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Who Killed WATERS? Mess, Method, and Forensic Explanation in the Making and Unmaking of Large-scale Science Networks

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Abstract

Science studies has long been concerned with the theoretical and methodological challenge of mess—the inevitable tendency of technoscientific objects and practices to spill beyond the neat analytic categories we (or their actors) construct for them. Nowhere is this challenge greater than in the messy world of large-scale collaborative science projects, particularly though not exclusively in their start-up phases. This article examines the complicated life and death of the WATERS Network, an ambitious and ultimately abandoned effort at collaborative infrastructure development among hydrologists, engineers, and social scientists studying water. We argue in particular against the "forensic imagination," a particular style of accounting for failure in the messy world of large-scale network development, and against two common conceptual and empirical pitfalls that it gives

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rise to: defaults to formalism and defaults to the future. We argue that alternative postforensic approaches to "failures" like the WATERS Network can support forms of learning and accountability better attuned to the complexities of practice and policy in the real world of scientific collaboration and network formation.

Keywords
academic disciplines and traditions, epistemology, methodologies, methods

In recent decades, science funders in the United States, Europe, and elsewhere have directed growing attention to the promotion of new forms of collaboration and new computational infrastructures across a wide range of scientific fields—a transformational mission often referred to as "cyberinfrastructure" in the United States, "e-Science," or "e-Research" through much of Europe. But as we detail, such efforts follow multiple, uncertain, and often contradictory paths that lead, as often as not, to "failure." Such was the case with the WATERS Network, a high-profile initiative linking hydrologists, environmental engineers, and social scientists with shared needs and interests in the study of national freshwater processes. Existing (in some form) since 2001, by 2010 the WATERS project was (in some form) dead. Like all such public deaths, the demise of WATERS attracted its fair share of explanation and intrigue: WATERS lacked science questions. WATERS lacked leadership. WATERS couldn't integrate the different worlds and needs of engineering, hydrology, and social science. WATERS was a victim of interdirectorate politics and personnel change at the National Science Foundation (NSF). WATERS couldn't figure out its relationship to water data efforts at other federal agencies. WATERS was a victim of Bush-era indifference to science and environmental policy. WATERS was a victim of the recession. WATERS was a victim of its funding category. WATERS was too many things to too many people. WATERS isn't dead at all, but lives on in successor programs and collaborations. WATERS never quite managed to exist.

This article explores this complicated and contradictory explanatory terrain, with a wider agenda in mind. Most immediately, we tell the WATERS stories, drawing upon more than two years of ethnographic fieldwork conducted before and after the seeming end of the WATERS initiative. Our work brought us into contact with many of the key players involved: project leaders and participants from across the hydrology, environmental engineering, and social science communities, NSF program officers and senior officials, external review committees and congressional staffers, true believers and skeptics, champions and the disaffected. In the "live" days of the project, we studied the forms of work and ordering that make large-scale infrastructure development in the sciences possible. Later, we studied the processes of collaborative demise, as looked-for certainties and agreements fail, infrastructure falls away, and crafted plans come apart.

This is not, however, a postmortem examination dedicated to the usual goals of forensic analysis: assignment of guilt and responsibility, the retrospective construction of accountability, and, in the policy sphere at least, the reproduction of historical experience in forms that can guide future choice and action. Such efforts and the analytic reductions they give rise to are valuable and necessary forms of analytic work, and provide a compelling rationale for going back into the histories of failed projects like WATERS—all the more so as the US NSF and other funders continue to invest in other large-scale collaborative science projects with similar field-transformative intent (Edwards et. al. 2013). Under the conditions of contemporary policy making and the sensitivities of collaborative failure, they are also systematically underproduced (Jackson, Steinhardt, and Buyuktur 2013; Jackson et al. 2007).

Approached too narrowly, however, forensic forms of analysis and the reductions and assumptions they embed may limit rather than advance the sense we can make of experiences like the WATERS initiative. Some of these effects are endemic to the nature of analysis itself, part of the inevitable trade-off between real-world complexity and reduction that all efforts at explanation encounter: every analytic act involves reduction, and no map captures the richness and complexity of the territory described. Others stem from the emergent but uncertain nature of new collaborative forms and infrastructures in the sciences, most especially in their moments of formation, where the shape, identity, and existence of projects may be precisely what's at stake. Without the convenience of skin and a pulse, the real and effective boundaries of the project turn out to be hard to assign, even for those intimate actors at the center of the WATERS initiative. Without authoritative markers of origins and endpoints, knowing when exactly the WATERS story begins and ends proves conceptually and empirically suspect (and can always be gain-said: "the real WATERS story goes back to 19xx"). And without a single and defining will at the center of the project, the question of who speaks for WATERS—and can therefore answer questions like what the project sought to achieve and how it is to be assessed and remembered—becomes muddled. These are fundamentally ontological questions, and go to the heart of what WATERS is, was, or might have been in the world. And they turn out to be precisely the sorts of questions that forensic analyses struggle to deal with. Like a butterfly on a pin, we reduce and immobilize such projects in order to understand them: and in so doing lose precisely the qualities of motion, ambiguity and flux that most defined and characterized their existence in live form.

This article considers the limits of forensics in the complex, distributed, and shifting worlds of large-scale collaborative network formation in the sciences. Our goal is not singular explanation of the ultimate or proximal causes that lead initiatives like WATERS to live or die, succeed or fail, but a better understanding of the complex and messy conditions of possibility and impossibility that go into the making and unmaking of such collaborative forms and infrastructures. "Postforensic" analyses of WATERS and like phenomena must find ways of dealing with this range and sprawl. They should show us some glimpse of the world as the actors encounter it—messy and unformed, recalcitrant and rich with possibility. They should let us share in the alternately mundane and exciting, hopeful and painful experience of imagining and building new collaborative networks in the sciences. And they should let us struggle with our actors to make sense of such outcomes, without pretending to a knowledge and clarity—the always tempting "view from nowhere" (Haraway 1990)—that leaves us above the explanatory fray.

Mess and Method: Blind Spots and Defaults

Leading examples of forensic analysis—and its limits when applied to complex, distributed and emergent phenomena of the sort studied here—have been called out in past STS and allied scholarship. Charles Perrow's ([1984] 1999) classic analysis of the Three Mile Island nuclear disaster points to distinctive features of complex systems that challenge both prediction and post hoc reconstruction of failure. In "tightly coupled" systems marked by deep and multiple dependencies, small breakdowns can spread quickly and unpredictably, ramifying across the web of interdependencies to produce a kind of "interactive complexity" in which "routine sins have very nonroutine consequences" (p. 10). Under such circumstances, even farsighted efforts to plan for contingency may set off chains of consequence that undermine the stability of overall systems. Moves to build security at the local level may *increase* global risk, transferring risk to the system level where breakdowns may prove both more catastrophic and harder to deal with or foresee.

The same features mean that failure, when it happens, can rarely be easily isolated, attributed, or assigned, despite the exhaustive efforts of public inquiries, hearings, and other forensic exercises to the contrary. These often reduce complex and interactive breakdowns to stories of single-point failure: discrete and mundane equipment failures; breakdowns or pathologies of organizational process or routine; and the widely cited but poorly formulated notion of "operator error." Against these forensic tendencies, Perrow (1999) argues that failure in systems like nuclear power is in effect ecological in nature—the product of multiple and interacting weaknesses that collectively (rather than individually) tip the balance of risk.

The same sensibility informs Diane Vaughan's (1997) exhaustive reconstruction of the Challenger space shuttle disaster of 1986. As Vaughan documents, media reports and the Presidential Commission charged with examining the explosion quickly converged on an explanation: the now infamous "O-rings" of the shuttle's Solid Rocket Booster subassembly, and lapses in organizational judgment and review that led NASA middle managers to disregard warnings and known problems with the O-rings under intense organizational pressures to achieve a successful launch. But as Vaughan's own account reveals, actions and decisions that showed up under the forensic eye as examples of error and bad judgment were often in fact framed by circumstances that made those actions reasonable in context. Proximal causes like failed O-rings and suspect managerial decision making could also be traced to deeper and wider roots, which at the margins approached a description of the entire world under consideration. At the end of the day, explanations for phenomena like the Challenger disaster (or more recently the Deep Water Horizon oil spill) must go beyond simple forensic fixation on proximal culprits like O-rings and blowout preventers to understand the wider ecology of choice and circumstance in which such outcomes are embedded. The Challenger disaster ultimately lives, argues Vaughan (1997, xiv), as "a mistake embedded in the banality of organizational life and facilitated by an environment of scarcity and competition, an unprecedented, uncertain technology, incrementalism, patterns of information, routinization, organizational and inter-organizational structures, and a complex culture."

Broadly, parallel instincts have driven science studies' enduring interest in the complexities hidden behind "failure," and its sometimes creative strategies for dealing with them. Our own work draws insight from two important sources: Bruno Latour's account of Aramis (1996), a failed French rail development project of the 70s and 80s; and a series of methodological reflections offered by John Law (2004) in "After Method: Mess in Social Science Research." Each of these takes issue with the sort of simple forensic narratives questioned here. In Aramis, Latour goes to extraordinary lengths to demonstrate and recapture the multiplicity of Aramis, as conveyed through an incongruent mix of actor interviews, project documents, and fictionalized exchanges between the book's engineer-turned-ethnographer narrator and his sage research supervisor, with cameos from Aramis itself. The picture that emerges is not a composite, with contradictory moments and elements reconciled in a higher and ultimately unified explanation (the classic forensic urge). Rather, it is fractured and multiple to the core, beyond all hope of unification. In life as in death, Latour insists, Aramis was and remains plural: different things to different actors at different times, with no clear or obvious authority to decide between them. We are left at the end of the day with the peculiarly ontological mode of failure experienced by complex objects and projects that never proceed beyond a certain point of realization. As Latour insists, Aramis failed by never quite managing to exist.

For Law, cases like Aramis open onto a broader terrain of mess and complexity that social science research has so far struggled to engage. This is reflected in the way we practice and talk about method, predicated all too often on adherence to a narrow conception of rigor, simplified notions of cause, and a quasi-ontological commitment to a fundamentally ordered and knowable world. From this starting point, method appears as a sort of machine for cutting through confusions cast by the appearance of the world, beneath which the careful and faithful methodologist will discern a more solid and coherent reality. It is this version of method that Law seeks to contest. As he describes,

> I would like to divest concern with method of its inheritance of hygiene. I want to move from the moralist idea that if only you do your methods properly you will lead a healthy research life—the idea that you will discover specific truths about which all reasonable people can at least temporarily agree. I want to divest it of what I will call 'singularity': the idea that indeed there are definite and limited sets of processes, single sets of processes, to be discovered if only you lead a healthy research life . . . To do this we will need to unmake many of our methodological habits, including: the desire for certainty; the expectation that we can usually arrive at more or less stable conclusions about the way things really are; the belief that as social scientists we

> have special insights that allow us to see further than others into certain parts of social reality; and the expectations of generality that are wrapped up in what is often called 'universalism.' (p. 9)

In the absence of such reimagining, we are left with a social science that is "clearly wrong," marked by a reductionist fallacy through which the complex multiplicities of things in the world get filtered and sorted retrospectively. The result is a sort of rearview mirror effect, in which "objects may be more complicated than they appear." The worlds left over are neat but impoverished, exhibiting a seductive but misleading clarity that will leave us less equipped to deal when we meet such phenomena again in the world—which is to say, in their native and messy state.

In the worlds of emergent scientific collaboration we study, such forensic errors show up in two characteristic forms. The first of these, *defaults to formalism*, stems from confusion between formal instantiations of the phenomena under study (here: large-scale science networks) and the looser but crucial forms of action and attachment that they depend on. Faced with Law's messy world, a common response is to grab the first and most obvious container available: here, the formal project definition. But this runs against most of what we know, professionally and practically, about how organized social phenomena in the world work. We know as researchers that our intellectual worlds are not fully mapped within the formal institutional terms that contain them, nor are our real and effective collaborations the same, exactly, as the on-paper grants that fund them, the management plans that guide them, or the structures of formal output and authorship that follow. Collaborations, then—and perhaps especially live and good collaborations—depend on pieces that exceed or subtend their formalized selves: dimensions, side effects, and unintended consequences that lay beneath, beyond, or simply apart from any formalized or formalizable element. But this sense tends not to travel well outside the lived experience of collaboration, either in accounts to outsiders (funders demanding accountability; nosy social scientists poking around) or in the retrospective accounts of failed or completed projects. Under such circumstances, we are prone to mistake the map for the territory, official accounts for the worlds of practice they describe.

This produces some unfortunate effects. First, it will tend to reify and exaggerate the boundaries of projects in their formal guise, to the general neglect of sprawl, spill, and connection across such boundaries. This sets up something like a territorial fiction, and all the question marks and ambiguities that territoriality invokes. Who is inside and who is outside? Who is a member and who is not? Are there categories or degrees of membership (and how are we to think of these)? But in the messy world of collaborative science, participants themselves may not know whether and to which collaborations, even formal ones, they "belong" (see, e.g., Lee, Dourish, and Mark 2006; Ribes and Finholt 2007), and "interest formation" may be a central (and unfinished!) project within the constitution of new collaborative forms (Vann and Bowker 2006). At the same time, defaults to formalism will make WATERS and similar phenomena appear retrospectively as things drier and more limited than encountered by their participants: creatures of documents, meetings, and formal agreements (which they *are*, but not exclusively), rather than worlds of human care and obligation, creativity and aspiration, possibility and commitment. The same tendency will tend to overplay formally organized initiatives at the expense of the less regularized forms of interaction and exchange that provide the fine suturing and much of the value of collaborative scientific life within and between fields.

A second error of reduction common to forensic accounts like the ones questioned here can be found in *defaults to the future*. Here we subjugate past and present to the future by taking the projected or eventual shape of projects or networks as templates for the "same" projects in earlier moments or stages of formation. There are two distinct flavors of this problem. One concerns the ambiguous status of plans and planning, and the tendency to treat all past actions as proceeding from or orienting toward some expression of collective goals. This reflects, but misrecognizes, what is in fact a formative role of the future in present collaborative action: namely, the importance of "proximal futures" (Bowker 2005) as a pole around which present interests and choices are oriented. Such proximal futures can and do show up in the form of plans, including the sorts of elaborate planning exercises endemic to collective projects of this scale. But they will also be more subtle and multiple than formalized project documents will allow, not least as individuals work out their "side bets" (Becker 1960) in and around diversely imagined futures. Even in their formal or collective guise, proximal futures will have an ambiguous relationship to present action, sometimes shaping, sometimes effectively ignored by it, as actors selectively deploy, enact, or reference such futures in pursuit of present agendas (see, e.g., Suchman 1987; Vann and Bowker 2006).

A different kind of default to the future lies in a problem long familiar to critics of Whig history (Butterfield 1931): namely, the tendency to let now-known futures infect our understanding of phenomena at earlier and different moments of possibility. Knowing how the story ends may cause us to read the record of the past asymmetrically, in search of clues to eventual problems (we know they'll get divorced in act 5: can we see signs of unhappiness in act 1?). More generically, the eventual (realized) shape of projects may come to unduly influence and limit our understanding of such projects at moments when the actors themselves could not yet have known what it was they would turn out to be working toward (and may have been orienting to different visions). As we'll see in the accounts that follow, the project to be known as WATERS was, at earlier moments of formation, a very different creature, oriented to different (and multiple!) possibilities. Defaults to the future of the Whiggish variety miss this range and heterogeneity, and risk reading these early, consequential, and could-yet-be-otherwise formations as nothing more than the incipient outlines of the now known future—proto-WATERS all along.

In the sections that follow, we draw on more than two years' worth of interviews, document analysis, and participant observation to construct a series of accounts that tell us something about what WATERS was (and failed to become), and the messy worlds of large-scale network construction it lived within. For much of the period of our fieldwork, the future of WATERS hung in the balance. Soon after we came to engage the project in 2008, a crucial planning document was given grave reviews by a National Research Council (NRC) review panel, and the project reorganized and changed leadership. Over the ensuing twenty-four months, we conducted twenty-six semistructured interviews with leaders and members of the WATERS team, NSF officials, and policymakers in the House, Senate, and White House with involvement in large-scale science funding decisions. We also observed and participated in ongoing network planning meetings, workshops on the management of large-scale science networks in general, the presentation of the final WATERS Science Plan at the National Academy of Sciences, and a post hoc re-scoping workshop among project leaders after the decision to discontinue funding had been announced. Finally, we reviewed the extensive documentary trail that the WATERS planning effort left behind: documents, reports, technical descriptions, external evaluations, minutes, meeting notes, newsletters, and press releases from various stages of the WATERS development effort. In the following sections, we offer a thumbnail sketch of this history; point in a more concrete way to some of the conceptual and empirical difficulties that forensics can lead us into; and provide some brief clues toward what a postforensic approach might look like.

Retelling WATERS: One Version

We begin from a beginning. In 2001, a small group of environmental engineers working on stressed environments and sponsored by the NSF's Engineering Directorate gather to sketch the outlines of a new collaborative network called CLEANER (Collaborative Large-Scale Engineering Analysis Network for Environmental Research). At around the same time and supported independently by the NSF's Geosciences Directorate, a group of academic hydrologists begin to organize under the umbrella of CUAHSI, the Consortium of Universities for the Advancement of Hydrologic Science, Inc. Over the next two years, the projects run more or less in parallel, gathering support and converging (independently) on a program of action centered on four main elements: (1) a network of observatories dedicated to the production of core and long-term datasets; (2) a set of 'cyberinfrastructure' tools and standards for the analysis, modeling, visualization, and storage of data; (3) plans for the support of synthetic and multidisciplinary research and education around water; and (4) a set of mechanisms to foster enhanced collaboration between engineers, natural and social scientists, educators, and policymakers (for CLEANER); or a new state-of-the-art measurement technology facility (for CUAHSI).

Faced with this independent convergence and the unlikely prospect of funding two costly and potentially overlapping networks, the projects' respective backers at the NSF (Engineering and Geosciences) press for consolidation, urging the two groups to join forces in pursuit of a single, integrated network dedicated to water research and problems at the national level. In December 2004, representatives of the two initiatives come together at the NSF for the first time, and over the next months and years the groups continue to work, separately and together, on the fundamentals of a joint research network. In November 2005, CLEANER and CUAHSI convene their first joint planning workshop, and a month later, at the annual meeting of the American Geophysical Union in San Francisco, announce their intention to seek funding for a new "dual-purpose" network to be named the WATer and Environmental Research Systems (WATERS) Network. The new network is to be dedicated to transforming research in both the 'natural' and 'engineered' water environments through the development of an integrated, large-scale, and multidisciplinary infrastructure for water research and education. It will seek funding from the NSF's Major Research Equipment and Facilities Construction (MREFC) program, an independent Foundation-wide account for large-scale equipment and facility construction across the sciences. For its backers, perhaps above all else, WATERS holds out the promise of scale: the possibility of moving beyond traditional small field studies to think comparatively and synthetically across problems in the 'natural' and 'engineered' water environments at national levels of concern.

In summer 2006, a joint WATERS Network Design Team begins the work of integrating the individual visions and planning documents coming out of the separate CUAHSI and CLEANER processes. Water-related social scientists (and the NSF's Social Behavioral and Economic Science (SBE) Directorate) also begin to show up as a third partner in the enterprise. In 2007, the former CLEANER Project Office is officially transformed into the WATERS Network Project Office, and a former CLEANER co-director named lead principal investigator of the now joint effort. A series of WATERS 'testbeds' are launched, with the goal of providing proof of concept and early infrastructure development for the wider network effort. Over the next several months, members of the design team convene a series of workshops, conferences, and town hall meetings to work out in conjunction with their wider scientific communities the Network's proposed science questions, infrastructure requirements, educational goals, and organizational structure. In February 2008, they deliver the results of these efforts to the NSF in the form of the WATERS Science, Education, and Design Strategy (SEDS).

Here the story takes a turn for the worse. Acting at NSF's request, an ad hoc and arms-length committee of the National Research Council reviews the SEDS document—and comes to sharply critical conclusions. It urges WATERS planners to narrow and focus the scope of the Network's proposed science questions; provide a clearer rationale for the national network strategy; and describe in much clearer detail the path to achieving the project's lofty goals. In addition, the science questions are found to overlap in confusing ways with the mission statements of existing water-related federal agencies, including the United States Geological Survey, Bureau of Reclamation, and the Army Corps of Engineers. The integration of social science also comes in for criticism: WATERS planners are urged to either make this a more central component of the wider proposal, or "pare back the focus of the proposal and not attempt to address human behavior in an all-encompassing manner."

Faced with this setback, the WATERS planning team reaches a new 1-year cooperative agreement with the NSF to continue their planning efforts, with the goal of producing a substantially revised science plan for the network. In the aftermath of the NRC review, the network reorganizes, appointing a new and more senior lead principal investigator and reorganizing the design team. In January 2009, following a series of earlier false starts, an SBE-sponsored workshop is held with the dual purpose of refining and developing the

> project's social science components, and deepening social scientific participation and engagement with the network. In June of 2009, the Network's revised Science Plan is officially presented at a meeting of the National Academies of Science in Washington, and sent out for a second round of NRC review.
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> This time the reviews are more positive, but still critical: while the Science Plan outlines a compelling vision for integrating hydrologic science, environmental engineering, and social science research in the service of pressing national water problems, and promises important advances in basic and applied water science and education, the NRC committee concludes that the argument for an integrated national network has not been made. The NRC committee also questions whether the MREFC approach, with its front-loaded design process and the need to sell the project in its entirety to congress, is the right vehicle for what WATERS seeks to achieve. The committee urges them to consider more incremental options, including the regular research accounts of the participating NSF directorates.
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> This proves to be the final nail in the coffin. In spring 2010, the NSF informs the WATERS leadership that the project is no longer under consideration for MREFC funding. The final issue of the WATERS Network Newsletter explains the gist of this decision: the NSF is no longer convinced that the MREFC route is appropriate for WATERS, echoing the NRC committee's logic that "it is probably more sensible to build the network incrementally and let the questions and experiments evolve in an adaptive framework. This approach, which is not constrained by MREFC timelines for design and construction phases, could take better advantage of advances in technology over time, such as for sensors and components of the cyberinfrastructure." The letter politely thanks the WATERS leadership for their efforts and reiterates the strengths and contributions noted by the NRC reviewers. It also points project leaders to a new (albeit much smaller) regular research solicitation released several months earlier: the new "Water Sustainability and Climate" competition overseen jointly by the Engineering, Geosciences, Biological Sciences, and SBE directorates—a possible successor program, target, or consolation prize for members of the WATERS Network? WATERS it seems, is dead. Long live WATERS?

What are we to make of the brief and neatened account above? On one hand, it has the virtues of a good story, with a clear narrative arc from early stages of development to the flurry of activity associated with the drafting of successive science plans, and from there to crisis and denouement with the NRC reviews and subsequent withdrawal of NSF funding. It even, arguably, leaves room open for a sequel, in the form of the new Water Sustainability and Climate program. And like all good stories, it has begun to circulate, passed among leaders and program officers in the world of large-scale project funding as a sort of cautionary tale around how projects can fall off the MREFC track, for any of the reasons outlined previously.

But from the standpoint of the postforensic position advocated here, these are exactly the kinds of stories that we should learn to question. For as even the most casually engaged participant in WATERS will insist, WATERS was simultaneously more and less than we've made it out to be so far: more because summative and retrospective accounts like the one above capture only a fraction of the possibilities that WATERS, at different moments and for different actors, embodied; less because, for all that promise, the actual footprint or legacy of WATERS in the world is maddeningly difficult to discern. The same ambiguity greets any effort to provide a meaningful answer to the reasonable but problematic question of *why* WATERS failed. This section provides some clues as to how a different and less reductive account of WATERS and its lessons might begin to be told.

We pick up the story with the closest thing we have to an official account of WATERS' demise: namely, the critique levied by the final NRC committee, as cited in the NSF's termination letter to project leadership. As noted previously, a central issue here had to do with the monolithic and fixed nature of WATERS in its proposed (i.e., MREFC) form, and the committee's apparent preference for a more incremental or evolutionary approach through the route of regular interdirectorate funding. From this perspective, WATERS failed because of the fixed and nonadaptive nature of its design, but also because, in the committee's view, the project team had failed to arrive at a sufficiently detailed plan and argument for network design, including its connection to the articulated science questions. A classic double bind: on one hand, WATERS faced the suspicion of being trapped and limited by its early design choices; on the other, it was criticized for providing too *little* in the way of concrete detail. Such contradictions aside, the "official" account provides at best a weak or proximal explanation of cause, for in noting these shortcomings, the NRC report and subsequent NSF letter tell us little about *why* the planning document before their eyes exhibited the features it did. For that and other explanations, we must dig deeper and go to other sources.

In our interviews with project participants, much was made of the distinctive features and limits of the MREFC as a mechanism for building large-scale science networks. For several decades, the MREFC and predecessor categories have provided the Foundation's principal means of support for large-scale infrastructure investments, traditionally in the form of big-ticket items (ships, telescopes, etc.) whose price tag would exceed or distort the annual research budgets of any single directorate. MREFC projects follow a strict and largely separate set of rules from most NSF activities, including authorization as a separate line item in the NSF budget. This makes them financially separate from directorate research budgets: an obvious attraction in times of limited or shrinking research budgets. But they are not, for all that, "free": directorates must bear the cost of the multiyear planning effort and later the ongoing operations and maintenance (O&M) costs—a substantial and sometimes overlooked price of MREFC development.

This structure, several of our informants reported, posed both process-based and categorical limits. By requiring detailed and up-front commitments to network infrastructure—in part to allocate and freeze cost overruns and Congressional criticisms associated with past NSF infrastructure projects—MREFC development ran the risk of fixing too early certain choices (major equipment, fixed infrastructure, key standards, etc.) that might be better and more flexibly managed through an evolutionary approach. Others attributed these features less to the nature of the MREFC category itself, and more to the lack of imagination on the part of NSF officials and NRC reviewers in interpreting it (a dispute we watched play out in our fieldwork). For these respondents, the MREFC was not *inevitably* hostile to an evolutionary and adaptive approach, but had been made so in the hands of officials too bound to the "ships and telescopes" model of traditional infrastructure development or too fearful of pushback at higher levels of NSF or Congressional review. Still other explanations centered on the moral hazard associated with the implicit back-loading of MREFC costs, and what happened when operation and maintenance costs passed back to the regular research directorates following the initial period of "free" construction. This question had fresh and painful precedent in earlier MREFC projects then moving into operation, whose newly absorbed O&M costs had severely depressed new award spending in their particular part of the Foundation; the Geosciences Directorate in particular was reported as being sensitive to this point and leery of creating "too many mouths to feed" in its downstream commitments.

A radically different class of explanations for WATERS' failure centered on the coordination challenges posed by the range of disciplinary groups engaged in the project—initially hydrology and environmental engineering, subsequently social science—and the somewhat forced and artificial nature of their combination under WATERS. In the most generic version of this complaint, it was argued that hydrologists and engineers (and later social scientists) simply saw the world too differently to deliver on the unified network sought by the NSF. The "problem-solving" approach of engineers was contrasted with the "curiosity-driven" nature of hydrology (with varying evaluative codings applied to each side). Others pointed to the mismatched organizational histories behind the CLEANER–CUAHSI union, noting CUAHSI's structure as a stand-alone university consortium that pre- (and indeed post-) dated WATERS versus CLEANER's lack of prior or separate institutional existence. Some respondents suggested that the engineering and hydrology and later social science communities simply didn't know each other well enough to pull off the kind of joint effort the NSF was demanding; others argued by contrast that the two sides knew each other only too well and were burdened by a fraught and complex history of relations.[1]

According to several informants, these and other disciplinary ghosts came to the fore when, in the wake of the "shotgun marriage" between CUAHSI and CLEANER, it came time to work out the details of the design and science plan for the now-joint network. This produced two characteristic effects: on one hand, strong sectional or partisan interests that shaped and sometimes disrupted the planning efforts; on the other, well-intentioned (but no less problematic) efforts to respect and accommodate different disciplinary interests through a sort of "Christmas tree" strategy that insisted the new network have something for everybody.

Other explanations for WATERS' difficulties centered on the shifting composition and nature of NSF management and advocacy around the project. Through the life of the broader WATERS effort (and even through the more contracted period of formal network planning), the NSF, with its distinctive rotational structure, went through numerous changes in personnel, including—perhaps especially—at its higher levels. The initial decision to unite CUAHSI and CLEANER in pursuit of an MREFC project was, by all accounts, made high up in the NSF hierarchy (and reportedly contradicted the earlier position of individuals in the Geosciences Directorate, who had until that time actively *discouraged* CUAHSI from pursuing an MREFC application). With changes in leadership at these levels—all three of the directorates in question went through at least one change in leadership between 2005 and 2009, though per MREFC requirements, the corps of Program Officers assigned to the project remained somewhat more consistent—enthusiasm and support for the MREFC undertaking seemed to wax and wane (and did so differentially among the participating directorates). Such transitions challenged both institutional memory and the consistency of message coming from the NSF, and made it difficult to attract and retain the sort of high-level champions that flagship projects like WATERS would need to make it through NSF and wider congressional review.

Still other explanations for WATERS' problems pointed to a looser set of circumstances and contingencies that, taken separately or in conjunction with other factors noted previously, limited and undermined the broader development effort. One of these concerns the notoriously tricky problem of leadership, and the question of whether WATERS ever (or through key moments of its development) had the personnel in place to pull off what was indeed a daunting and perhaps impossible task: engaging and mobilizing the often partisan interests of three loose and putatively defined "communities," establishing working relationships with existing mission agencies, and satisfying the (shifting) interests and advice of multiple NSF constituents, all the while navigating the demands of a strict but ambiguously defined planning process. Others chalked the WATERS difficulties up to what amounted to unfortunate accidents of timing. This included word that the NSF and/or Congress were rethinking the MREFC, and were perhaps in the process of doing away with it for good—a point which may have tempered enthusiasms within and beyond the NSF. Others pointed to the unflattering comparison posed by National Ecological Observatory Network (NEON), an ultimately successful MREFC initiative that was moving through the approval process one step ahead of WATERS (though more careful analysis of the NEON case reveals similar "near-death" experiences (Jackson, Steinhardt, and Buyuktur 2013)). At the same time, through the core period of WATERS' planning, the federal government had gone from surplus to deficit and to an administration arguably unfriendly to the basic goals and priorities of environmental observatory networks like WATERS. These and other accounts paint WATERS as an unfortunate and unintended victim of circumstance, which under friendlier conditions and moments, might well have come to pass.

Explanation after Forensics, Meaning without Hygiene

What are we to make of this explanatory landscape? At this juncture, a number of points are in order. The first is that the round of causal explanation gathered previously is already an impossibly neatened and reduced version of what we encountered in the field. In practice, most of the points conveyed as singular explanations previously gather multiple explanatory flavors and strands, many of which cross and diverge when followed to their more local or microscopic levels. Indeed, the matrix of accounts that these stories are based on runs to over one hundred pages, and is itself already a distillation of the "raw" ethnographic field materials. Any effective *application* of postforensic thinking (as opposed to a call and argument for it, as offered here) would need to engage this level of detail through strategies and to an extent impossible under present page constraints.

Our second point concerns the interactive and occasionally contradictory nature of the accounts shared here, both separately and together. Offered as a set, it is easy to read the above as a kind of causal summary or checklist, a simple recitation of the three, four, ten, or twenty ways in which WATERS went wrong. But such an approach underestimates the way in which these explanations fit together, in complementary and contradictory fashions. If we hold that WATERS' difficulties stemmed from the category politics of the MREFC, can we still invoke disciplinary divergence as a cause of demise? If we argue for the short-term institutional memory at the NSF, is it still useful or reasonable to point to the (inherent?) challenges of the MREFC mechanism? What about the counterevidence of other projects that went through successfully under more or less the same timeline and conditions that WATERS faced and faltered on? And what about the counterarguments excised from the account above that point to why each and every one of the explanations offered above is *wrong*? Such effects complicate and frustrate the forensic dream of the clean causal chain, the stepwise and irrefutable movement from action to effect that is the hallmark of all good television crime dramas, and many post hoc efforts to produce accountability and learning in the policy world. Efforts to restore or create such clarity through the kinds of "hygiene" imposed by forensics have important work to do in the world. But they do so at the cost of violence and distortion to the phenomena themselves that may ultimately limit rather than advance the broader analytic purposes they are meant to serve.

Finally and most significantly, the singular causal narratives gathered previously found on a simple central fact: namely, that the object at the center of these discussions is not stable or fixed, but is itself a moving target, changing shape, vision, constitution, and identity throughout the period in question. Under such circumstances, efforts at forensic explanation must always begin by fixing and defining the unity of WATERS itself (though this work is rarely acknowledged). But this contradicts our experience of the object and those of our actors: whether measured by time, location, or participant perspective, WATERS was and remains a *variable* object, different things to different people (or places, or times). The whole goal of the WATERS development process is to bring these variations into alignment, *but the work is never completed*, save for its highly partial representation in artifacts like the Science Plan, a pale and constrained reflection of the larger whole. Under such conditions, the forensic imagination finally and definitively hit its limits. For it is precisely the sprawl of WATERS, its uncertainty, its could-have-been-otherwise (and at certain moments, could-yet-be-otherwise) qualities that are routinely missed in the work of singular explanation at the heart of forensic analysis. To converge on cause we converge on identity, and in so doing short circuit or fix the meaning of our objects, whose identity is precisely in question.

These blind spots speak to weaknesses inherent to forensics in general, especially though not exclusively in the messy, heterogeneous, and underformed worlds of large-scale network formation we study. They also go to the very heart of the relationship between the conceptual and the empirical that this volume is meant to address. As we have argued, the conceptual stance of forensics assumes or prefigures a certain kind of clarity and order in the world, and is thus empirically disposed to objects and artifacts that seem to reflect, replicate, or embody that clarity—yet another instance of the way in which forms of knowledge and forms of order are coproduced in the world (Jasanoff 2004). It is therefore too easily impressed by the appearance or representations of order—plans, formal structures, and other "official" accounts—and too prone to overlook elements or aspects of the empirical world that violate or complicate those expectations. Under such circumstances, discrepancies and contradictions are likely to be resolved in the direction of the more official or formalized accounts, regardless of their real and effective force in the world. This makes the forensic imagination a sucker for defaults to formalism of the sort critiqued above—and likely to miss or misconstrue the alternative practices and visions that constitute inevitable and perhaps necessary parts of complex distributed phenomena like the WATERS Network.

At the same time, as the discussion of clean causal chains implies, forensics imagine a stepwise world that orients ultimately toward the coherence of known outcomes—witness the classic arc of the television crime shows. This makes forensics vulnerable to the sort of "default to the future" criticized here. Death, too, when read backward can impose a certain kind of clarity. Knowing how the story ends can shape and limit how we conceptualize earlier moments of possibility—the projects WATERS never became, the paths not fully taken—even where these possibilities remained live and real to their contemporary actors. Here again, the forensic imagination assumes privileges and burdens of knowledge that blind us to the churn and diversity of the empirical worlds we study.

How else then to do this work? Pointing to the limits of forensic thinking does not, in itself, pose an alternative, except in the still useful negative sense of identifying how such pitfalls and blind spots may limit our ability to understand and learn from experiences of "success" and "failure" in the large collaborative project space. We recognize and respect therefore the basic interest behind the forensic urge: we too want to help build better and more effective policy and infrastructure in the sciences, and think better and more careful analyses around projects like WATERS can help with that. Such efforts at learning are notably underdeveloped under the conditions of contemporary policy making; the greatest single danger coming out of the WATERS experience may be not that it is misunderstood, but that it sinks without a trace, producing little insight that can help guide understanding around the complexities of action and possibility in this space. For this reason, our interest in mess is not merely to point it out (still less celebrate it) but rather to build from this starting point to tell different stories around matters of analytic concern, including ones oriented to different kinds of analytic purposes. How can we do this, but in a postforensic way? Can we do more with mess than gesture at it? Are there ways of achieving meaning without hygiene?

A first and obvious strategy in dealing with the mess and complexity of an object like WATERS is to attempt to recreate in the retelling something of the multiplicity and uncertainty encountered in the phenomenon itself. This is partly a simple matter of patience and space: resisting our forensic urges long enough to unfold the story in ways more consistent with how it has been encountered in the world (a point admittedly out of step with the beautiful clarity of the one-hour television crime shows, and sometimes with the publish-or-perish mandates of the academic world). More fundamentally, such efforts run up against deep-seated constraints and assumptions of narrative, many of which resist the kind of multiplicity and sustained uncertainty called for here. It is this fact that accounts for the experiments in narrative form that characterize many science studies efforts to meet complexity on its native ground: the complicated vignettes, transcripts, multiple narrations, field materials, and stories-within-stories of Latour's *Aramis* (1996), the double narrative of Ann-Marie Mol's *The Body Multiple* (2002), and so on. For many of the same reasons, other scholars have abandoned text altogether as a sole or primary medium, and sought to approach their objects of study through post or extratextual forms: for example, Latour and Hernant's "Paris: Ville Invisible";[2] or Goldberg and Hristova's "Blue Velvet: Re-dressing New Orleans in Katrina's Wake."[3]

A second postforensic strategy lies in the artful mining of discrepancy and departure—the sites, moments, and actors who diverge from main lines of development (and usually therefore from the story, in standard narrative and analytic approaches). This borrows but stretches science studies' longstanding reliance on controversy and breakdown as methodological opportunity, and the related methodological injunction to "follow the actors." In the particular case, we study, and we suspect many others, the shape and texture of the project is defined at least as much by discrepancies and variations which never rise to the level of controversy, all operating beneath the level of and only weakly reflected in planning or other formal process. Controversy draws on these energies and movements, but rarely fully captures or exhausts them. At the same time, replaying other debates within science studies (Star 1991), the actors worth following prove at the margin difficult to define, and strategies that borrow the definitions of central actors (e.g., omnipresent but elusive references to "the WATERS community" [Ribes and Finholt 2008] may lead us straight back into the errors of formalism and futurism described previously). Our own research benefited greatly from interactions with actors with no apparent connection to WATERS (unaffiliated hydrologists, policy actors unconnected to the WATERS initiative, etc.). Principled attention to such discrepant and outside actors may help avoid the methodological pitfalls and limits of forensics, and constitutes a central piece of the postforensic stance advocated here.

A third strategy centers on the distinctive temporalities of phenomena like WATERS and better means for analytically respecting and accommodating these. At an immediately practical level, this confronts the necessary shift in actor perception and accounts as complex distributed phenomena like WATERS go through successive stages of possibility and reduction. As we know from Ethnography 101, actors will change and reinterpret their stories in light of present circumstance, and are remarkably skilled at producing narrative meaning and coherence out of the jumble of worldly events. But the same actors also leave stories and traces that *can't* be retrofitted to reality in this way: statements and claims, traces and documents that are already out in the world circulating and therefore beyond the capacity of actors to retract or amend. Fragments of the paths not taken, such traces constitute raw material for the postforensic effort recommended here (and are indeed central to the historicized form of institutional ethnography practiced by Vaughan). In our case, the WATERS planning effort generated a large body of such traces—in the form of public statements, presentations, meeting notes, documents, and so on,—that could be selectively mined to produce different imaginings of the project at different points and places in its history. We also joined the project before the turn, and were therefore able to watch as WATERS flipped from moments of expanding imagination to moments of fixity, foreclosure and demise, shedding possibility like skin. Such points and flows provide portals through which radically different versions of the object may be accessed. Any serious effort to avoid the pitfalls of forensic explanation must recognize and seek out such entry points.

But these strategies rest in turn on a certain care and skill in our ways of putting theory and world together—or in the language of this volume, a more artful accommodation between the conceptual and the empirical. As the editors and contributors to this special issue suggest, science studies have much to offer on both sides of this divide. It can contribute powerfully to better empirical understanding of worlds of technoscience that are incorrectly or incompletely described in other scholarly traditions, including the worlds of large-scale science network development referenced here. It can also contribute new conceptual frameworks that open familiar objects to new and promising questions. Where maximally effective, these efforts go hand in hand. If we are to avoid the point-and-click sociology that has sometimes characterized the uptake of key science studies concepts (Latour 2007; Star 2010), our theoretical work should build honestly from the difference and recalcitrance that the empirical reliably (if sometimes frustratingly!) provides. And if we are to continue to bring fresh and relevant insight to matters of common concern, our empirical investigations should proceed with the kind of care and creativity that conceptual foundations, suitably engaged, can provide.

Conclusion

In spring of 2010, the WATERS story came to its apparent end. The NRC delivered its final report, and the NSF announced its intention to discontinue funding of the planning efforts. The WATERS test beds were wound down, spun off or continued under other funding, and remaining funds were spent out (including through a workshop of key participants and new players thinking about lessons and future possibilities in this space). The "WATERS community," such as it was, drained away. WATERS was effectively dead.

But as we have argued, the clarity of this outcome can be exaggerated. To begin in a classically forensic vein, it remains unclear whether the cited reasons (those given in the NRC review and noted in the final letter to the project leaders) were the effective ones, or whether there were other and harder realities at work here (including some offered by the participants, as noted previously). In the bureaucratically structured and highly stylized world of science policy making, we find at play certain "repertoires of account" (Tilly 2006; Boltanski and Thevenot 2006), formal and informal norms and rules governing the way we account for and justify action—for example, that it's all about the science; that all disciplines are created equal; or that intellectual merit trumps all. It's also possible, pace Aristotle, that the causality is layered in ways that make the proximate reasons for WATERS' demise not its final ones.

These are real and important questions, not least for the ongoing project of learning from past experience to make better sense of what's working—and not—in contemporary efforts to imagine, fund, and build new forms of collaborative infrastructure in the sciences. But in the end, they are not the only or necessarily most helpful ones. If our goal is to understand phenomena like WATERS in their live form, we must push beyond such forensic forms and analysis. We must look for conceptual and empirical strategies that produce insight (even order!) from mess, but do so without recourse to reductions and simplifications that harm at least some of the insights and possibilities that STS analyses can bring to the world. We must become comfortable with multiplicity, and engage the world of speed, churn, and uncertainty that WATERS and other such collaborative projects will inhabit. Under such circumstances, postforensic efforts to gather and share the experiences of phenomena such as the WATERS network become doubly important: as a more faithful and naturalistic record of development in large-scale science projects that can aid future and parallel efforts, and as a methodological test or puzzle for science studies scholars committed to avoiding the old reductionist traps.

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Notes

1. Through much of the Foundation's history, hydrologic research was funded by the National Science Foundations (NSF's) Engineering Directorate. In 1984, the Hydrology and Hydraulics Program was shut down and proposals redirected to

the more general Environmental Engineering Program. In 1991, academic hydrologists responded by publishing their now-famous "blue book" report, arguing for the needs of hydrology as an independent branch of science separate from and beyond its engineering "applications." One year later, the NSF's Geosciences Directorate instituted a core program in Hydrologic Sciences, formalizing the split.

2. http://www.bruno-latour.fr/virtual/index.html.
3. http://vectors.usc.edu/projects/index.php?project=82.

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