steps:

1. Identity: Name the change required. For example, "I need a new car."
2. Communication: Tell other stakeholders, such as your spouse and a car salesman, about your need.
3. Narratives and images: Explain why you need a new car. "My old car needs work; I want a car with better gas mileage; my neighbor just bought a snazzy new car." Visualize your new car. See yourself driving it, putting fuel in it, impressing your friends.
4. Discordance: Suddenly, you have conflicting thoughts. "If I buy a new car, I will miss the comfortable feeling that my old car gives me. I will have car payments again, perhaps higher than before. I'm not sure that I can get all of the accessories I want."
5. Reconstitution: Once you have the new car, you start reconciling all of those discordant thoughts. "I am starting to get comfortable again. The new car payments aren't that bad. I got all of the accessories I needed."

Northwest Biotech has an identity (as a "Center for Excellence in Gene Expression"), with the

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associated narratives and images that make it both independent of and part of Mega Drugs. What Northwest lacked, though, was communication between stakeholders. Many small groups within the company became so focused on their priorities that there was little coordination with other, seemingly unrelated, groups. When the groups did communicate, the language they used varied between groups. For example, one group used "Form 1" to start a project, while another didn't file "Form 1" until they were sure the project would succeed. Development had become a period of discordance, which requires a method for reconstitution before the company can become a mature organization. This discordance resulted from differences in narratives between the research and software development groups.

> "Many small groups within the company became so focused on their priorities that there was little coordination with other, seemingly unrelated, groups. When the groups did communicate, the language they used varied between groups."

Rude (2004) compared science writing and business writing: "[D]ifferent types of problems require different approaches to solutions. The scientific problem differs from the problem of deciding about future action, and even though both analyses result in reports, the two situations require variations of the [report] genre." Is this true? Is there no way to reconcile scientific discovery with business development?

Northwest Biotech includes four development groups: science, software, equipment, and automation. The software group is perhaps unique within the software industry, with a technical communicator (me) embedded within the development group. This communicator, well-experienced in the software development life cycle, works side-by-side with the software developers, ensuring that software projects are properly documented when gathering requirements, as well as during development and testing. Although the science group is experienced in documentation of the science, they do not understand software development. The equipment and automation groups do not contain technical communicators, although the software communicator will sometimes assist these groups.

An analysis of each group within Northwest Biotech showed four divergent processes for development. Scientific development was performed using a waterfall method—four distinct development periods, each documented and presented for upper management approval before beginning the next development period. Software development was performed using an iterative method which included standard software development lifecycle documentation—with upper management approvals only required at the requirements phase and before deployment to production. Equipment did not have a formal development method, and only required short Installation Qualification, Operational Qualification, and Performance Qualification (IQ/OQ/PQ) phases before deployment. Automation involved both equipment and software development, but had no formal method in place; as a result, documentation generally consisted of large flowcharts and diagrams that could only be printed on a plotter and that were understood by few outside of the automation group.

As the need for a combined development process became apparent, a project team was formed to devise a development process that all groups could use. The project team included members from each of the four development groups, with the technical communicator as the project leader. Each team member was given the authority to speak for the others in their group. The deliverables for the project would be a unified development process, a policy defining who would use the process for specific types of projects, and any needed forms and templates to support the process.

Rude (2004) states that "Various report genres guide different types of inquiry and reflect in their structures some assumptions about method and evidence. Genre analysis should begin with the context in which the genre functions rather than with form." As our discussions progressed, the project team agreed with Rude's approach, concentrating on the need for various genres instead of exactly how the forms and templates would look.

As I mentioned previously, a genre can be a document having a particular form, content, and technique. Our group needed to decide the types of documents that should be required, the content of each document type, who would be

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responsible for creating the document, and what type of approval would be needed for each. The technical communicator then chose a "look and feel" for the final templates that would be used by the employees.

The two best-defined development methods—scientific and software—each referred to a large number of documents that served as checkpoints during development, but the documents seldom shared names or templates. Some documents appeared redundant; other documents that were expected (e.g., feasibility studies and safety assessments) lacked any formal definition. The team agreed to follow the path of "just enough documentation", weeding out redundant deliverables while including those required by Mega Drugs policy.

"The project team agreed with this point of view as well—that a well-thought-out process and related documentation would become a power within the development process."

Winsor (2003) concludes, "As a genre, documentation is one of the resources that may be deployed to create relations of power and hierarchy." The project team agreed with this point of view as well—that a well-thought-out process and related documentation would become a power within the development process. If the definition of the development process was clear, becoming a checklist of actions to perform through a project, the employees would be more likely to follow the prescribed process.

Two of the best results from this project were the project charter and "Form 56". Previous to this process, it was seldom clear when a project began, or who was responsible for a project. Many "stealth" projects existed because a single person would begin working on a new idea, without thinking about the consequences of the project in the greater scheme of things. Sometimes what seemed like a small project would turn out to be huge; some projects were endangered because the number one priority for a group of scientists was not necessarily the number one priority for the software or automation groups.

The project charter became a new mandatory document for the beginning of any project. This document encouraged a person beginning a project to think of all of the consequences: who should be involved in the project, how risky the project would be, and what resources would be needed. Although the form is not especially easy to fill out (the average time to complete the form is 2 weeks of gathering information), it allowed management to plan more efficiently. All projects became visible to all groups, allowing better scheduling for employees. Each employee would know what their responsibilities were, and how their actions could affect other groups.

Form 56 never had a name other than the official number given to it. Form 56 assembled a list of all forms and tasks required to complete a project from the project charter to system retirement. The form included instructions for defining the size and scope of a project, listed the required tasks and documents based on the size and scope, and defined who was responsible for seeing that the task or document was completed. It became a simple checklist that a project manager could hang on a wall and refer to as the project progressed.

Our first project that was completed under this new process could have quickly become a nightmare in the old system: an 18-month effort involving new software, hardware, robots, and science. The project charter took several weeks to complete, but allowed strategic planning of the work. The software portion of the project was completed a week ahead of time, and the entire project was completed on time and is considered a success that should be emulated by other laboratories.

Just as a parent gently leads a teenager through life, we discovered that change can be managed through genre by thoughtfully creating templates, forms, and procedures that gently guide employees through the development process. All groups now speak the same "development language", and communications between groups has become clearer. **Ω**

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"Several misconceptions work against technology's public image. Theoretical science, so the myth goes, may be an imaginative activity, but engineering is strictly analytical—like accounting, it isn't supposed to be creative. Technology is just applied science, a byproduct of whatever happens in the laboratory; or it is inspired tinkering done by visionary (and often solitary) geniuses. And in any case, technology is completely subservient to the rules of economics: the cheapest or most efficient ideas will inevitably win out. These are the kinds of assumptions that drive historians of technology nuts."—Alex Soojung-Kim Pang, *Iron, Coal, Burgers, and Beer*

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"I regard the social sciences as continuous with the natural sciences. The light passes through the telecope on to the eye, and understanding the perception requires understanding the astronomer, not only astronomy. That study is much harder, but it is growing too."—Philip and Phylis Morrison, *The Ring of Truth*

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"Research in this country is going down. Prior to World War II, the United States was rather poor in research; that's why radar was invented in England and Germany. We learned the value of resarch in World War II. But today the quickest way to save your botttom line is to cut off research. In the automibile industry, for example, the average CEO's tenure is just 4.7 years, so the money you spend on research won't help while you are CEO. That's why there is great pressure to do something that will sell now, but on a national basis, this kind of ethic is very dangerous."—Amar Bose, founder of the Bose company, known primarily for its acoustic products, describing why his company spent 24 years developing electromagnetic suspensions for cars

Book review: Spring Into Technical Writing for Engineers and Scientists

Rosenberg, B.J. 2005. Addison-Wesley, Upper Saddle River, NJ. [ISBN 0-13-149863-0. 318 p., including index. US$29.99 (softcover).]
Previously published in Technical Communication *53(1):96–97. February 2006.*

by Jackie Damrau (jdamrau3@airmail.net)

Spring Into Technical Writing For Engineers and Scientists is written with an interesting, enlivening flair using technical humor. Barry Rosenberg uses a simple approach in delivering his advice about writing technical or scientific material correctly when writing is not your main job. His style, like that in the *For Dummies* books, makes this book an easy read on a complex subject.

In the book's four sections and twenty chapters, you'll read about (1) planning what you are going to write and for whom; (2) general writing principles, including words, sentences, paragraphs, lists, and professional secrets; (3) writing specific documents, such as manuals, Web sites, proposals, lab reports, presentations, and e-mail; and (4) document editing and production (here, you get a great beginner's explanation on fonts and typography). Rosenberg leaves a breadcrumb trail footer as a navigational aid for knowing the chapter and section you are in. For example, in the "Sentence transitions" section of Chapter 6 ("Paragraph and sections"), the footer is: "Paragraphs and Sections > Sentence Transitions."

Rosenberg says that writing requires you to capture the audience's attention when imparting information. Successful technical communication relies on the "success and failure in the technical world... to communicate effectively" (p. 7). He compares technical writing to engineering and science by showing similarities and differences. The similarities are that successful projects require careful planning, testing, and iteration; the "principle of parsimony" is paramount, and achieving parsimony (aiming to arrive at the simplest possible solution) is difficult. The difficulties are that writing cannot be successfully reduced to formulas and that writing and science require different thought processes. Looking at this from an

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engineer's or scientist's view, they seek objectivity, whereas non-scientific technical communicators seek subjectivity.

Spring Into Technical Writing for Engineers and Scientists has an excellent discussion on graphics. Rosenberg covers the typical chart types, callouts, orientation, color blindness, photography versus line art, and layout strategies. His coverage of layout strategies provide useful information, such as placing graphics strategically to attract the reader's attention, keeping the reader's eyes focused on the page, and using white space to break up the amount of technical information that you impart.

Formulaic writing is prevalent in technical documentation within the scientific and mathematical professions. Engineers and scientists tend to write algebraic-type formulas using the following six-step algorithm:

1. State the rule or principle formally.
2. Supply the relevant mathematical formula.
3. State the rule or principle casually.
4. Provide an example that shows the rule in action.
5. Describe special cases or limitations with the formula.
6. Provide a succession of examples that use the formula.

Rosenberg says that when writing for a lay audience, engineers and scientists should "focus on steps 3, 4, and possibly 5."

Spring Into Technical Writing for Engineers and Scientists is worth the reading if nothing more than for the humorous examples and the brevity with which Rosenberg has taken a complex area and provided excellent guidelines for non-technical communicators. Ω

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Book review: Picturing Machines 1400–1700

Lefèvre, W. 2004. Cambridge, MA: MIT Press. [ISBN 0-262-12269-3. 347 p., including index. softcover, US$40.00.]
Previously published in Technical Communication *53(4):483–484. November 2006.*

by Deborah Andrews (dandrews@english.udel.edu)

"How did drawing shape the practice and the notions of early modern engineers?" (p. 1). This is the central question addressed in this collection of nine well-illustrated and engaging essays. The essays look closely at the context and consequences of technical drawings as tools of communication. The editor also provides an excellent introduction to the collection as a whole and to each of its five parts.

By "technical drawings", the authors refer to drawings that "were either traced (or commissioned to be traced) and used by technicians in the pursuit of their professional life or derived from such practitioners' drawings" (p. 13). Though earlier studies have looked at the esthetics of these drawings or have found in them evidence for the development of technology and science, this collection takes a different approach in examining the social and rhetorical role of drawings. It concentrates on machine drawings and a few architectural drawings. That approach makes this a wonderful resource for scholars and practitioners in technical communication.

In his introduction, Lefèvre traces five factors that led to the emergence of technical drawings at the end of the Middle Ages and their development during the Renaissance, first in the construction of cathedrals (high technology in their time) and then in the devices of warfare, as well as in ship building, mining, and milling. In brief, one factor was the shift from the flat organization of a craft to the hierarchical division of the labor in new production sectors (such as the building of cathedrals) that required "complex structures of cooperation, responsibilities, and command" (p. 3). Second, these new production sectors also

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required new forms of "knowledge propagation" and, third, new forms of learning and instruction beyond the "learning by doing" approach of apprenticeships and travel. Technical schools developed whose major curricula centered on the skills of producing and reading technical drawings. Fourth, the "broad range of competence" necessitated by new technologies led to a new "social figure", the engineer, who designed, planned, and coordinated the realization of "ambitious projects" through the labors of several different crafts. Finally, technical drawings helped establish technology as a "matter of public interest" (p. 4).

All of these factors conspired in ensuring the role of technical drawings as a preeminent means of communication, and *Picturing Machines* neatly addresses their social functions, most of which are familiar to us today. Drawings have served to propose new ventures, to instruct, to confirm decisions, to argue, to control, and to plan. Emphasizing the flexibility of the drawing medium, Lefèvre and the other essayists suggest how architects and engineers of the early modern period invented a new graphic language to fit the new technology.

They reinvented and further enhanced ancient projection techniques as they also developed ways of showing machines on a flat surface—"flat models on paper" through "an arsenal of artificial views" (cutaway, exploded, and the like) (p. 6–7). Drawings served as a means to shape ideas and to test alternative arrangements of machine parts. They were easily changed and allowed designers to focus on items of interest while ignoring the rest of the machine. They were essential in fostering engineering design, which is the essence of innovation.

The nine essays in the collection are divided into five parts, each with a brief introductory essay by the editor. The arrangement is both thematic and historical. The first part, "Why pictures of machines?", contains a single chapter by Marcus Popplow that surveys and categorizes drawings of this period. The second part, "Pictorial languages and social characters", contains two chapters (one by David McGee and one by Rainer Leng) that analyze whether or how the style of machine renderings reflects their social functions, with a focus on 15th-century Italy and Germany. Part Three, "Seeing and knowing", also contains two chapters (one by Pamela O. Long and one by Mary Henninger-Voss). These authors examine the knowledge assumed in or conveyed by (or unable to be conveyed by) technical drawings. In Part Four, "Producing shapes", three authors (the editor, along with Filippo Camerota and Jeanne Peiffer) detail the development of drawing techniques based on geometry or geometric optics. Finally, Part Five, "Practice meets theory", provides a single chapter (by Michael S. Mahoney) that examines how engineering drawings relate to theoretical mechanics.

Each essay is grounded in extensive research into primary manuscripts, some well known (for example, the drawings of the Sienese engineer Taccola, Leonardo da Vinci, Francesco di Giorgio Martini, Albrecht Dürer) and others less well known, including the workshop drawings of anonymous gun craftsmen. Authors discuss a full range of types and styles of drawings as well as the many audiences served by those drawings, from technical colleagues to the royal courts. In each chapter, the rhetoric of these drawings is emphasized, a perspective that makes this collection particularly appealing. The essays are detailed and highly readable, with an abundance of black-and-white illustrations that bring home the particular points being made. Anyone interested in the history of technology, in the visual presentation of complex information, in the profession of engineering and of the development of engineering design—in effect, anyone reading *Technical Communication*—should find the arguments and stories and pictures in this anthology engaging. **Ω**

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ries. The difference between Mendel's time and our own lies in the development of a science of statistics. Not only do we collect *data*, but we also collect data *about* that data. The power of statistics is that it tells us not just what we think, but also how strongly we have reason to believe what we're thinking. Statistics, according to the prevailing modern dogma, makes knowledge and our confidence about the reality of that knowledge objective.

Or so we tend to assume. In fact, it would be more accurate to state that statistics makes us *more* objective, or perhaps *less subjective*. For example, most statistical tests are considered to produce significant (i.e., real) results at a probability level of 5%; that is, statisticians have made the arbitrary and entirely subjective decision that if an experimental result could have occurred by chance only 5% of the time, we can be reasonably confident that the result is real. But why not 10% or 1%? Indeed, 5% means half the risk of a random result implied by a significance level of 10%, but compared with a 1% probability level, represents five times the risk of not having found something real. Even the concept of relying on a percentage scale is a subjective and unfounded assumption: surely a scale of 1 to 1000 would be even better because of the higher level of precision it affords? Clearly, the importance of statistics is not, as some seem to think, that it provides certainty. Rather, its importance lies in its ability to provide a consistent, standardized, mathematically rigorous measure of our uncertainty.

> "... the importance of statistics is not, as some seem to think, that it provides certainty. Rather, its importance lies in its ability to provide a consistent, standardized, mathematically rigorous measure of our uncertainty."

There's also a cliché that science is an objective process and that scientists are more objective than non-scientists. Though this cliché is broadly true, it's also a myth that we have created about our profession. Consider even the limited example of statistics, and you'll see both the assumption and how over-reliance on that assumption can lead us astray. The assumption is that because the tools of mathematics allow for no subjectivity, statistics eliminate subjectivity: a number is a number, independent of the observer, and the same set of data will produce the same statistical results no matter who analyzes the data. This is true as far as it goes, and it's particularly true for the clean data generated by a rigorously designed experiment.

The problem is that most data are not clean. Results that are perfect or "too good to believe" are a common red flag that suggests not everything is kosher; for example, a graph with a series of data points that fall almost exactly along a mathematical equation is unlikely to represent real data outside of the relatively predictable worlds of physics and chemistry. Outside the lab, human error, random variations in the environment in which data are collected, errors inherent in the measurement devices and the process by which we use them, and factors unaccounted for in designing an equation (such as neglecting to include certain key parameters) introduce variation into our measurements and predictions. The better the researcher and the equipment, the better our knowledge of the physical processes, and the more carefully controlled the experiment, the smaller this variation will be, but particularly in early experiments—those first tentative explorations of a particular dark area that is as yet unilluminated by knowledge—some variation is inevitable.

Indeed, in most graphs of experimental results, you'll actually see a cloud of data, concentrated to a greater or lesser degree around a trend line that nominally describes (in objective mathematical form) the process that was observed. Equally often, you'll see an occasional data point that lies far outside the main cloud of data points. These points are called *outliers*, though sometimes *outright errors* might be a more honest description.

In messy sciences such as biology, where seemingly infinite factors in the environment of the phenomenon being studied can vary, thereby influencing the results, researchers tend to ignore outliers on the assumption that they represent random errors, usually because some uncontrolled aspect of the environment exerted a stronger than usual influence on the system being observed. Because that influence is unusual (witness its absence in the cloud of points lying nearer to the trend line), they assume that it can be safely ignored. Often this is a reasonable assumption, and the correct one, but it's no

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less an assumption despite its reasonableness, and because it's an assumption, it bears further examination.

Careful scientists recognize this, and most concede that before one can ignore an outlier, it's necessary to at least attempt to provide a reasonable explanation. After all, those factors that are unaccounted for in the equation bear watching because they can sometimes surprise us. Physics, in marked contrast with biology, is considered a clean science because it's so much easier to control the external variables than it is in messy biology. Because of this degree of control, physicists know that the part of the universe they're observing usually functions with the smooth precision of a clock, and that any deviations from that smooth function have a reason—and possibly an important one. In physics, researchers are more likely to recognize that outliers cannot simply be ignored, nor can they simply be "explained away". On the contrary, the explanation itself becomes an important hypothesis that must be tested in the hope that the results will reveal the cause of the discrepancy. Those results can sometimes lead to important findings that a less rigorous thinker might miss. For example, astrophysicists long believed that they had nailed down the basic workings of galaxies, until a few troubling "outliers" led them to identify the fact that galaxies are accelerating away from each other faster than originally predicted (http://en.wikipedia.org/wiki/Cosmic_acceleration), and that the vast majority of the mass in the universe has not yet been accounted for (http://en.wikipedia.org/wiki/Dark_matter).

Please note that I'm not suggesting any inherent superiority of physics over biology. As a former biologist, I clearly understand that the two fields of research operate under very different constraints, and that those constraints lead to very different working assumptions. Indeed, the physicist's belief in a clockwork universe is also an assumption—though by the evidence collected thus far, it's a darned good one. I'm using this comparison solely to help focus on the differences in the working assumptions and what those assumptions mean for the advancement of knowledge.

How does all this relate to scientific communication? For one thing, working with scientists all the time may lead us to make the assumption that it is sufficient for us to honor the conventions of science—hard data, logic, and an attempt to eliminate subjectivity from our conclusions and any decisions we make based on those conclusions. *It's not*. Except in the limiting case in which we are writing for scientists, this assumption leads us astray because we forget how different our real audience may be from our scientist colleagues. Arguments based solely on numbers and rigorous logic have great power, but cannot by themselves persuade audiences who don't share our assumptions about that power. Without understanding the emotional and cultural and historical contexts of our non-scientist audiences, we occasionally risk a disastrous communication failure.

> "Arguments based solely on numbers and rigorous logic have great power, but cannot by themselves persuade audiences who don't share our assumptions about that power."

A recent example makes this clear. When it was announced that scientists at the Relativistic Heavy Ion Collider (RHIC) of the Brookhaven National Laboratory expected that some of their experiments would create microscopic black holes (www.bnl.gov/rhic/black_holes.htm), they recognized that this would raise some concerns. If, as the popular stereotype suggests, black holes will devour everything in their vicinity, wouldn't those black holes then devour the accelerator, the scientists, the lab, and shortly thereafter, the entire planet and everyone on it? "No," the scientists blithely replied. "There's only a very small chance of that happening."

I'll pause here for a moment while you digest that "reassuring" statement.

What the scientists meant, of course, was that the laws of physics make it inevitable that such small black holes will "evaporate" long before they attain enough mass to become self-sustaining. Thus, based on their confidence that they understand the laws of physics at that scale, they felt that there was little fear that any other process could intervene and lead to an insoluble and quite fatal problem for our world. Of course, as I've noted above, there are always outliers in

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the data, and in physics, those outliers represent opportunities to improve our understanding of the universe by revealing new phenomena. Being good physicists, they felt it was important to explicitly acknowledge this possibility and the fact that there was a small chance they were wrong and that something... *interesting*... might happen. The consequences of that something being unthinkable to anyone but a physicist overjoyed at the prospect of new horizons of discovery appears to have escaped them.

As scientific communicators, it's exactly this kind of problem we must be aware of and must explicitly confront. In any effort to communicate, we must remain very aware of our assumptions and the possibility that those assumptions may lead us astray—possibly not to the extent that our world is devoured by a black hole, but occasionally to the extent that the audience reaction will prove to be an unpleasant surprise, as the RHIC physicists discovered. Ω

References

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"Do you see why I am worried? I myself have spent some time in the past trying to show 'the lack of scientific certainty' inherent in the construction of facts. I too made it a 'primary issue'. But I did not try to fool the public by obscuring the certainty of a closed argument—or did I? I'd like to believe that, on the contrary, I intended to emancipate the public from prematurely naturalized objectified facts. But was I mistaken? Have things changed so fast?"
—Bruno Latour, *Why has criticism run out of steam?*

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"It's bad enough that so many people believe things without any evidence. What is worse is that some people have no conception of evidence and regard facts as just someone else's opinion."—Thomas Sowell

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"In ancient times they had no statistics so they had to fall back on lies."—Stephen Leacock

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"Determination of truth from nature is deucedly difficult and encumbered with mensurational, statistical, and interpretive errors."—George W. Thomson, *The Brocken Specter, Acceptable Compromise, and Other Illusions*

Discussion groups

Scientific Communication community

STC and our community run an e-mail discussion group that provides a quiet, friendly place to turn for help if you've got any questions concerning scientific communication. To join, point your Web browser to:

http://lists.stc.org/cgi-bin/lyris.pl?enter=stcscsig-L

There's no cost to join, and you can expect a very low volume of mail. Of course, the more people who join, the more traffic there'll be, so please join. It's a great way to make the community work for you.

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