Theor Soc (2008) 37:65–81
DOI 10.1007/s11186-007-9056-7

Bourdieu and organizations: the empirical challenge

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Published online: 5 January 2008

Abstract Emirbayer and Johnson critique the failure to engage fully Bourdieu's relational analysis in empirical work, but are weak in giving direction for rectifying the problem. Following their recommendation for studying organizations-in-fields and organizations-as-fields, I argue for the benefits of analogical comparison using case studies of organizations as the units of analysis. Doing so maximizes the number of Bourdieusian concepts that can be deployed in an explanation. Further, it maximizes discovery of the oft-neglected links among history, competition, resources, sites of contestation and struggle, relations of dominance and domination, and reproduction of inequality. Perhaps most important, case studies can identify the connection between macro-, meso-, and micro-level factors in the formation and shaping of habitus. To support my claims empirically, I draw from case study research (Vaughan *The challenger launch decision: Risky technology, culture, and deviance at NASA*, 1996; *Signals and interpretive work: The role of culture in a theory of practical action*. pp. 28–56, 2002) that verifies Bourdieu's as the "Theory of Practical Action" that supplies the micro-level component to the new institutionalism (DiMaggio and Powell, *Introduction*. pp. 1–41, 1991).

Bourdieu developed his theory over his career in case-by-case examination of different forms of social organization. Sallaz and Zavisca (2007) trace this trajectory by topic and chronologically: social structure and social action from the Kabyle people (1962, 1979); the reproduction of inequality in education in France (Bourdieu and Passeron 1977; Bourdieu 1988); social inequality in cultural production and consumption (1984, 1993, 1996), the theory of the state through language (1991), education (1998), and housing (2005). In "The Social Structure and the Economy" (2005) Bourdieu mentions "the firm as field." In other works, he speaks of educational organizations themselves as fields. Given that he developed the theory by comparing different forms

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of social organization, extending the theory's relevance to formal organizations is a logical step. Emirbayer and Johnson's accomplishment is to extend and generalize the specifics of Bourdieu's theoretical apparatus to "organizations" and to "organizations-as-fields." The term "organizations-as-fields" is theirs.

Like Bourdieu, Emirbayer and Johnson are engaged in analogical theorizing. "Analogy" refers to correspondences in some particulars between things, otherwise unlike. By analogical theorizing, I mean developing theoretical explanations by cross-case analysis that compares similar phenomena in different social forms that vary in size, complexity, and function (for rationale and exegesis of method, see Vaughan 1992). Shifting the unit of analysis from one social form to another is the essence of developing theory by analogical comparison. Many sociologists seem intuitively to theorize by this method without acknowledging analogy's role in their theorizing, without drawing attention to their use of cross-case rather than same-case comparison, or giving a theoretical rationale or logic for such a comparison (see, e.g., the body of writings of Erving Goffman (1961) or, more recently, Goldstone and Useem 1999). To proceed systematically, some theory or concept is used heuristically to organize the data.[1] Then that theory or concept is used to sort the data, looking for both analogies and differences. The differences found are the innovative elements that lead to reconceptualization. One of the benefits of proceeding thus is that comparing different social settings produces different data, at different levels of analysis, yielding new insights that challenge or advance the theory or concept in new directions.

Emirbayer and Johnson's thoughtful extension of Bourdieu's theory to organizations could add new life to organizational analysis. Although they incorporate many examples from research in their exposition, the article is primarily a theoretical one. They have laid out the turf. My response to their article focuses more deeply on the empirical challenges that their reconceptualization presents, how we who study organizations can use this theoretical extension empirically, what might be learned from the application of Bourdieu's relational analysis to organizations-as-fields that might affirm, contradict, or refine his theory, and how such an extension might build organizational sociology.

Separation of concepts and the demise of relational analysis

Emirbayer and Johnson argue that the potential for a fully relational analysis has not been realized in organizational sociology due to the separation of three of its central concepts: field, capital, and habitus. This, say Emirbayer and Johnson, is a misappropriation of Bourdieu's ideas because the relational component – the dynamic properties of this theory and the relation between the concepts – is lost. It is true that each of these concepts has established a separate historical trajectory in research in organizational sociology. However, this practice is not limited to organizational sociology. Sallaz and Zavisca (2007) found that all the articles citing

[1] We always bring to our research certain assumptions about how the world works and theories derived from our reading and research. Even though we are not by design testing those assumptions and theories, making them explicit at the outset, then looking for similarities and differences, is a good way to guard against self-fulfilling prophecies.

Bourdieu in four prominent American sociology journals between 1980 and 2004, only 9% (21 of 235) employed all of his main concepts relationally.

A concept that has been excised from the context of its original theory and travels alone is not unusual in sociology. In fact, many have been excised thus, becoming institutionalized, some falling into the vernacular and used without citation. A few examples will suffice. The concept of opportunity structures, which originated in Merton's 1938 "Social Structure and Anomie," has had a long and productive life torn from its place in an integrated theory of deviance (Merton 1995). Similarly, the concept of isomorphism (DiMaggio and Powell 1983) has separated from its theoretical point of origin, diffusing across specializations within sociology and across the disciplinary boundary to other disciplines. Recall also the excision from the original theoretical context of concepts such as black box (Latour 1987), co-optation (Selznick 1949), standpoint (Smith 1987), and loose coupling (Weick 1976). Although in some instances the original theorist might correctly conclude that the concept-in-use is so far from its original meaning that its use is inappropriate, casual reading suggests that in most circumstances the concept imported alone affirms its own strength, consistent with but independent of the original theory. Further, the nuance of the concept is discovered when examined in different social settings, the variety and comparison of data elaborating its meaning and use (see, e.g., Merton 1995). "Capital" and "field" are examples that make this point. Thus, although the relational tenets of Bourdieu's theory may be violated, excising a concept and examining it in different social settings has its benefits.

However, we must wonder why, in sociology in general but in particular in relation to Bourdieu and organizational analysis, a concept becomes separated from the whole, especially since a theory by definition requires the interdependence of parts to formulate an explanation. The easy answer is scholar preference: the excision of a single concept is a combination of personal interest and the scholar's research problem. However, choice, as we know, is shaped by social circumstances. In the diffusion of ideas, a little-investigated process is the cognitive aspect of theorizing. A concept or theory becomes relevant as an explanation of our data when we recognize analogies between our data and the concept. However, this cognitive connection may not be made because of (1) a strong competing theory or paradigm (Kuhn 1962), (2) the complexity of the theory in question, or (3) specialization and data availability. Thus we arrive at a logical explanation for Emirbayer and Johnson's complaint. It is not that organizational sociologists have failed to grasp the relational aspects of Bourdieu's theory. One or all – competing paradigms, theory complexity, and specialization and data availability – can be obstacles to incorporating a full theory into an empirical analysis because any or all of the three may prevent us from seeing the relevance of the whole, even as these same factors may enable us to see the relevance of one or more of the theory's parts.

Consider first how the dominant paradigm in organizational sociology may restrict the importation of the full Bourdieusian formulation. Central to Bourdieu's theory is the dynamic of domination and the relation between dominator and dominated. The field is a site of ongoing struggle; structures of power reproduce and are reproduced by inequality. Although some organizational sociology always has attended to power and conflict as a central inquiry (see, e.g., Dalton 1959; Burawoy 1979; Perrow 2002; Pfeffer 1981; Morrill 1995), several scholars have noted that

historically it has focused more on other aspects of organizations than domination relations, contestation, inequalities, and pathologies (Perrow 1986; Scott 1998; Vaughan 1999). This general tendency has been reinforced since the publication of DiMaggio and Powell's theory of institutional isomorphism (1983), in which legitimacy replaced competition as the engine of structural change in organizational fields. As an outcome of the search for legitimacy, organization structures come to look alike, but change occurs gradually in response to institutional forces. Widely adopted in sociology and other social science disciplines, the new institutionalism has contributed to the continuing displacement of power, domination, conflict, and struggle from the principal research agenda of organization scholars (for exceptions see DiMaggio 1988; Brint and Karabel 1991; Podolny 1993; Scott et al. 2000; Dobbin and Dowd 2000; Colyvas and Powell 2006; Mohr and Guerra-Pearson 2007).

Second, even in the clear language of Emirbayer and Johnson and the simplification they impose, the complexity of the theory and thus the requirements of research on formal organizations that is relational and includes the three concepts are enormous. The Bourdieusian social world is complex and in their extension Emibayer and Johnson depict it so. First, the social is a multi-layered space: organization fields are themselves embedded in a larger system of fields (Scott 1994). To ignore the relations between the organizational field and this larger, semi-autonomous field in which it is situated is to miss an important dynamic. Further, fields themselves comprise individual organizations that are intra-organizational fields. The possession of capital is a resource that can be deployed as both weapon and stakes in struggles within and between fields. Moreover, the kind of capital in use at a particular moment can vary, depending on the situation and on the field. The concept of habitus may apply to either individuals or organizations, but in either application history is a crucial component of the analysis and must be included.

As Emirbayer and Johnson's discussion moves beyond the "theoretical triad," the challenge becomes even greater. How to add to this already rich mix the state as a field of bureaucratic-administrative agencies and the concept of symbolic violence? The dynamics of dominance and domination between and within multi-layered levels of analysis? Reproduction and change? Race, class, and gender of collective actors? How can we also incorporate the symbolically meaningful position-taking within the field as a space of position-takings – which they stress is essential to understanding contestation – given the multiple goals of organizations, variability in goals over time, and the number of organizations within a field? The murkiness of a relational analysis with so many complex inner-workings and concepts makes integrating them in a single research project a daunting prospect.

The remaining obstacles to wholesale exploration and importation of a theory are specialization and data availability. Emirbayer and Johnson decry "counterproductive divisions of intellectual labor in organizational analysis such as, for example, between social psychologists who study microprocesses and sociologists who study macroprocesses" that keep the promise of a Bourdieusian framework from being realized. (E&J:5). This observation is true not only for organizational analysts, but for sociologists generally. The reasons for this divide are complicated. To use Bourdieu's theory as a tool for organizational analysis calls for making the macro-micro connection, which in turn requires data at different levels of analysis that also

allow the relational aspects of his theory to be explored. How to find data that capture it all, history included? If found, the unwieldiness of data sufficient for a conceptually complete Bourdieusian relational analysis still may limit the extent to which the full theory can be explored, forcing the researcher to be selective in concepts investigated.

Further, a Bourdieusian relational analysis requires researcher skills and methods appropriate to different levels of analysis. Although graduate education introduces us to macro-level perspectives and micro-level approaches and theories that espouse the connection between the two, our training does not include instruction on how to make that connection empirically. In fact, professional training and job markets work against our doing so by channeling us to specialize in either macro- or micro-level approaches, not both. Moreover, from the diversity of methods initially learned, as professionals we tend to specialize in a certain research style – either quantitative or qualitative – and sometimes, within those categories, a particular technique. This occurs for a number of reasons, including our area of specialization, favored theoretical stance, graduate department strengths, influence of advisors' research, or our own empirical preferences, strengths, and weaknesses.[2] Of the obstacles to doing the kind of fully relational analysis that Emirbayer and Johnson suggest, this latter one is perhaps the easiest to surmount: collaborative research between scholars trained in macro-level analysis and those trained to explore the micro-level. However, for scholars to get to the point of contemplating a research design for a fully relational analysis, the other constraints of a competing paradigm and theory complexity would have to be less influential.

Realizing the potential

How then might the theoretical potential of a relational analysis be realized in the empirical world? Emirbayer and Johnson warn that the potential will not be fulfilled "if the methods for conducting empirical research within it remain obscure even to interested scholars" (E&J:49). However, their concluding section on "Methodological considerations" receives relatively little attention compared to the amount of space devoted to translation of Bourdieu's theoretical apparatus. Understandable in light of journal publication page limits, "Methodological Considerations" is written in broad strokes. They return to the theoretical triad with which they began, setting aside the complexity of the theorizing they have accomplished to focus on the central problem of relational analysis. Scholars should not be fixated on one method; multiple methods can be used, they argue. To capture the relational element, they suggest several methods for doing so in organizational sociology (E&J:50–55). The first three – correspondence analysis, network analysis, and surveys – are devices for mapping a field and connecting positions and position-takings within it. The fourth is ethnography that, they point out, would be especially effective for examining habitus of different actors in organizations – as-fields, cautioning that any ethnographer must recognize that "the actors in question interact as occupants of

[2] Scholars do combine methods but they tend to combine at the same level of analysis, not in pursuit of the macro-micro-link.

positions in a structure of relations (and thereby as bearers of different types of dispositions from within a space of dispositions" (E&J:52).

History is, in their view, the biggest methodological challenge because the historiography of a given field requires a researcher to master a massive amount of history and because histories are necessarily restricted by disciplinary specialization and historical conditions at the time a history is produced. They offer no suggestions about how to surmount this obstacle, only optimism that it can be done (54). Capital, a concept known for its empirical ambiguity (Lamont and Lareau 1988; Sallaz and Zavisca 2007) gets no specific discussion, left implicit in their discussion of methods for fields. Each method is appropriate and useful, but for different conceptual purposes. They do not make suggestions for how to combine them in a relational analysis that uses all the central concepts. Inadvertently, their discussion continues the separation of concepts that they aim to rectify in the preceding pages. Finally, however, they raise the question of case studies: is Bourdieu's agenda for a generalizable theory of fields, or of organization fields, possible only with "large-n" research design? What is the role of the case study in identifying the invariant properties of fields? Emirbayer and Johnson answer that case studies offer the potential to move from the particular to the general, but only if scholars who conduct them strive always for the relational, avoiding what they view as the weakness of most case studies (and paralleling their caveat for ethnography) to ignore the effects of "the broader system of organizations within which it is located" (E&J:55).

Indeed, case studies may offer the greatest potential. Recall that Bourdieu began as an anthropologist; his initial elaboration of his theory was based on a case study of the Kabyle (on Bourdieu as a fieldworker, see esp. Wacquant 2004). Case studies are known for inductively developing theory and for exploring the new and the relatively unexplored. For formal organizations, the relational aspects of Bourdieu's theory *are* relatively unexplored, thus case studies of organizations-as-fields would help fill theoretical gaps. One advantage of analogical theorizing is that shifting the unit of analysis brings different kinds of data, previously unavailable, often resulting in data at a different level of analysis. Thus, case studies of organizations-as-fields have the potential for joining micro-actions within organizations and macro social structures. Including history also becomes an empirical possibility. History of organizations is found in accounts other than those of historians: formal organizations keep records and records are kept on them by instruments of the state as well as by other organizations in a field, so history for both the field and organization-as-field can be traced. Interviews and observations can also be done. Habitus can be known by observing the enactment of dispositions in practice.

The combination of these diverse methods and the kind of detailed data that a case study in a naturalistic setting can produce are more likely to capture the intricacies of Emirbayer and Johnson's conceptualization: for example, their suggestion to examine the perceptions of actors in a field, based on position in social space and capital, and how those perceptions resolve into strategic action seem best exposed by case study data. Specialization and available data are less likely to restrict the number of concepts that are deployed because to explain the case, all data must be considered, which has the effect of forcing the researcher to entertain alternative hypotheses (Vaughan 1992:196–199). Not all concepts may come into play, depending on the research question and the data, but the potential for

a relational analysis that uncovers many of Bourdieu's concepts in one research project is there.

Organizations-as-fields within fields: making a case for the case

To illustrate the potential of case studies, I use my (1996) inquiry into the National Aeronautics and Space Administration's (NASA) 1986 space shuttle *Challenger* accident and NASA's flawed decision to launch, over the objections of engineers. Bourdieu's theory became an important part of both my ongoing theorizing process throughout and, finally, one of the major theories explaining the case. That explanation joins neo-institutional theory with Bourdieu's habitus to form what DiMaggio and Powell call "A Theory of Practical Action" (1991:1–38). The analysis shows how institutionalized dispositions from the field materialized and were elaborated upon in the organization, such that habitus derived from layered structures – macro-, meso-, and micro- – affected working engineers' meaning, interpretation, and actions.

The research began with one question: why did NASA launch *Challenger* against the advice of concerned contractor engineers, who warned that the predicted cold temperature was a serious risk to the rubber O-rings of the Solid Rocket Boosters? The government investigation raised a second, even more worrisome one: why did NASA continue to launch with repeated damage to the O-rings for five years preceding *Challenger*? The answer to both questions was the "normalization of deviance." By that I mean that unexpected technical deviations discovered after a shuttle launch and initially defined as risky were, upon engineering analysis, deemed acceptable. Occurring repeatedly, the anomalies became expected and routine for shuttle flights. Each time, the boundaries of acceptable risk expanded to include more serious incidents. Because of my earlier work on organizational deviance, the NASA accident seemed, by the circumstances publicly known at the beginning of my project, to fall into that category. The causal factors between cases appeared to be analogical. Using analogical comparison, this time the unit of analysis would be a government agency rather than a corporation, as before.

The research was an historical ethnography: an attempt to reconstruct structure and culture from archival documents and interviews to see how people in a different time and place made sense of things. The analysis was inductive, proceeding by grounded theorizing. However, contradicting the theoretically neutral stance of grounded theorizing, analogical comparison takes for granted that researchers have many theories and concepts in their repertoire. Making those explicit creates the possibility of becoming more systematic by looking for differences as well as analogies. At the outset, theories of organization deviance guided the analysis, but as the data analysis dictated, other theories dominated. The government commission investigating this accident published a five-volume report and placed over 200,000 NASA documents and 9,000 pages of interview transcripts on reserve at the National Archives, Washington D.C. I used transcripts and videos of the official public hearings and I interviewed key participants, asking questions designed to fill gaps in the analysis and theoretical explanation. Unusually abundant data like NASA's are not necessary for a relational analysis. Indeed, I did not use the entire database, but

selectively drew from it, based upon the requirements of my initial research question and new questions raised as the research progressed.

Emirbayer and Johnson discuss the sticky problem of empirically defining fields (E&J:7–10). This case was not a study of an organizational field per se (e.g., its trajectory or transformation over time); rather, it was the study of an organization-as-field that was also an actor in and affected by a larger organization field. Case studies of organizations-as-fields within fields need some narrowing of the boundaries of both because otherwise the project may be impossibly unwieldy. NASA is a sprawling, geographically-dispersed, government bureaucracy in the aerospace industry. NASA's institutional environment is equally sprawling.[3] NASA has many programs and projects, each involving different parts of the NASA organization as well as different organizations in the organization field. How to define these fields empirically was not clear to me at the outset, but only became clear as my knowledge of NASA, the relevant organizations, and the case grew. What was clear was that the answer to both research questions required not only understanding the relationship between these two fields, but also the semi-autonomous field of international actors and relations in which they were embedded.

The dependent variable, decision making about Solid Rocket Boosters, which were the technical cause of the accident, determined the relevant actors and thus the boundaries of fields. The boundaries matched natural distinctions in social organization. Neither field was a network because not all interacted with each other or with NASA about the boosters. NASA-as-field included layered intra-organizational structures related to Solid Rocket Boosters decision making, listed here top-down: (1) NASA Headquarters top administration because of goal setting and policy decisions crucial to the shuttle program and the accident, (2) Marshall Space Flight Center, Huntsville, Alabama (responsible for the Solid Rocket Booster Project and several other shuttle component Projects). I included top administrative units participating in pre-launch Flight Readiness Reviews and launch decisions, internal safety regulatory bodies, engineers in the Science and Engineering Directorate responsible for designing and implementing safety tests, and establishing safety criteria, and (3) within Marshall, the Solid Rocket Booster Project (project office, project manager, personnel), and (4) within the Project, the Solid Rocket Booster work group:[4] those engineers and managers of NASA and Thiokol, the Utah contractor producing the boosters, whose main assignment was collaborative hands-on technical work and engineering risk assessments.

Similarly, I divided the organization field into layered structures: (1) historic actions in the international arena that affected the overall political and economic climate of the aerospace industry, which in turn affected the space program (2) organizations that were connected to all NASA operations, either directly or

[3] Minimally, it would include contractors, safety regulators, the White House and Congress, other government administrative departments, business partners (e.g., the Department of Defense, the Russian and Japanese space programs, research institutions, university science and engineering departments), competitors in private enterprise and foreign government defense and space programs, etc.

[4] Formally designated as Level IV, the work group was responsible for all the hands-on engineering work for the boosters and also formal risk assessments prior to each launch, which are forwarded up the hierarchical launch decision chain.

indirectly: educational institutions that trained engineers, external safety regulators, the White House, Congress, and the Office of Management and Budget, (3) suppliers, customers, and partners directly related to the Space Shuttle and the Solid Rocket Booster Project.

In the next section, I indicate how relational analysis and Bourdieu's concepts figured into the study. The case shows habitus as the connective tissue between macro-, meso-, and micro-levels of analysis. The reproduction of dispositions in nested fields also was a principal theoretical finding. Affirming that shifting the unit of analysis from the organization to a subunit also can produce different kinds of data, the data for the eve of the launch reveal conflict, dominance and domination, and capital not visible in the history data. The theoretical explanation below is condensed, but still sufficient to demonstrate the explanatory power of Emirbayer and Johnson's extension; however, its brevity also reduces the complexity of both Bourdieu and the case. Necessarily, supporting data are omitted, thus neglecting especially individual interaction and the social construction of meaning so significant to explaining events at NASA. Further, it precludes citations to the many other authors whose ideas were crucial to theorizing the case.

The history of decision making: field, history, and habitus

What explains the normalization of deviance in the years prior to *Challenger*? At the micro-level, history mattered: the first decision to fly with an anomaly justified others; then the pattern of information as launch succeeded launch affected the Solid Rocket Booster work group's definition of the situation. The immediate social context was important to their construction of risk. The shuttle was an experimental vehicle, thus problems were expected on all parts of the shuttle. So having an anomaly was not in itself a signal of potential danger, but in fact the norm. When the first unexpected booster anomaly occurred, a cause was identified, a corrective action taken, and the boosters defined officially as an "Acceptable Risk" for the next launch. Each flight with anomalies was followed by one or more without incident. Risk was mediated by the pattern of information: signals of potential danger appeared mixed, weak, and routine. Had anomalies been occurring on every launch, people might have reacted differently, but they believed that the cause changed for each incident and they had fixed it. The repeating decision pattern indicated the development of a construction of risk that became cultural: shared by Solid Rocket Booster managers and engineers alike and passed up the NASA hierarchy.

This disposition to fly with a flawed design was reinforced by macro- and meso-level factors. Emirbayer and Johnson write of the possibility of an organizational habitus. Wacquant supports this reading: "... our categories of judgment and action, coming from society, are shared by all those who were subjected to similar social conditions and conditionings (thus one can speak of a masculine habitus, a national hiatus, a bourgeois habitus, etc.) (2005:316)." For NASA that organizational habitus reflected categories of judgment and perception of the organization field: "institutionalized beliefs, rules, and roles" that constitute shared cognitive systems (Scott 1991:165). Habitus is the analytic link that connects individual behavior and social structure. Institutions are essential to this perspective because of their

connection to the distribution of dispositions. The NASA case showed how sets of organizing assumptions institutionalized in the organization field trickled down through layered structures – the NASA organization, the Solid Rocket Booster Project, the Solid Rocket Booster work group – shaping individual cognitive processes and actions. These institutional logics were elaborated upon by the organization, the project, and the work group and thus transformed into substantively crafted, situation-specific scripts. At the micro-level, they created a way of seeing composed of shared understandings about risk, how the work should be done, and the criteria for decision making, The macro-meso-micro-connection worked as follows:

Historic political and economic decisions made by major players in the organization field changed the NASA organization, affecting decision making. During the early phases of the space program, consensus existed for the importance of NASA's mission; thus abundant money from the federal budget was assured. However, at the inception of the Space Shuttle Program in 1970, the situation changed. Formerly a well-funded, autonomous R&D agency, the space agency became politicized, laden with bureaucratic rules, and was forced to operate more like a business. The result was a changed NASA organization culture in which schedule, budget, following rules and procedures, and allegiance to hierarchy displaced safety and deference to the expertise of working engineers. The data showed how these layered dispositions played out in what people said and did. NASA managers and engineers at the time of *Challenger* consistently asserted situation-specific scripts: "We were absolutely relentless and Machiavellian about following through on all the required procedures ..."; "No one has to tell you schedule is important when you see people working 12 hours a day around the clock, evenings and weekends;" "The problem was the increasing launch rate ... the system was about to come down under its own weight just because of the necessity of having to do all these procedural things in an ever accelerating fashion;" "I told my boss my concerns, but they can take our analysis and throw it in the trash can if they want to. I've done my job."

Historically, these organizational dispositions are traceable to the semi-autonomous field in which NASA's organization field and NASA-as-field are situated: capitalism, as it materialized in the international competition for space, the conflict and competition in the aerospace industry, and education systems training professional engineers. Education sensitizes students to the competitive world of engineering they are entering. Professional schools prepare them to work in bureaucratic production organizations, where cost, efficiency, and schedule are valued and prioritized. Engineers learn (1) that their place in a hierarchical system and the importance of conforming to rules and procedures, (2) that the quality of a technical design typically is compromised by competitive pressures and the need for efficiency and economy, such that safety is often compromised, (3) that in experimental technologies, like the Space Shuttle, having anomalies is normal.

These institutional logics of the profession and the aerospace industry were taken-for-granted aspects of worldview that preexisted managers and engineers' entry into the workplace and were reproduced and reinforced in the NASA organization. At NASA, they became cultural imperatives. In their view, managers and engineers were not engaging in deviance by continuing to fly with a flawed design, as their

actions appeared to outsiders after the accident. Instead, they were conforming – to the scripts of professional engineering, the industry, and to NASA cultural imperatives of bureaucratic rule-following, cost efficiency, and meeting the schedule. Launching with anomalies was expected unless the evidence indicated the problem could take the shuttle out of the sky. Based on their social location and their interpretation of the data available to them, O-ring anomalies were an acceptable risk, not a threat to flight safety.

The eve of the launch: contestation, capital, and reproduction

In theorizing by analogical comparison, the smaller unit often proves to be a microcosm of the layered structures in which it is embedded. Such was the case for the Solid Rocket Booster work group that convened on the eve of the *Challenger* launch. The teleconference was a site of conflict in which existing inequalities; bureaucratic, cost, and production imperatives at NASA; and the dispositions of professional engineering training and the aerospace industry were reproduced and affirmed. Here's what happened:

On the eve of the *Challenger* launch, consensus about risk was shattered by the prediction of unprecedented cold weather. The teleconference participants included 34 NASA and Thiokol employees located in facilities in Utah, Alabama, and Florida. The Thiokol engineers in Utah presented a data analysis to support a no-launch recommendation. A debate ensued, during which the Solid Rocket Booster Project Manager at Marshall contested Thiokol engineers' no-launch recommendation, challenging nearly every point of their risk analysis. Thiokol administrators, responding to the strong opposition from their customer, called for a Thiokol caucus off the telecon line. In the caucus, Thiokol engineers repeated their engineering analysis. Reaching no consensus, Thiokol administrators made a "management risk decision" to proceed with launch, excluding contractor engineers from the vote. Back on the telecon line, Thiokol managers announced the reversal as the new Thiokol position, not mentioning that engineers had not voted. The Marshall manager requested further comments from all on the line. No one expressed dissent. The work group once again converted an anomalous condition to an official "acceptable risk;" NASA proceeded with launch.

What explains this outcome? The data show how the organization field and organization-as-field influenced the decision, the configuration of power relations, and how capital and position in a structure affected position-taking and the outcome. History was present in three important ways: (1) at the micro-level, the cultural belief, created in the history of decision making, that O-rings could withstand damage and were an acceptable risk, and (2) at the meso-level, a NASA organization culture dominated by production pressure, bureaucratic rule-following, and allegiance to hierarchy, (3) at the macro-level, reinforcing dispositions from the organization field that affected practices. Schedule and cost concerns had a subtle but powerful affect on the proceedings from the outset. The *Challenger* launch date had already been deferred several times. Thiokol engineers hurried to put their analysis together so that if the launch had to be cancelled, it could be cancelled before midnight, thus saving the expense of fueling the shuttle. To save time, they

divided up the engineering analysis. Specialists worked separately on individual charts while a small group worked on the launch recommendations charts. Hurrying, they did not collectively examine their analysis prior to faxing the charts to the other locations, so they did not realize the data did not adequately support the no-launch recommendation.

Cost and schedule again affected the proceedings as Marshall's Solid Rocket Booster Project Manager vigorously challenged the analysis.[5] The configuration of power by position was as follows. By virtue of formal authority, expertise, and experience the Solid Rocket Booster Manager possessed symbolic capital over the other telecon participants. However, organization-wide, his position was different. He was responsible for meeting the Project schedule and reporting the no-launch recommendation to his superiors in the launch decision chain. Strong quantitative data were valued at NASA. The engineering analysis was flawed, putting him in the position of defending an indefensible engineering risk assessment in order to stop the launch. As the contractor, Thiokol was dependent upon NASA for continuation of their contract, which was up for renewal. Top officials in Utah did not want to delay the launch on the basis of evidence that their customer found unconvincing. The power/dependence relation between Marshall and Thiokol and the symbolic capital of the Marshall manager led Thiokol officials to call for an off-line caucus and make a management decision to go forward. They responded to the objections and symbolic capital of NASA's Project manager, reproducing system inequality as they disempowered their own engineers.

Social location was crucial to the outcome. Position affected access to information, ability to interpret it, willingness to speak, and ability to convince others. Information that might have turned things around was not presented. Engineers, who held less symbolic capital, were complicit in their own subordination. Some who had data about O-ring resiliency tests were silent because they were no longer assigned to the Solid Rocket Booster Project, were not up to date, therefore were in doubt about the significance of their information. Thiokol engineers at Marshall and in Florida said nothing, feeling that their coworkers in Utah had the most recent information and they did not. When challenged by Marshall, Thiokol engineers in Utah persisted but then stopped. They did not object further because the system of domination was scripted in norms as well as formal rules: management risk decisions were the norm when no consensus was reached on technical issues. At the conclusion, when the Marshall manager asked whether anyone on the telecon line had anything more to say, engineers in Utah who dissented said nothing. They knew their place (recall the norm "... they can take our analysis and throw it in the trash can if they want to. I've done my job"). People in the other two locations did not know the engineers had not voted. Further, Thiokol engineers never knew they had support from engineers in other locations.

The data show how and why positions in the structure were associated with different perceptions of managers (responsible for budget and schedule as well as safety) and engineers (primarily concerned about safety) and differences in strategic

[5] He pointed out that the engineering analysis did not prove an association between cold temperature and O-ring erosion because the charts contained mixed, weak, and routine signals. Hadn't erosion occurred for many reasons in the past? Wasn't the most serious erosion on the warmest launch?

action that night. Position-taking was either for or against launch. Symbolic capital associated with the top position in the Solid Rocket Booster Project trumped other kinds of capital, such as expert capital of working engineers. Position acquired symbolic capital because organizational habitus valorized hierarchy and people recognized the unequal distribution of power as legitimate (Bourdieu 1991:118). Differences in social or economic capital of the participants are not known, but did not appear to have a direct affect on the discussion and outcome, although they may have had an indirect effect.[6] Bourdieu writes about the contingency of the accidental, which makes innovation and change possible (1990:55). The cold weather was that unexpected contingency. That night, uncertainty prevailed. Thiokol engineers initiated a break with the past by putting forward an unprecedented no-launch recommendation. However, it was innovation within limits. We can see the operation of habitus throughout the teleconference: all participants conformed to the rules and norms of past engineering decisions in the Solid Rocket Booster work group, and the rules and norms of the NASA organization, the industry, and professional engineering, reproducing them as they again engaged in the normalization of deviance. We know that people in conditions of uncertainty tend to fall back on habit and routine to get them through. Those assembled followed all the rules, acting out of history and experience, reproducing and maintaining those very rules in interaction on the eve of the launch.

Bourdieu and organizational analysis; organizational analysis and Bourdieu

In analogical theorizing, we must ask what the extension of a theory to a different unit of analysis produces theoretically. Based on this example, what we can conclude about Emirbayer and Johnson's suggestion about the utility of case studies of organizations-as-fields? What does it indicate about the theoretical benefits of Bourdieu's theory for organizational analysis? What about the theoretical benefits of organizational analysis for Bourdieu's theory?

Although this case was unique because a government investigation made unusually extensive data available, it underscores the potential of their idea about organizations-as-fields and suggests that even without a government investigation, case studies make relational analysis possible because interviews, observations, and archival records can combine to show the nexus of layered spaces and practices invisible under other research strategies. The records kept by NASA on all shuttle technical decisions allowed me to trace habitus, connecting structure with agency, connecting history to the present, showing how and why the normalization of deviance occurred. The dispute on the eve of the launch splayed open the social for examination, showing how dispositions of both field and organization-as-field affected that crucial decision made in the Solid Rocket Booster work group. The dispute illuminated the micro-dynamics of conflict, dominance and domination, position and position-taking, organization-specific capital, history and habitus, and the social reproduction of structured dispositions and system inequalities. The data

[6] Social and economic capital no doubt influenced who was employed by NASA, who was hired at what level, and who was promoted.

and an inductive analytic procedure made macro- and meso-level effects manifest in micro-level meaning and actions, thus showing how case studies and ethnography can account for the situated nature of social action: the interaction of actors as position occupants in a structure of relations and the broader system of organizations within which an event or activity is located.

How does this application to an organization-as-field challenge or contribute to Bourdieu's theory? It confirms Bourdieu's theory, verifying that the repetitive quality of much organized life cannot be explained by a consequentialist rational actor model, but by the preconditions of choice. The persistence of practices lies in their taken-for-granted quality and their reproduction in structures that are, to a great extent, self-sustaining. Bourdieu argues that habitus makes it possible to inhabit institutions, to draw on them practically, enacting their organizing principles and thus reproducing them but at the same time allowing for revisions and transformation (Bourdieu 1990:57) But innovation and revision did not happen here. History was reproduced in the present. Moreover, 17 years later, and despite post-*Challenger* changes, NASA personnel normalized technical deviation on another shuttle component, resulting in the 2003 loss of the space shuttle *Columbia*. The government investigation of this second shuttle disaster showed that the causes of *Challenger* had not been fixed: the same constellation of institutional, organizational, and social psychological factors were still at work (Columbia Accident Investigation Board 2003).

The differences found by examining organization-as-field suggest future research directions. This case demonstrates the salience for Bourdieu's theory of social organization at the meso-level. It shows how formal organizations can build upon and vary dispositions and schemas derived from macro-level structures, tailoring them specifically to practical activity in everyday life. This finding suggests that instead of identifying habitus as social location, defined as history and experience shared by the same class, habitus might better be investigated as the product of social location(s): positions in multiple structures that cut across class location. Understanding meanings and action at the local level sociologically means recognizing that individuals belong to multiple organizations (and to subunits and groups within them), such that the habitus is modified to fit the immediate local setting. Verifying the importance of the meso-level for Bourdieusian analysis and habitus, regardless of the size or complexity of the organization, is Wacquant's (2003) ethnography of boxing and a boxing gym and Hallett's (2003) analysis of organization culture in a corporate accounting firm. Both show the dispositions of habitus from which practices are formed.

Organization analysts might elaborate Bourdieu's theory by investigating whether different kinds of organizations are likely to reproduce collective beliefs to a greater or lesser extent; they may foster reproduction or innovation and change to varying degrees (Zucker 1977). Although the *Challenger* accident is explained by a combination of institutional, organizational, and social psychological factors, we must wonder to what extent NASA's rule-laden, rule-valorizing bureaucratic, hierarchical structure and culture accounted for conformity to these organizing principles. Another interesting revelation is the complexity of the relationship between capital and position in a structure. It shows that symbolic capital associated with position in the structure had salience over other kinds of capital in a discussion

in a formal meeting. At NASA, the Solid Rocket Booster work group was composed of members from various levels of the hierarchy. When a group or organization has a relatively flat hierarchy, what kinds of capital are influential and how does this vary by the activity? By gender? Does the salience of kinds of capital vary with the formality or informality of the discussion?

We must also ask what the case suggests about the benefits of Bourdieu's theory for organizational analysis. Its explanatory power in this case was central to understanding what happened. It verifies the suggestion of DiMaggio and Powell (1991) that habitus can join with neo-institutional analysis to form "a theory of practical action," filling in the microsociology of the new institutionalism. The similar role of structural predispositions in both theories and their compatible positions on choice (agency, yes, but choice within limits) make habitus an appropriate link between structure and agency. To date, most neo-institutional research has concentrated on organization fields (Scott 1994; Powell and Colyvas 2008). However, in analogical comparison, some aspects of a phenomenon can be studied at a different level of analysis because of structural and processural similarities (Vaughan 1992). Thus, findings about organization-as-field can generate hypotheses about organization fields and vice-versa. An example is the Solid Rocket Booster work group on the eve of the launch, which displayed the organization-as-field in microcosm, revealing the relational connection of many of Bourdieu's concepts (for a three-case comparison using Bourdieu and organizational analysis, see Vaughan 2002).Integrating Bourdieu's relational analysis into the sociology of organizations has general benefits, regardless of the method deployed. When concepts are excised from a theory and develop an independent trajectory (e.g., field, capital), the rest of the theory may be forgotten, falling into disuse by scholars who specialize in one concept within a theory. Emirbayer and Johnson's useful explanations and examples show scholars how relational analysis must proceed.

Finally, the joining of Bourdieu's theory and organizational analysis could be of great benefit to both. Bourdieu's emphasis on the social conditions of the emergence of economic actors, systems of exchange, the structure of the distribution of resources in a field, and the interactions among organizations around those resources lend themselves to inquiry into conflict and competition. Indeed, what better economic actors to examine than organizations? Organizations are twice-stratified: they have a position in a field of other organizations; the organization-as-field is stratified within by division of labor and hierarchy. Inter-organizationally and intra-organizationally, attaining desired resources is necessary to maintain position in the game, to increase position, or to keep from dropping out altogether. However, ability to achieve these goals may be constrained by the source, nature, and abundance of the resource, the behavior of other organizations or of subunits within the organization-as-field, or the resources already possessed and pre-existing demands on those resources. Thus, all organizations – non-profits and profit-seekers alike – must compete for the strategic resources they need. Add to this the fact of rising expectations: once a goal is achieved, a new one is set, thus some competition and scarcity will always be a factor. Consequently, organization fields and organizations-as-fields could be the richest units of analysis for studying links between resources, sites of contestation and struggle, relations of dominance and domination, and

reproduction of inequality. Moreover, this extension of Bourdieu could renew interest in these topics, currently displaced from organizational analysis.

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