

### 3 Analogy, Cases, and Comparative Social Organization

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**A** GRADUATE STUDENT ONCE SAID TO ME, “WE LEARN THEORIES, but we don’t learn how people theorize.” Theorizing is the process of building an explanation: theory is the product. We read the polished final products, but the process of theorizing remains private, the undiscussed backstage of our craft. Perhaps this is so because we are not aware of our own process, or because theorizing is tacit knowledge, hard to articulate. How do we have theoretical insights? What are the sources of ideas and the cognitive mechanisms behind our interpretive work?

In the 1980s, as I started my career, I became aware that my own process of theorizing was analogical: Against tradition, I was creating explanations by drawing in theories and concepts developed to explain similar events or activities in social settings different from mine, both in substance and in level of analysis, because they nonetheless fit my data. Wondering if this were a common way of theorizing, I began exploring the possibility. Research in cognitive science showed that analogy was the primary way that children and adults explained the world to themselves. Analogical reasoning is the cognitive process of structural alignment and mapping between separate, distinctive domains and their parts (Gentner 1983). It is a complex process that we experience in a flash of recognition.

That flash of recognition of similarities across different domains matched my experience. Logically, if analogy was a common way of explaining the world to oneself, it followed that other scholars would use analogical

reasoning in developing theoretical explanations. I began to search for uses of analogy in sociology. While other disciplines had acknowledged its role in the production of knowledge, sociology had all but ignored it. A few organization theorists had written about the role of correspondences in theorizing; however, they concentrated on metaphor, not analogy (Manning 1979; Morgan 1980). The exception was Stinchcombe (1978), who advocated a method of case comparison for historical sociologists that called for systematically searching for both analogies and differences across *similar* cases (i.e., social revolutions in nation-states). Writing before Gentner, Stinchcombe described historical case comparison as a search for structural equivalence relations. When a great number of structurally equivalent pairs were found, it constituted a deep analogy. The relative silence about analogy in sociology was in contrast to evidence of its use in some classic works. Although analogy was unnamed and no method discussed, the authors were generating theory by comparing similar activities across different social settings: cross-case rather than same-case comparison.

Goffman was a master at it. In *Asylums* (1961), his concept of the total institution was inductively developed from his fieldwork in a mental hospital, then further elaborated by cross-case comparison: of prisons, army training camps, naval vessels, boarding schools, and monasteries. The resulting theory was that the most important factor in forming the member was the institution, not individual characteristics, so the reaction and adjustment of the individuals to one total institution was analogous to those confined in the others. In *Power and Exchange in Social Life* (1964), Blau began with an analysis of love in a dyad using interactional data to identify basic principles of power and exchange. Then in succeeding chapters he elaborated his theory by considering first groups, then complex organizations, then inter-organizational relations.

These examples explicitly demonstrated analogical theorizing based on cross-case comparison, where the cases were different, stand-alone forms of social organization with different members. In *Street Corner Society* (1943), however, Whyte had innovated. His famous insight was that the informal rankings of cornerboys from the Norton Settlement House were analogous to their formal ranking on the bowling team, based on scores. His theoretical explanation was that the opportunity to change street rank through athletic skill was precluded by group dynamics, which limited social mobility, creating structural equivalence between member ranking in the informal

organization of the corner and the formal organization of the team. Writing long before Stinchcombe, using ethnographic rather than historical data, Whyte had identified a deep analogy and the dynamic that created it.

I thought that if we could harness our use of analogy, making the heretofore invisible and automatic visible and explicit by developing it into a systematic method, it would have the potential for innovative theory generation. Moreover—and in response to the graduate student's complaint—it could be taught. I began experimenting with such a method, using cross-case rather than same-case comparison. Since I began this project in the 1980s, sociologists have become more aware of the importance of the theorizing process (Weick 1995; Becker 1998; Burawoy 1998; Hedström and Swedberg 1998; Swedberg 2012b). Moreover, analogy as a mechanism for theory building has received some support. Tsoukas (1993) recognized the potential of analogy, proposing Gentner's structure-mapping theory (1989) for building organization theory. More recently, Abbott identified analogy as a key heuristic for the social sciences, giving examples of concepts and theories that have been imported across disciplinary boundaries, traveling from one research topic to another, resulting in innovative theorizing (2004: 110–20).

Because it is seldom discussed, we know very little about how people theorize. Analogical theorizing is one way, among several others (see, e.g., Van Maanen 1995; Becker 1998; Abbott 2004; Swedberg 2012; Timmermans and Tavory 2012). Undoubtedly, others are undiscovered because people are not sensitized to their own theorizing process. My purpose in this chapter is to create an awareness of the uses of analogy in social research and to introduce the method of analogical theorizing. Analogy is relevant to this project in two ways: as a case comparison method and as a way of thinking analytically. I begin by briefly describing the method. Then, to demonstrate, I retrace how I used the method to develop a general theory from a three-case comparison, explaining how and why things go wrong, such that organizational actions and outcomes deviate from social expectations: *Controlling Unlawful Organizational Behavior* (1983), *Uncoupling* (1986), and *The Challenger Launch Decision* (1996). Despite variation in organization size, complexity, and function, each case demonstrated similar causal patterns, which combined with the differences to drive theorizing forward.

Throughout, I focus on the cognitive backstage of theory development: my iterative process and the choices I made, showing both the missteps that led to revision and the positive advances that produced new concepts and theoretical

innovation. The comparison shows analogy operating at every stage of the research process: case selection, developing concepts and theoretical explanations; the material practice of comparison; and generalizing beyond the case to other examples. Although my examples are substantively specific, the method itself is theoretically neutral, so can be used for other substantive topics and by scholars with theoretical orientations and research methods that differ from mine. In the conclusion, I address the general uses and relevance of analogical theorizing for sociology and its potential for developing generic explanations, theoretical innovation, and theoretical integration.

### Analogy and the Heuristics of Case Analysis

Analogical theorizing depends upon cross-case rather than same-case comparison to explain some similar event, outcome, or activity. Supporting the legitimacy of this approach is the formal sociology of Georg Simmel (1950), who argued that the central task of sociology is to separate the social form of some phenomenon from its content in order to identify generic patterns across cases. So, for example, if comparing domestic violence, gang violence, and war between nation-states, we would be able to identify the general properties of violence found across social settings. The approach embraces differentially organized social forms, from the simpler and less structured, as in the earliest organizational studies of the Chicago School (Abbott 2009), to modern formal and complex organizations. Their comparability originates in a fundamental principle of group life. Regardless of differences in size, complexity, and function, all organizational forms have characteristics in common. They share basic aspects of structure: hierarchy, division of labor, goals, normative standards, patterns of coming and going. Further, they share common processes: socialization, conflict, competition, cooperation, power, culture. This means we can compare them, generating theory based on analogies and differences that we find (Vaughan 1992).

The focal point of explanation—our case—will be an event, activity, or outcome situated in an organizational form or forms: either a group, formal organization, complex organization, or some combination—substantively, a family, gang, neighborhood, community, profession, university, nation-state. The cases can be ethnographies, interview- or document-based, or historical studies, all producing lots of detailed information as a rich base for theorizing. The first use of analogical comparison comes in at the point of case

selection. We always begin research with a starting theory, concept, or definition in mind that shapes our selection of a case. So from the beginning, we are making a comparison between our case and what we expect to find, based on a theory, concept, or characteristics of other cases. More than one case can be analyzed in the same study, or cases can be analyzed sequentially. Alternatively, a focal case can be compared with a case or cases done at a different time and place by other researchers, these differences qualifying it as a cross-case rather than same-case comparison (see, e.g., Burawoy 1979).

Case selection is a key heuristic in the discovery process. A case is selected because of the event or activity to be explained. However, intentionally shifting the unit of analysis from one organizational form to another is essential. Doing so can contribute to theory building in several ways: (1) when studying similar events in different organizational settings, we get different kinds of data that reveal previously unrecognized aspects of the problem; (2) often shifting the unit of analysis shifts the level of analysis as well, allowing new insights into the micro-elements of a macro-level explanation, or vice-versa; (3) it can be advantageous for elaborating theories and concepts focusing on large, complex systems that are difficult to study; and (4) it promotes integration of the research and theory of different scholars studying events, activities, and outcomes in variously organized social forms and at different levels of analysis, thus building a stronger conceptual base and moving us toward general theory that narrows the scope conditions in which a particular theory will apply.

Our first goal is to determine empirically what a given case is an example of. The starting theory or concept is used as a heuristic to loosely organize the data. A complex starting theory may be reduced to a set of its key concepts, organized as a skeletal analytic framework to aid discovery. The case selected is itself a hypothesis. A circumstance or event appears to be an example of  $x$  based on certain characteristics of  $x$  that we know and recognize (or suspect) are there, either on the basis of research of our own, others' research, or personal experience. The idea is to proceed in a systematic way that forces us to discover and confront what we do not expect to find. To further this goal, the analysis is framed as situated action (Vaughan 1996, 1998). Following sociological theory indicating that social interaction occurs in layered social structures and processes, we situate individual interaction within the organizational form in which it occurs and also within the organizational environment: relevant political, economic, cultural, institutional, and historical

conditions. Drawing boundaries is a creative aspect of the theorizing process that can limit or enhance the similarities and differences we find. By framing our analysis as situated action, we are forced to consider the relation between environment, organization, and individual choice, meaning, and action. Most often, empirical work focuses on one, or possibly two of these levels of analysis, not all three. With a situated action approach, we are forced to look in new directions, beyond our personal theoretical orientation toward micro-, meso-, or macro-, even though in a given case we may find we do not have data for all three.

We treat each case independently of others. Although the analogies—with the starting theory, with other cases—confirm what we suspect, the differences are crucial because they lead to theoretical innovation. The differences sensitize us to our mistakes. They can disqualify the case as an example of *x*, for it may turn out that we do not have an example of what we thought (see, e.g., Vaughan 2004). Whatever the result, identifying the defining patterns of each case is a necessary first step. The process of comparison is important to developing an explanation. Intuitively developed and practiced before I knew of the writings of C. S. Peirce, analogical theorizing conforms to Peirce's process of abduction (cf. Vaughan 1992). In a 1903 lecture and subsequent writings on how to theorize, Peirce saw theorizing as three interrelated yet independent and distinct operations: abduction, deduction, and induction (Swedberg 2013). He described abduction as a series of mental processes: a continuing iteration and adjustment between alternative hypotheses, theory, and data to either refine, correct, or expand a theory in new directions, narrow its scope, or define it as inappropriate. Abduction depends upon weighing anomalous findings against existing theories to construct new theories (Tavory and Timmermans 2009; Timmermans and Tavory 2012).

These several processes of comparison are essential to discovering both patterns across cases and the novel and unexpected. The discovery process is aided by the systematic use of research strategies that help us guard against our biases and force-fitting our data into a preconceived theory (Vaughan 192: 195–99). Analytic induction calls for expanding and correcting the explanation to take into account discrepant information that contradicts what we expect to find (Lindesmith 1947; Katz 2001). Blumer (1960) argued for “sensitizing concepts” that point us in a general direction, telling us where to look rather than dictating what we will find. In contrast to Glaser and Strauss's (1967) take on grounded theory—that we proceed inductively, always

beginning from an objective, neutral theoretical position—in order to let the theory “emerge” from the data, analogical theorizing assumes that we always have a set of theories and concepts in mind, so must make them explicit in order to reject, reconceptualize, or extend theory (Vaughan 1992). Concurring with Peirce, Timmermans and Tavory (2012) prioritize abduction over grounded theory, concluding that theory generation is not solely inductive, and must be an iterative, recursive process that takes alternative hypotheses—those theories and concepts we have “in mind”—into account.

Once the defining patterns for a case have been identified, the patterns can be treated as a heuristic device to see if the relevant features are found in other cases or theories that appear to be analogical. This works both backward and forward: once resolved, a case is weighed against the starting theory, concept, or case and also becomes a hypothesis for the next case. Critical to this method is the backward comparison: setting aside the substantive findings of the present case to clearly distinguish similarities and differences that specify the theoretical consequences of the comparison. When we do not take this final step, we discourage others from building on what we’ve done. The goal is to develop a body of cumulative knowledge across cases, based on the identification of generic patterns, incrementally narrowing the scope of phenomena to which they apply.

### Theorizing Organizational Deviance: How and Why Things Go Wrong

To demonstrate analogical theorizing as both a cognitive process and a cross-case comparative method, I retrace my incremental development of a theory of organizational deviance from a three-case comparison. Although strikingly different substantively, all three were examples of how things went wrong in organizations that varied in size, complexity, and function: corporate misconduct in one, deteriorating intimate relationships in another, and NASA’s Space Shuttle *Challenger* tragedy. Throughout, I attempt to make clear my theorizing process: the theoretical tools I brought to the project and how analogical reasoning and comparison led to new insights and concepts. Three caveats: First, I present an overview, based on the main ideas of my previous research as I moved from writing project to writing project. Each book appeared separately, in isolation from the others; here their stories are joined to show the connections between them. The resulting shortcoming is that each project

cannot be presented here in the detail of the original, so the complete logic of the process and the theory that are crucial to explanation are not visible. Second, the need to condense and put the narrative in a logical order distorts the process of theorizing, which is more a meandering process with intermittent order than the linear one presented here. Third, none of what appears here would have been possible without the work of literally hundreds of scholars. For in-depth explanations of each case, the theory, and how the work of others went into its development, interested readers should see the originals.

*Controlling Unlawful Organizational Behavior (1983)*

As a graduate student, I specialized in the sociology of organizations and in deviance and social control. For my dissertation, I wanted to merge the two in a study of organizational misconduct. This was an innovative idea at the time. Organizational pathologies were not the domain of organization specialists. Instead, the topic was restricted to specialists in deviance and social control whose focus was white-collar criminals—high-status individual offenders, acting in their official roles, violating laws and rules to achieve corporate goals (Sutherland 1949). Fortuitously, a case hit the local papers that was exactly what I had in mind: one organization had violated the law, victimizing another organization. Revco Discount Drug Store, a Medicaid provider, had used a computer-generated double-billing scheme to defraud the Ohio Department of Public Welfare of more than a half-million dollars in Medicaid funds. I analyzed the case, the social control network that developed to investigate it, and the outcomes in organizational terms. My empirical analysis was followed by three chapters that I organized into the three topics that dominated the literature: the competitive environment, from the corporate crime literature; organization characteristics, from the organizations literature; and regulation, which mainly came from the sociology of law, but with some crucial bits of organizational sociology mixed in. The three chapters were thorough, and they were original because they merged the organizations and deviance literatures. However, they stood alone as disconnected pieces of the puzzle. They described but did not explain.

It was while revising my dissertation as a book that I discovered that my own process of theorizing was analogical. While working on the chapter on “organization characteristics,” I discovered myself writing a sentence about organization characteristics as “legitimate means” to accomplish deviance. From the toolkit of concepts I carried around with me all the time, without



thinking I had extracted one concept —legitimate means—from Merton's Social Structure and Anomie theory (1968[1938]) because it resonated with my data. Merton had conceptualized legitimate means as opportunity structures at the societal level. Thinking of organizations as the units of analysis, I saw that organization characteristics could be conceptualized as opportunity structures to attain organization goals unlawfully. His concept of opportunity structures came to me because of analogies—structural equivalences—between his problem, individual deviance, and mine, organizational deviance.

Realizing this connection, I saw how the parts related to each other and to the whole: I reorganized my three descriptive chapters into three interrelated parts of a causal explanation. For this abbreviated space, I reduce the theory (i.e., the three chapters) to its core concepts and subconcepts. The major elements of this theory, how they connect with each other, and the major subconcepts that constitute each are:

1. The competitive environment (competition, scarce resources, and norms), which puts structural pressures on organizations to violate laws and rules in order to attain goals (Vaughan 1983: Chap. 4, 54–66);
2. Organization characteristics (structure, processes, transactions, and technology), which provide opportunities to violate (1983: Chap. 5, 67–87);
3. The regulatory environment (autonomy and interdependence), defined as the relationship between regulatory organizations and the organizations they regulate, which tends to compromise the capacity to control and deter violations, thereby encouraging the decisions of individual organization members to violate in the organization's behalf (1983: Chap. 6, 88–104).

Although each of these three components is related to violative behavior, I saw that they were interrelated, such that misconduct resulted from the three in combination. All three factors were necessary to a causal explanation because they *combined* to affect individual meaning making, choice, and action. The subconcepts were interrelated as well, together explaining the dynamics within each of the three core factors. Thus, I concluded, misconduct was the product of an organizational system.

Reflecting on what I had done to arrive at this new place, I realized I had switched units of analysis, using Merton's societal-level theory, which was designed to explain rates of individual deviance, and instead applying it to

organizations. Thinking in terms of organizations had exposed some weaknesses in Merton's conceptualization and at the same time had suggested how it might be altered to explain organizational misconduct. In fact, his theory seemed better at explaining the behavior of organizations than individuals. Below I describe some of the cognitive process of theorizing that led to (1) linking the three major concepts into a causal system, (2) developing the sub-concepts within each core element and their relation to one another, and (3) a critique and revision of Merton's theory.

*The Competitive Environment.* Scripted into Merton's explanation but never made explicit were the ideas of competition and resource scarcity. His societal-level theory explained the crimes of the working class by arguing that not all can achieve the culturally approved goal of economic success because the legitimate means (opportunity structures) to that success were limited (education, jobs, etc.). Thus, Merton's unrecognized dynamic was that individuals had to compete for scarce resources. When stymied by blocked access to legitimate means, many resorted to illegitimate means to attain them. However, I saw that *both* means and ends could be in short supply. Thus, I reconceptualized both as scarce resources. Further, I realized that he neglected to note that even those individuals who have the opportunities to get into the competition still must compete for scarce resources: acceptance at graduate school, promotion, meeting the profit goals for the quarter, achieving tenure, winning the NCAA basketball tournament. Obviously, deviance was not solely a working-class phenomenon. Middle- and upper-class people could also be compelled toward deviance (Vaughan 1983: 54–66, 70–73, 85–87). The cross-case comparison had generated a critique of the social-class basis of his theory.

This realization led to the next theoretical advance. Analogically, it followed that what was true for individuals was also true for organizations. Organizations compete for scarce resources, which I defined broadly to include not only financial resources, but also qualified employees, sales territory, market share, members, customers, parishioners. Those organizations at the top of the organizational stratification system compete to retain their status among equals or move up, those in the great middle compete to maintain or increase theirs, and those at the bottom strive to stay in the competition or to keep from failing altogether. This continuous dynamic of competition and scarce resources, together with the unclear norms regulating business behavior, in combination exerted structural pressures on *all* organizations, regardless of place in the stratification system, to engage in misconduct. Marginal and

failing organizations were wrongly viewed as the perpetrators of misconduct. Lacking resources, they were just more likely to be caught and punished.

*Organization Characteristics.* Adding the organizations literature led to a major break from the macro-level Mertonian-inspired part of the explanation, bringing meso- and micro-level factors into the explanation. Fleshing out the idea of opportunity structures, I saw that organization structure—the hierarchy, division of labor, geographic dispersion, specialized units—provided opportunities: (1) it created many locations where misconduct could occur; and (2) the specialization and separate locations of those divisions and subunits created an invisibility, blocking oversight and providing opportunities for individuals to violate laws and rules in the organization's behalf. I described the effect as "structural secrecy" to show how structure promoted misconduct through concealment.

But the meso-level explanation was not yet complete. The Revco case had indicated that the fraud generated by the double-billing scheme had gone on for a long time without detection. Why was it not detected? Revco carried out the fraud by submitting falsified claims entered on tape in a computerized invoice submission system. The welfare department reviewed these tapes using a computerized program that spot-checked for irregularities. Because of the clever way the falsified information was organized on the invoices, the computer spot check missed the continuing fraud. Again, my theorizing was triggered by analogical comparison. These data resonated with a theory designed to suit a different but structurally equivalent problem: economist A. Michael Spence's (1974) theory of market signaling.

Spence wanted to explain how firms make decisions when hiring from among a large pool of applicants. Given the high costs of getting to know each candidate well, firms elect a short-cut method of evaluation, treating information as indexes (characteristics of a candidate that cannot be changed, like race or sex) and signals, which vary (where a candidate earned a degree, letters of recommendation, previous employment). Because signals vary, firms rely on them as an efficient means to discriminate between candidates' qualifications. It struck me, first, that because signals vary, they could be manipulated; thus fraud was possible. Second, the high cost of thoroughly screening information means that any fraud is likely to go unnoticed. These insights led to my treating information as signals, and explained why early warning signs of the fraud were missed. Thus the concepts of "transaction systems," "technology," and "missed signals" completed the meso-level explanations.

*Regulatory Environment.* The theoretical problem remaining to be resolved was individual action: why do some organization members working in organizations that are subject to competitive pressures, and who have access to internal opportunities, act unlawfully to achieve an organization's goals while others do not? Since rewards and punishments influence choices people make, the ability of other organizations to impose costs affects the probability that opportunities for misconduct will be used. Thus the regulatory environment became the final conceptual building block of the explanatory scheme. Owing to the effects of autonomy and interdependence between regulatory and regulated organizations (Pfeffer and Salancik 1978), social control was systematically compromised, thus encouraging individual decisions to violate in an organization's behalf (1983: 88–104).

At the time of publication, I believed that my effort to theorize organizational misconduct was limited by the lack of micro-level data—in my case and in the white-collar crime literature generally. The model most frequently supported in that literature was the “amoral calculator” model, a rational choice theory. Whereas many cases, including Revco, appeared to fit, the data on organizational factors were always inadequate and other organization decision-making models, such as bounded rationality, had never been weighed. A serendipitous classroom experiment suggested how to move forward. In a lecture in my large undergraduate course on criminology, I used the theory to explain corporate crime, using the Revco case as an example. To simplify the theory, I reduced it to its major concepts and subconcepts, putting them on the chalkboard as an analytical framework to hang the major ideas on. It looked like this:

<i>Competitive Environment</i>	<i>Organization Characteristics</i>	<i>Regulatory Environment</i>
Competition	Structure	Autonomy
Scarce Resources	Processes	Interdependence
Norms	Transactions	
	Technology	

In the past, I had followed my corporate crime lecture with lectures on police misconduct and domestic violence, treating them as separate patterns of crime, with nothing in common. But that semester, as I began the police misconduct lecture, I saw that the theory qua framework fit the police data, adding meso- and micro-level insights to it. Encouraged and curious, in the following class I used the framework again to explain domestic violence. It

worked: my students saw that all three cases could be explained as organizational phenomena. And I saw the heuristic possibilities of a simplified analytic framework and cross-case comparison for fleshing out a theory at different levels of analysis. The success of this experiment led me to think about writing a book on organization misconduct using these three as cases; plus I wanted to add a fourth case of misconduct in which the offender was not a corporate profit-seeker but a large nonprofit. This was not easy to find in 1983.

While I was searching for one, I worked on a project on deteriorating intimate relationships. I did not see this situation as organizational misconduct, but I did see it as an example of organization failure. Intimate relationships are, after all, the smallest organizations we create. Even though the participants may not have defined it as a failure, and even though the participants may have reconstituted the relationship as a friendship, as an organizational form (a group) the intimate relationship no longer existed. I did not guide the research with the analytical framework above because I was not expecting the analogies that I found. Surprisingly, the process of relationship decline took my 1983 theory in important new directions.

*Uncoupling: Turning Points in Intimate Relationships (1986)*

In “The Social Construction of Marriage,” P. Berger and Kellner (1964) described how two people with separate identities come together and, in interaction, redefine themselves as a couple. This redefinition is a gradual process. Habits and routines that used to affirm the singular identities of each are reconstituted to affirm the partnership. The two people create new sets of social relations: in addition to the friends of each, they develop “our friends.” They move in together. These actions publicly define them as a couple. Breaking up, I believed, was the analogue of this theory, but in reverse: a gradual process of “uncoupling” in which the two people again redefine themselves, in their own eyes and in the eyes of others, as single and separate identities again.

My chronological accounts from married and cohabiting couples, both gay and straight, confirmed that uncoupling is a gradual transition with identifiable stages. Both people in the couple make the same transition, but it starts and ends at different times for each. The main pattern is that one person, whom I call the initiator, begins leaving the relationship socially and psychologically before the other. By the time the partner being left behind realizes the relationship is in serious trouble, the initiator is socially and psychologically

distant, making saving the relationship difficult. The puzzle to be answered was, how was it possible, in the smallest organization we create, for two people to get so far apart without one person noticing and acting to reverse the decline? I immediately saw analogies between uncoupling, Spence's market signaling theory, and the Revco case: a long incubation period, information that deviated from expectations introduced gradually, and early warning signs that were missed or ignored. However, the uncoupling case provided social-psychological data missing from both Spence's signaling theory and the Revco case.

*Situated Action.* Following Spence, I again treated information as signals. The key to the patterns in uncoupling lay in "the display of discontent": how the initiator communicated unhappiness and how the partner interpreted that information. The initiator's signals varied in strength, affecting their meaning. Some were weak signals. As the initiator's discontent grows, the signals become more frequent and stronger. But even the signals that initiators believe are strong and direct may not get the partner's attention. Why? Despite changes in the relationship, the partner does not define the relationship as a serious problem because the salience of these signals is reduced by (1) the pattern of information and (2) the social context. Each warning sign is interjected one at a time into an ongoing stream of information that the partner reads as indicators that all is well. The pattern of information renders the initiator's signals as weak, mixed, or routine.

Further, at the organization level, the partner's world view comprises taken-for-granted assumptions about what is possible in the relationship, based on the organization culture and the routines and rituals of everyday experience in the past. Those elements symbolize stability, tending to obscure change. At the macro level, the partner's interpretive work was shaped by institutionalized cultural beliefs in the larger society about the value of relationships, their socially expected duration, the priority of the group over the individual, and gender and commitment. Partners expressed cultural expectations about the quality of relationships: "All relationships have trouble. Ours wouldn't be normal if we didn't." "After a while, all couples lose their interest in sex." Within the context of layered cultural beliefs, the problems in their own relationship are normal, natural troubles, not signals of danger. As a result of this disconnect between the two people, the initiator has been in transition for some time. Only when the initiator is socially and psychologically ready to go does that person send a clear strong signal that the partner

cannot miss or deny. At the point of physical separation, the initiator is prepared; the partner is not. The partner then begins going through the stages of the transition that the initiator went through long before.

Generalizing from the case in the last chapter of the 1986 book, I used examples to indicate that the process of uncoupling was analogical to other kinds of leave-taking, suggesting a basis for a sociology of transitions. The social-psychological data elaborated the original theory of misconduct in several ways. As the Revco case allowed me to elaborate on/break with Merton's theory by bringing in meso- and micro-level factors, so uncoupling elaborated the concepts of signals and signaling, adding the social construction of meaning, nested within organizational and macro-cultural factors. The case raised the possibility of a mistake contributing to organization failure, and showed how mistakes could be systematically produced in a very small organization. I did not know it at the time, but these conceptual developments would be crucial in explaining the data from my next case—the case of misconduct by a large nonprofit organization.

*The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA (1996)*

The Space Shuttle *Challenger* exploded seconds after launch on January 28, 1986, killing the crew and Christa McAuliffe, the “Teacher in Space.” Immediately a Presidential Commission was formed to investigate the accident. The cause was quickly identified as a technical failure. However, further inquiry revealed the NASA organization had failed as well. NASA managers were warned by engineers that the weather conditions that prevailed prior to launch were risky. The engineers recommended against launch, owing to possible dangerous effects on the shuttle's solid rocket boosters—the technical component responsible for the *Challenger's* demise; but despite the warning, managers proceeded with the launch, apparently in order to keep to the launch schedule, violating rules in the process.

The case had all the markings of organizational misconduct: competition for scarce resources, cost/safety trade-offs and production pressures, and violations of rules in pursuit of organization goals: managers had failed to pass relevant information up the hierarchy as required. Moreover, the *Challenger* case was different in size, complexity, and function from the Revco and intimate relationship cases, a requirement for cross-case analysis. Drawing from my classroom experiment, I used the three

core elements and subconcepts of the analytic framework heuristically to organize the data. Initially, my reliance on press reports and volume 1 (a summary) of the five-volume Presidential Commission Report) confirmed my misconduct hypothesis. But when I was deep into the enormous body of archival data that the commission had amassed, I discovered information that contradicted many of the aspects of the case that had, for me and the public, indicated misconduct (Presidential Commission on the Space Shuttle Accident 1996: Chap. 2).

I was wrong. Crucially, for the case to be misconduct, there had to be rule violations by individuals acting in their organizational roles in behalf of NASA goals. However, I discovered that the actions of NASA managers that the commission identified as rule violations actually conformed to NASA rules. With a short time to master NASA language, the commission had misunderstood. For the case to be misconduct, there had to be rule violations, and I hadn't found any. If not misconduct, what explained it? The commission's inquiry had revealed that NASA had been flying with known flaws on the solid rocket boosters since the first shuttle flight in 1981. I started over. The research became a historical ethnography: a reconstruction of the past to see how people at another time and place had made sense of things. For each launch decision, I analyzed engineering pre-launch risk assessments, memos, testimony, and interview transcripts to understand why, after a technical anomaly was discovered on the solid rocket boosters, they continued to fly. These historical records allowed me to compare testimony after the tragedy with what engineers and managers believed when they were making decisions.

I spent years reading engineering documents, without a clear picture of what I was finding or where it was leading theoretically. The discovery I initially experienced as a setback—a negative surprise—set me on a new path, filled with additional surprises (Vaughan 2004). These surprises I interpreted as mistakes on my part: mistaken assumptions, hypotheses, and theories. First among them, I found that in every launch decision NASA personnel had conformed to NASA rules. Affirming theorizing as a meandering rather than a linear process, after six years of research I concluded that this was not a case of amoral calculating managers and misconduct, but I did not yet know what it *was* an example of. The conceptualizing was incomplete, and a full explanation was a few years away.

In the end, the starting theory's three core concepts, their subconcepts, and the links between them nonetheless explained the case. The analytic



framework and the original theory still applied, but had to be reconstituted to fit the developing explanation and new concepts. In the book, the competitive environment as structural impetus (competition, scarce resources, norms) became “The Culture of Production” (Chap. 6). Organization characteristics that provide opportunities (structure, processes, transactions, technology) are reconstituted as “The Production of Culture” (Chaps. 3–5) and “Structural Secrecy” (Chap.7). The regulatory environment (autonomy and interdependence) and its connection to decision making also appear in “Structural Secrecy” (Chap. 7). Then in combination, they explain the *Challenger* launch decision in “The Eve of the Launch Revisited” (Chaps. 8–9).

*The Production of Culture: The Normalization of Deviance.* By production of culture, I mean how, in interaction, managers and engineers produced a cultural belief in risk acceptability of the solid rocket boosters in the years preceding the launch of the *Challenger*. By “normalization of deviance,” I mean the remarkable fact that—growing concerns and objections of individual engineers in the year before *Challenger* notwithstanding—in all official risk assessments and launch recommendations, engineers analyzed evidence that the design was not performing as predicted and reinterpreted it as acceptable and non-deviant. I discovered a five-step decision sequence in which technical deviations first were identified as signals of potential danger, then, after engineering analysis, were redefined as an “acceptable risk,” a formal category at NASA. This decision sequence—*anomaly, risk acceptance, fly*—was repeated, the repetition indicating the institutionalization of a cultural belief in acceptable risk. More amazing, NASA gradually expanded the bounds of acceptable risk. The first decision to accept risk established a precedent to fly with recurring anomalies. The production of culture and the normalization of deviance explained how they gradually accepted more and more technical anomalies. Again, my question was why.

*The Culture of Production.* Competition, scarce resources, and norms played out in new ways, perpetuating the normalization of deviance and decisions to launch. Powerful elites in the White House, Congress, and NASA set the agency goals high and constrained resources, changing NASA’s R&D culture to one that operated more like a business. To meet performance expectations, NASA leaders accelerated the launch schedule and shaved costs, as if they were in the kind of bureaucratic production system that engineers normally inhabit. The norms of professional

engineering contributed to the normalization of deviance. Engineers are trained to work in technical production systems that are organized by the principles of capitalism and bureaucratic hierarchy. During professional training and then on the job, they develop a world view that includes attention to costs and efficiency, production goals, conformity to rules, and acceptance of hierarchical authority. Compromise between cost and safety were routine. Documents showed that at the time decisions were made, engineers agreed that proceeding with launches was an acceptable risk. Explaining in testimony why they continued launching, they said it was not the best design, but it was working: “. . . you’ve got to have a strong reason to go in and redesign something, because it costs dollars and schedule. You have to be able to show you’ve got a technical issue that is unsafe to fly. And that really just was not on the table that I recall by any of the parties . . .”

*Structural Secrecy.* Each question answered raised yet another. The production of culture explained how managers and engineers normalized the technical deviations, gradually expanding the bounds of acceptable risk; the culture of production explained why. But the problem had gone on for years. Why had no one recognized the anomalies as warning signs and intervened to halt NASA’s incremental descent into poor judgment? Unexpected, and indeed startling, were the analogies with Spence’s theory, Revco, and uncoupling: again, the data showed a long incubation period with early warning signs that were either misinterpreted or ignored. The NASA data allowed an explanation of missed signals that showed the connection between macro, meso, and micro levels of analysis.

Like uncoupling, at the micro level patterns of information obscured problem seriousness, affecting the definition of the situation. Early warning signs were affected by their position in a stream of information: some signals were weak, and others were mixed. Each time an anomaly occurred, it was examined and fixed, and the safety of the component was confirmed by the next successful mission: thus a signal that something was wrong was followed by a signal that all was well. When anomalies became more frequent and serious, the change was gradual. The technical deviations became a routine signal, not a signal of danger. Further, organization structure created missing signals. As information was passed up the hierarchy, it was condensed, eliminating ambiguities in the engineering analysis. Finally, the structure of safety regulation contributed. Autonomy handicapped the external safety regulators, leading to dependence

on NASA for information. Unless NASA engineers defined something as a serious problem, they did not bring it to regulators' attention. Interdependence affected NASA's internal safety system. Because of budget shortages, NASA leadership cut the resources and authority of those internal safety organizations. In the absence of regulatory intervention, the cultural belief in acceptable risk of the solid rocket boosters persisted throughout the NASA organization.

The normalization of deviance in the years preceding *Challenger* was explained by the production of culture, the culture of production, and structural secrecy, in combination. I returned to the *Challenger* launch decision, now positioning it as one decision in a stream of decisions. Engineers were arguing against the launch, based on a new condition: the predicted cold temperature. They felt the condition made launching an unacceptable risk. As I put together the testimony and interview transcripts of all participating parties into a chronology, I realized this was the first time I had considered all of the accounts together. In striking analogy, I saw that the explanation of the history of decisions—the production of culture, the culture of production, and structural secrecy that had reinforced the normalization of deviance in the past—explained what happened on the eve of the launch. I concluded that the decision to launch was a mistake, the result of conformity to norms, rules, and patterns of the past, not deviance (for details, see Vaughan 1996, Chaps. 8 and 9). Analytic induction had driven the analysis to this unexpected conclusion.

This case confirmed that the method of analogical theorizing can lead to discovery of generic patterns and innovative theorizing. For the first time, I had data showing the direct link between macro, meso, and micro connections and outcomes: how elite leaders and powerful organizations in the political environment acted, changing the space agency structure and culture, affecting engineering decisions. The key new concepts were “the normalization of deviance,” the “production of culture,” “the culture of production,” and “structural secrecy.” The role of cultural beliefs emanating from the environment and organization, important in the uncoupling process, was repeated here as a major causal factor. Analogical to the new institutionalism (DiMaggio and Powell 1991), the case revealed cultural understandings as a mediator between institutions, organizations, and individual choice. Analogical to Bourdieu (1990), the case showed how the occupational habitus of engineers penetrated the organization, the pre-existing dispositions reproducing structure, culture, and ways of being.

The theory and several concepts generalized beyond the case. Structural secrecy, patterns of information, and missed signals were key in all three cases and were analogous to many examples of organizational failure, from national security before the September 11 attacks to child welfare offices and foster child abuse cases. Signals and interpretive work were neither solely social-psychological nor solely structural, but a product of the two. Although NASA's decision to launch the *Challenger* was an example of mistake, not misconduct, the normalization of deviance offered an alternative to amoral calculation as an explanation of how people who are upstanding citizens can engage in illegalities and deviant acts in behalf of their organizations. In addition, the concept may apply to other situations—neither mistake nor misconduct—in which behavior that outsiders view as deviant and unacceptable is viewed by insiders as normative and conforming in their organization and industry: for example, the practices of the banking industry in the 2008 U.S. credit crisis (MacKenzie 2011).

Theorizing didn't stop with the publication of the book. The cross-case comparison expanded my understanding beyond the explanation of each case individually. Although I started with an interest in organizational misconduct, at the end my interest was in the general subject of how things go wrong in organizations. Pursuing this broader agenda in a review of the literature on mistake, misconduct, and disaster in "The Dark Side of Organizations" I identified analogies and differences between the three types (Vaughan 1999). In all three, the outcomes resulted from the intersection of environment, organizations, and individual choice, meaning, and action. Thus the outcomes they produced could be treated as the result of organizational system failures. From the comparison, inductively I arrived at a set of definitions to be used heuristically—as Ideal Types—in future research. Though different, each of the three types (also my three cases) fit the larger category of organizational deviance, which I defined as "an event, activity, or circumstance occurring in and/or produced by a group, a formal or complex organization that deviates from formal design goals and/or normative standards and expectations, either in the fact of its occurrence or in its consequences, and produces a suboptimal outcome" (Vaughan 1999: 273). Mistake, misconduct, and disaster all fit this general definition, but to account for variation, each had a more precise definition of its own. Finally, I concluded that although the theory (as it stood after the *Challenger* analysis) developed from three cases of organizational deviance, it also seems applicable to deviant organizations, in which deviance

is the formal goal of the organization (Rafter 2011): for example, state genocide, organized crime, terrorist organizations, and instances of corruption that are organized and thus expected and non-deviant at the societal, country, community, or industry level.

### The Relevance of Analogical Theorizing for Sociology

In this chapter, I introduced analogical theorizing, a Simmelian-based method in which analogy operates both as a cognitive process and as a material practice of cross-case comparison. In my demonstration across three substantive cases I have shown that analogy comes into play at every stage of the research process: case selection, developing concepts and theoretical explanations, and the material practice of comparison. I suggest that these uses of analogy are common to the theorizing process, regardless of a researcher's preference for quantitative or qualitative methods, or whether the research design is intentionally cross-case comparative, or, I strongly suspect, whether or not the scholar doing the work is a sociologist.

Moreover, analogical theorizing is integrated in the research processes of citation, importation, and generalization. In the simple act of citation, often we are making a cross-case comparison, identifying either a similarity or a difference between our own case and the one we cite. We also are theorizing analogically when we import another work into our own in a more significant way, bringing in a theory or concept from research done in a different time or social setting to frame our case, to explain all or part of it, or to contest the other research. Importation occurs countless times in any published work. Here, because of space limitations, I demonstrated this in a limited way: I imported Merton into the explanation of the first book, Berger and Kellner framed the second, and Spence was generic to the explanation in all three. Finally, analogies are integral to generalizing beyond a case to other examples having similar generic patterns, as I indicated in the Challenger case concepts of structural secrecy, the normalization of deviance, and signals and interpretive work (for others, see Vaughan 1996: 400-415). Since I began the project, additional empirical cross-case comparisons have been done by scholars of different theoretical orientations, in different subdisciplines of sociology, using different modes of analysis. These examples show that cross-case comparison can be the core of a single study, and need not be as time consuming as my three-book sequence. In historical sociology, for example, Tilly (1985)

famously wrote a theoretical essay analyzing war making and state making as organized crime. Goldstone and Useem (1999) identified five principles of state-centered revolutions, then examined thirteen case studies of prison riots to identify analogies and differences. Using nonparametric statistics, they found that state-centered principles could be usefully extended to explain prison riots. In contrast to these two macro-level applications, Katz (1988), a symbolic interactionist and ethnographer, compared six types of crime, inductively raising a theory of situational transcendence that worked across types. The offender's emotional response to particular situations allowed him or her to transcend it, so that at the moment the crime was committed the criminal offense became a morally justified act in the offender's eyes. The variety of methods and the rich and novel insights in these examples suggest the promise of analogical theorizing for theory generation in sociology. Notably, none of the authors discussed the role analogy played in their theorizing or the method behind their work.

Swedberg (2012) has stressed the importance of teaching how to theorize. My purpose in this chapter has been to transform the invisible and unacknowledged process of theorizing into a visible, intentional, systematic process of cross-case comparison that can be taught. Moreover, I have shown how situated action, using concepts and theories as Blumerian sensitizers, and analytic induction, lead to theoretical innovation. In the introduction, I indicated that shifting the unit of analysis from one organizational form to another can contribute to theory building in several ways: (1) when studying similar events in different organizational settings, we gain access to different kinds of data that reveal previously unrecognized explanatory factors; (2) often shifting the unit of analysis shifts the level of analysis as well, allowing new insights into the micro elements of a macro-level explanation, or vice-versa; (3) shifting the unit of analysis can be advantageous for elaborating theories and concepts focusing on large complex systems that are difficult to study; and (4) it promotes integration of the research and theory of different scholars, studying organizational actions in various social settings and at different levels of analysis, moving us toward general theory. My three-case comparison demonstrates points 1 and 2 above, so I will not repeat those here, instead focusing on its advantages in relation to points 3 and 4.

Some aspects of large complex systems may be difficult to study, either because of access problems or restricted data availability, or because their size, complexity, and the kind of data that are available do not readily lend

themselves to fully answering the research question. Looking for answers in an analogous circumstance at a different level of analysis can be productive. Recall that Blau (1964) started with the intimate dyad, where details of interaction were available, then applied those same principles to groups, complex organizations, and some inter-organizational forms, elaborating his theory along the way. The development of concepts, too, can benefit from cross-case comparison at different levels of analysis. Consider “loose coupling,” which has a long history (Gouldner 1968; Weick 1976). Weick used the concept to explain educational organizations as loosely coupled systems, commenting that because of measurement difficulties with education systems, the concept lacked precision, but still could be used heuristically, as indeed it has been. Then Perrow (1984) introduced the concept of tightly coupled systems. Tight coupling also has enjoyed widespread use, but similarly left some degree of ambiguity. Greater ambiguity is desirable because it opens new research questions. Qualitative research on social movements, networks, or cohabiting couples, for example, could explore the extent to which parts of an organizational unit are interdependent, bringing greater clarity about what it means to be tightly or loosely coupled, the range of variation, circumstances when both can coexist in one organization, or how an organization can move from one to the other.

The fourth point, how analogical theorizing can build toward general theory by promoting theoretical integration, speaks directly to the issue of disciplinary specialization. We tend to develop a professional niche for ourselves, whereby we selectively study a particular organizational form that has a particular function: nation-states, corporations, families, hospitals, courtrooms, social movements. While our depth of knowledge and expertise enhance our ability to develop theory within our own area, the overall result is that the production of knowledge occurs in fragmented rather than integrated ways. While in-depth study of one type helps us to more clearly specify the patterns and variations within type, disciplinary boundaries prevent finding support for and challenges to our own theories that could come from reading across types. Also, specialization blocks exposure to theories and concepts developed to explain similar events or outcomes in other organizational forms. Finally, specialization leads to methodological preferences that consistently locate our work at the same level of analysis. We tend to define problems at either the macro or micro level, but typically not both. We see and investigate a problem in a way that limits our interest in data at other levels of analysis

and precludes our integration of other theories or concepts that might bridge the gap between macro-specialists and micro-analysts.

Analogical theorizing can promote theory building by theoretical integration across disciplinary specialization and the macro-micro divide. Framing a case as situated action is one way to bridge this gap. Creative selection of cases for comparison is another. Everett Hughes, influenced by Simmel, was fond of asking his students, “What do a prostitute and a doctor have in common?” Interested in professions, Hughes answered that both are entrusted with client secrets that, if revealed, had legal implications for the client (Becker, personal communication, November 1991). Traditionally, we think of comparative work as same-case comparison. Breaking away from structured professional predispositions and using our natural inclination for analogical reasoning in the research process can stimulate both theoretical innovation and theoretical integration across subdisciplines within sociology, and between sociology and other disciplines as well (on signaling theory, e.g., see Vaughan 2009: 704–06). Analogical theorizing has demonstrated the capacity to develop a cumulative conceptual base and to move us toward general theory that integrates macro, meso, and micro levels of analysis. I am not suggesting that there is some perfect explanation or analytic endpoint that can be found for a case or across cases. However, a stronger conceptual base, discovery of generic explanations, and intra- and inter-disciplinary exchange of theory and concepts not only leads to better theory, but also benefits sociology as a discipline.