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Organizational identities and the hazard of change

Michael T. Hannan, James N. Baron, Greta Hsu and Özgecan Koçak

We examine how the life chances and financial performance of nascent high-technology firms were affected by two kinds of organizational changes: altering founders' blueprints for the employment relation and replacing a founder–chief executive officer (CEO) by an outsider. We argue that both events destabilize organizations but that changes in employment blueprints are tied more tightly to the organization's identity and thus are more destabilizing. We analyze three dimensions of organizational performance among a sample of young high-technology companies in California's Silicon Valley: survival versus failure, launching an initial public offering (IPO), and changes in financial valuations among organizations that underwent an IPO. As predicted, changing the employment blueprint increased the hazard of failure and diminished growth in market value. Appointing an outsider as CEO did not affect the hazard of failure appreciably but did depress the rate of growth in market capitalization. The implications of these results for ecological and institutional perspectives on organizations are discussed.

1. Imprinting, identity, and disruptive change

The proposition advanced by Hannan and Freeman (1984) that altering an organization's core features is hazardous has been the focus of much theorizing and empirical research (Barnett and Carroll, 1995; Carroll and Hannan, 2000). This research has generally emphasized one side of the inertia story: the disruption entailed in reorganizing routines and architectures. The other side of the story—that changes in core features might be viewed as violations of deep-seated, taken-for-granted expectations by key organizational constituents—has received less attention. This article attempts to redress the balance by developing an identity-based notion of an organization's core and testing the proposition that changing such core features is especially destabilizing.

Notions of identity played an important part in shaping the original inertia argument. For instance, Hannan and Freeman (1984: 155–156) motivated their argument with the example of the university: although some features, such as textbooks, constantly change in an adaptive way, changing a curriculum from liberal arts to vocational training would be extraordinarily difficult. "The curriculum is difficult to

change, then, because it represents the core of the university's organizational identity and underlies the distribution of resources across the organization. In these ways, it can be said to lie at the university's 'core'." We argue that organizational changes go to the core when they challenge a well-established identity. Such change is risky because it confuses—and often angers—internal and external constituencies (Baron, 2004; Pólos *et al.*, 2002; Hannan *et al.*, forthcoming).

The hierarchy of inertial forces proposed by Hannan and Freeman (1984) rests on assumptions about the contention, cost, and disruption caused by changing specific domains. They assert that the most difficult organizational elements to alter, in descending order of flexibility, are mission, form of authority, core technology (including employee skills), and marketing strategy (ways of relating to external constituencies). These elements are "core" precisely because efforts to change them "raise fundamental questions about the nature of the organization" (Hannan and Freeman, 1984: 156).

The general argument that changing core features disrupts organizations has been tested using information on changes in leadership of newspapers (Carroll, 1984), administrative arrangements in hospitals (Zucker, 1987), land ownership of wineries (Delacroix and Swaminathan, 1991), lines of business of savings-and-loan associations (Haveman, 1992), geographic coverage of airlines (Kelly and Amburgey, 1991), frequency of newspaper publication (Amburgey *et al.*, 1993), college curricula (Kraatz and Zajac, 1996), engine characteristics in automobile manufacture (Carroll and Teo, 1996), product portfolios of semiconductor firms (Barnett and Freeman, 1997), formats of radio stations (Greve, 1999), strategies of social movements (Minkoff, 1999), bicycle designs (Dowell and Swaminathan, 2000), and niche width, defined in terms of the range of engine capacities offered, of automobile manufacturers (Dobrev *et al.*, 2001). The authors of most, but not all, of these studies interpret their results as supporting inertia theory. More to the point of the present research, these researchers emphasize the more "technical" account—that deep change causes reliability and accountability to fall (Péli *et al.*, 2000) and also causes organizations to incur costs of reshaping operations and to miss opportunities during periods of reorganization (Hannan *et al.*, 2003). Some of the studied changes (e.g., changes in curricula and in social movement strategies) might have challenged identities, but the research does not make clear whether this was so.

This issue is complicated by the fact that external audiences might not define identities in terms of features of the organization itself. Hannan *et al.* (forthcoming) propose that audiences might codify features of products, attributes of the organizations that produce them, or both in defining categories. For instance, genre distinctions in art worlds generally focus on features of the product, as do differentiations among many consumer "brands." However, the distinction between artistic and commercial photography attends to characteristics of the photographer (Becker, 1982). The classic example of codified distinctions that build on both kinds of characteristics is American microbrewing, where the identity is restricted to producers whose products conform

to a clear code (concerning ingredients and forms of presentation) and who are not part of a larger organization that brews and markets "industrial" beer (Carroll and Swaminathan, 2000). In the first example, organizational features might be irrelevant to outside audiences, but changes in product features might cause a strong devaluation. In the latter cases, organizational changes might be more relevant to outside audiences.

The situation is simpler for insiders: they generally care about organizational features, especially those that specify their relationship to the enterprise. Across different types of industries and organizations, insiders' expectations regarding the nature of the employment relationship are likely to form a significant component of the identity they hold for their respective organizations. Therefore, studying the consequences of changes in organizational features that matter to employees might prove a promising strategy for testing the implications of the identity argument regarding organizational inertia.

This article explores whether identity-based resistance to change in employment relations disrupts organizations and degrades performance. By so doing, we hope to contribute to the fast-developing stream of sociological work on organizational identity[1] and to possible extensions of that work to other areas of sociology, especially social movements and culture (Hannan, 2005).

2. Employment relations and organizational identity

Identity refers to defaults for relevant audience members that organizations will possess certain features (Pólos *et al.*, 2002). Identities prescribe characteristics and behavior whose absence exposes organizations to negative evaluations and consequences (Zuckerman, 1999; Hsu and Hannan, 2005).

Without necessarily couching their arguments in terms of identity, diverse lines of theory have posited that (i) organizations embody particular cultural blueprints for organizing, including premises about employment relations and (ii) cultural premises guide subsequent organizational development (Guillén, 1994; Baron *et al.*, 2001). For instance, organizational ecologists have argued that survival prospects are enhanced by features that promote reliability and accountability, particularly clearly specified forms of authority and well-understood bases of exchange between members and the organization (Hannan and Freeman, 1984: 153). Hence, organizations benefit from beginning with coherent blueprints that foster reliability and accountability.

Altering such a blueprint, once it is imprinted, is risky and costly, particularly when such changes erode existing bases of identity and therefore go the organization's core. In a similar vein, neo-institutionalists contend that designers of organizations draw

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[1]Space constraints preclude review of this literature. Our formulation is influenced heavily by Zuckerman (1999), Carroll and Swaminathan (2000), Ruef (2000), Phillips and Zuckerman (2001), McKendrick and Carroll (2001), and Rao *et al.* (2003, 2005).

on cultural templates and conceptions of control in crafting employment relations (and other features) because this enhances legitimacy and because their own enculturation precludes doing otherwise (Fligstein, 1990). Such templates often govern subsequent adoption of many other features of the organization, such as organizational structure, core technologies, and strategic focus.

Existing work on inertia generally analyzes data from archival sources covering complete histories of industries. Thus, it is not surprising that attention has been directed at changes in technical features such as products and technologies that get recorded in industry directories. Although such distinctions might correspond to salient identities in the minds of members and important outside parties, we believe that an organization's identity often emanates at least as powerfully from the nature of its employment (or membership) relations. Product offerings, senior leadership, legal ownership, financial structure, production processes, and the like can and regularly do change without compromising organizational identity. It is harder to imagine an organization altering the premises underlying its employment relationships without affecting its identity.

In the context that we study—knowledge-intensive industries—employment relationships appear to form a key facet of organizational identity (Baron, 2004). Premises about employment practices guide an organization in selecting particular types of people to pursue particular goals in particular ways, motivated by particular kinds of rewards. Changes in these features likely meet with resistance and opposition, due to the powerful stake that members usually have in retaining the status quo.

Indeed, when employment relations are enduring, training investments are substantial and firm-specific, or knowledge requirements are high, an organization's human capital often constrains its potential product portfolio more than vice versa. The point is probably self-evident in the university context, where, given the institution of tenure, curricula are profoundly constrained by employment relations with senior faculty. Yet, this reasoning applies to many other industries that depend on developing and retaining the intellectual capital of their core employees, for example, pharmaceutical companies, advertising agencies, consultancies, and accounting firms. Insofar as identity (for some audiences) becomes embedded in an employment system, distinctions based on how organizations relate to the *labor market*, not simply or primarily to the product market, might be appropriate for studies assessing disruptive effects of change (Baron, 2004).

Without gainsaying the importance of technical features, we contend that characterizing organizations by their employment relations provides a more genotypic characterization of forms in the sense that it speaks more directly to issues of identity.

3. The research focus

We examine the effects of changing the templates for employment relations set down by organizational founders. We analyze a sample of young high-technology companies

in California's Silicon Valley. We assess the effects of change on three outcomes: (i) survival versus failure, (ii) undergoing an initial public offering (IPO), and (iii) growth in financial valuations.

We think these three outcomes are appropriate for gauging the initial success of entrepreneurial ventures. They mattered keenly to employees and other stakeholders. Given the financing and incentive arrangements commonly used in Silicon Valley, completing an IPO was usually indispensable for gaining financial rewards. Moreover, an IPO required firms to surmount a series of hurdles to the satisfaction of external audiences: major investors, investment analysts, bankers, and regulators.[2] Growth in market capitalization obviously affects employees whose compensation consists partly of equity. It also clearly reflects widespread judgments about the success of organizations. The outcome we label as failure needs little justification of its salience to employees and other stakeholders.

Adapting the general arguments about imprinting and identity to this research context leads us to propose that: (i) founders' initial blueprints for employment relations have enduring consequences because they are tied strongly to organizational identity, (ii) the blueprints resist change, and (iii) efforts to redraw them cause disruption.

We focus on founders' initial premises because they arguably shape the initial features adopted by organizations and initial perceptions of the organization's identity. Identity researchers have highlighted the importance of top managers in shaping and maintaining others' perceptions of identity (Goia and Thomas, 1996). We believe that such influence is especially strong for small and young organizations, such as those in our sample, whose founders often work side by side with other employees and may have personally recruited many of them. Moreover, given the centrality of employment relationships in organizational life, these issues are likely to be salient subjects of reflection and discussion, and therefore perceptions are more likely to be shared.[3]

In the following section, we describe the typology of employment blueprints we employ. If that typology captures socially meaningful recipes for organizing and founders' blueprints tend to get imprinted in identities, then efforts to change blueprints should be disruptive. We therefore offer the following propositions

H1: Changes in blueprints for employment relations

(a) *increase the hazard of failure,*
(b) *lower the hazard of IPO, and*
(c) *depress the rate of growth in market capitalization.*

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[2]In hindsight, the hurdles in place during the stock market bubble of the late 1990s were probably lower than in other periods.

[3]Founders' perceptions of their organizations' identities may differ from the perceptions held by other employees, internal and external audiences, and stakeholders. To the extent that this is so, we should find changes in these identities to be no more disruptive to the organization than departures of the founders and their replacement with senior managers recruited outside the firm.

Silicon Valley over the period of the study (1994–2001) is a good context for testing these hypotheses for two reasons. First, as noted above, success was tied tightly to the competence embodied in the organization's workforce. Second, relevant labor markets were extraordinarily tight, meaning that the costs to employees of leaving a firm whose identity they did not embrace were minimal.

On the contrary, some common observations about Silicon Valley suggest that Hypotheses H1a, b, and c might not be supported. For instance, firms in this setting are often portrayed as competing in technology races, in which fast development of superior technologies outweighs organizational capabilities in generating success (Bahrami and Evans, 2000; Benner, 2002). Furthermore, geographical proximity, intense labor mobility, labor market intermediaries, and dense network ties among organizations give founders timely information about the activities of other enterprises, which should foster the diffusion of managerial approaches and lower the difficulty and cost of changing them (Castilla *et al.*, 2000; Suchman *et al.*, 2001; Hyde, 2003). Our focus on young companies might make this test conservative because it takes time for organizations to establish themselves as legitimate and valued entities in the eyes of important constituents (Hannan *et al.*, forthcoming). One might, therefore, expect that organizational identities generally become stronger and more resistant to change as organizations age.

To sharpen our research question we examine another potentially destabilizing change, but one that we think is less tied to identity: appointing an outsider as chief executive officer (CEO). Extensive research reveals that executive succession generally degrades organizational performance, at least for a while. Top-management turnover disrupts work routines and communication patterns and creates insecurity among employees, leading to increased conflict in the workplace and, ultimately, lowered performance (Gouldner, 1954). Analyses of managerial turnover for professional baseball (Grusky, 1963) and college basketball teams (Eitzen and Yetman, 1972) uncovered a negative association between turnover and winning percentage. Haveman (1993) and Carroll (1984) found that top-management succession increased mortality hazards of savings-and-loan associations and newspapers, respectively.

Weberian reasoning suggests that modern organizations depend less on any specific individual than they do on their underlying cultural logics. Weber noted numerous means by which beliefs and principles associated with visionary leaders can become routinized within organizations—through informal traditions and lore or through more formal rules, procedures, and institutions. From this perspective, it is not replacing leaders *per se* that destabilizes organizations but rather the changes in premises and practices associated with leadership succession.

Consistent with this view, managerial succession has particularly severe effects when an outsider replaces a top executive, because it brings greater organizational and cultural change (Carlson, 1961; Helmich and Brown, 1972). This pattern is likely to be especially strong for the replacement of founders, who likely personify much of the organization's cultural blueprint, critical routines, and operating procedures, which may be lost if not institutionalized before the founder's departure (Carroll, 1984).

In our view, changing leadership might destabilize operations in most organizations, consistent with the technical side of inertia theory. However, it need not challenge identity. Indeed, we argue that change in leadership is especially destabilizing when it is accompanied by changes in cultural templates that call into question the organization's identity. Moreover, we think that changes in these templates are destabilizing even when leadership does not change. Hence, we hypothesize that even this extreme type of managerial succession will prove less destabilizing (i.e., have weaker effects) than changes in blueprints

> *H2: Appointment of an outsider as chief executive destabilizes organizations less than changes in blueprints for employment relations.*

4. The research context

Understanding the effects of founding conditions necessitates information about the earliest days of a sample of organizations. The Stanford Project on Emerging Companies (SPEC) was designed to produce such information. This project analyzed a sample of technology firms in Silicon Valley, which were no more than ten years old and had grown to at least 10 employees when sampled. The project focused on computer hardware and/or software, telecommunications (including networking software and equipment), semiconductors, and medical/biological technologies.[4] The focus of SPEC on one region and a single broad sector of economic activity holds constant many key labor market and environmental conditions, as well as some institutional influences, thought to shape organizational development.

In total, 186 CEOs agreed to have their organizations participate in the study. Nine sampled companies turned out to be wholly owned subsidiaries, reducing the sample to 177 independent organizations. Because three firms began as public companies (due to mergers and acquisitions involving already-public companies), the design provides information about the IPO process for 174 companies. When we examine the failure, we consider only organizations that were still independent at the time of sampling, as we explain below. This restriction yields a sample of 171 organizations for the failure analysis.

Measures of early organization building were derived from interviews the SPEC researchers conducted with founders and then-current CEOs, which were not completed for another 18 companies (17 of these were at risk of failure as of the time of sampling). Hence, our analyses of the IPO process examine a subsample of 156 organizations, and our analyses of the failure process utilize a subsample of 154 organizations. Finally, in analyzing growth in market capitalization, we examine the 42 organizations that had IPOs after the time of sampling.

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[4]For details regarding the SPEC study design and sample, see Baron *et al.* (1999a, b: 528 529).

4.1 Data collection

The SPEC gathered information using survey, interview, and archival methods. Trained research assistants conducted semistructured interviews with the CEO at the time of sampling. The CEOs identified the founder best equipped to provide information about the organization's origins and the best informant about human resource practices. These informants were interviewed about company history and human resources, respectively. The SPEC companies were tracked through June of 2001 by repeatedly surveying relevant archival data sources, especially those collected in *Lexis/ Nexis Academic Universe* (Reed Elsevier Inc., 2001).

4.2 Characterizing organizational change

In open-ended interviews, the SPEC researchers asked each founder whether he or she had "an organizational model or blueprint in mind when [you] founded the company." (The then-current CEO was asked a parallel question about the period corresponding to the date of the interview.) Analyses of transcripts of these interviews revealed that premises about employment relations varied on three main dimensions—attachment, coordination/control, and selection—each characterized by three or four fairly distinct options or approaches. The responses of each founder and CEO were coded on these three dimensions, unless missing data precluded this.[5] Previous studies utilizing the SPEC data have shown that patterns of responses on these dimensions cohere and can be used to characterize the implicit organizational blueprints of founders and CEOs (for an overview and additional details, see Burton, 1995; Baron and Kreps, 1999: ch. 19). Here, we briefly summarize the approach.

Three bases of employee attachment were discerned in interviews with founders. Many founders anticipated providing opportunities for interesting and challenging *work* as the main basis for attracting, motivating, and (perhaps) retaining employees. Others envisioned creating a strong sense of personal belonging and identification with the company—in a sense, *love*, and some regarded the relationship as fundamentally an exchange of labor for *money*.

The second dimension concerns the primary criterion for selecting employees. Many founders sought employees with the *skills and experience* needed to accomplish some immediate task(s). Others focused on long-term *potential*; still others concentrated their search on *cultural fit*, emphasizing how a prospective hire would connect with other employees.

The third dimension concerned the principal means of coordinating and controlling work. The most common conception involved reliance on *informal control by peers* (perhaps shaped by an overarching culture). Other founders intended to rely on control via *professional norms*. A third group took a more traditional view of control

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[5]For details on the process used to classify SPEC firms into blueprint categories, see Burton (1995, 1999) and Baron *et al.* (2001: 965–966).

as embedded in *formal procedures* and systems. Finally, some founders planned to rely on *direct oversight*.

These three types of attachment and of selection and four types of coordination/control yield $3 \cdot 3 \cdot 4 = 36$ possible combinations. However, the observations cluster in a few cells, which we call the *basic model types* (Table 1).

Baron *et al.* (2001: 968–969) refer to these five configurations as the "basic employment blueprints," noting that not only are they prevalent in the SPEC sample, but they also exhibit internal coherence, resonate with Silicon Valley insiders, and reflect logics of organizing encountered in other familiar institutions. Some responses differed from one (and only one) of the basic models on only one dimension. Previous analyses based on the SPEC sample have referred to these as *near-model* types, combining them with the nearest basic type. However, in examining organizational performance, we find that the contrasts among blueprints are starker—and stronger in statistical significance—when we isolate the basic types and distinguish them from the other observed configurations. Therefore, we distinguish among the five basic types, and we add a sixth residual category, which we call *nontype*.

We use this typology to characterize the blueprint of each founder at the organization's inception (retrospectively), and each CEO at the time the company was sampled. Table 2 summarizes that the two blueprints were the same for 94 organizations (60%).

We constructed histories of tenures in top-management positions and coded whether each new incumbent of the top position had been associated with the firm before being appointed as CEO. In analyses that we do not report in detail here, we found that the departure of a founder who had been a CEO had no detrimental effect for the organization, independent of blueprint change. We thus focus on the hiring of an outsider CEO as the more drastic form of leadership change (and a tougher comparison with blueprint change).

4.3 Methodological concerns

This effort to characterize organizational blueprints raises a host of conceptual and methodological issues, discussed in detail by Baron *et al.* (1999a). Here, we touch

Table 1 Five basic employment models, based on three dimensions

Dimensions			Basic model
Attachment	Selection	Coordination/control	
Work	Potential	Professional	Star
Work	Skills	Peer/cultural	Engineering
Love	Fit	Peer/cultural	Commitment
Work	Skills	Formal	Bureaucracy
Money	Skills	Direct	Autocracy

Table 2 Founder and CEO models of the employment relation

Founder's model	CEO's model						
	Commitment	Star	Engineering	Bureaucratic	Autocratic	Nontype	Total
Commitment	7	0	0	0	0	4	11
Star	0	6	1	1	0	5	13
Engineering	0	0	25	12	1	11	49
Bureaucracy	0	0	2	5	0	1	8
Autocracy	0	0	0	0	3	2	5
Nontype	3	1	9	8	1	48	70
Total	10	7	37	26	5	71	156

briefly on several concerns. First, the conceptualization and coding effort for blueprints sought to measure the premises of founders and CEOs, which might or might not correspond to reality (for some evidence that they *do* in this sample, see Baron *et al.*, 1996). The classification of responses on the three dimensions relied not on what respondents claimed they were actually doing but instead on what they recounted about their premises. This approach is felicitous for our purposes for at least two reasons. On theoretical grounds, we believe that founders' employment premises are more fateful, more closely tied to identity, and more disruptive to alter than are specific practices. Moreover, we suspect that the young SPEC companies might vary more in their architects' assumptions than in their initial configurations of practices.

Another concern is that founders might have selectively reconstructed the past. Although we cannot definitively rule out retrospection bias, previous results provide some reassurance on this score. For instance, Baron *et al.* (1999b) reported that the founder's initial premises relate strongly and systematically to an independent measure of administrative intensity at the time of sampling, suggesting path dependence in bureaucratization. In contrast, the CEO's blueprint at the time of sampling was unrelated to administrative intensity at that time. If respondents had selectively tailored their stories to match or rationalize reality, then responses from present-day CEOs should have done a better job of predicting present-day organizational arrangements than did founders' recollections of their initial premises.

The results reported here avoid a different kind of methodological problem that might have been more salient in previous research using SPEC data. For the most part, the previous research has related organizational features measured at the time of sampling to contemporaneous outcomes, leaving problematic the direction of causation (Baron *et al.*, 2001). Here we relate features measured at the time of sampling to *events that occurred afterwards*. This design does not, of course, rule out the possibility that some omitted variables create a spurious relationship between the covariates and

the outcomes; we return to this issue below. However, it does rule out the story (quite plausible in the context of cross-sectional analyses) that observed associations between changes in blueprints and performances actually reflect a simple causal path *from* performance *to* organizational change.

5. Measurement

5.1 Outcomes

Data on performance were obtained by reconstructing the life histories of the SPEC companies. Because no single event reliably marks the initiation of an organization, the starting date was defined as the earliest of three events: legal incorporation, hiring an employee, and selling a product. (Usually these events occurred within a few months, but the sequence varies considerably among the companies; see Baron and Hannan, 2005.)

Companies were tracked until they ceased to exist as independent companies. All surviving histories are right-censored by the design at June 30, 2001. By definition, no sampled organizations had already failed when it was listed in the publications that formed the basis of the sampling plan. Therefore, we restrict analysis of failures to the *postsampling period*, defined as the start of the year in which we created each sampling frame.

Efforts to establish precise dates of ending events encountered some problems. Searches for records of events in *Lexis/Nexis Academic Universe* (Reed Elsevier Inc., 2001) and *Dow Jones Interactive* (Dow Jones Inc., 2001), on the Internet, in Securities and Exchange Commission (SEC) filings, and in other business media identified 70 acquisitions, 12 mergers, and 7 disbandings. Eleven more organizations vanished without a trace: they had neither an active telephone listing nor a web page, and our archival searches did not disclose that they had changed names or moved from the region. We set their ending times of vanished companies to the midyear of the last year in which they were known to be operating, based on our searches.

In the case of acquisitions, we wanted to identify cases in which the acquisition terminated an unsuccessful venture. We coded reports (from industry analysts, newspaper journalists, and others) that turned up in our searches, assigning a success code ranging from 1 to 5 to each ending event. (For obvious reasons, the coding was blind with respect to blueprints.) A value of 1 indicates an abject failure, and 5 denotes an acclaimed commercial and/or technological success. Values of 2 and 4 correspond to clear, but less decisive, failure and success, respectively. We assigned values of 3 to those cases in which there were mixed signals of success and failure or unclear signals. Values at the low end of the scale were assigned when the commentators reported that the organization was on the brink of bankruptcy or that the acquirer sought only the assets of the target, suggesting that the organization *per se* had little or no value. For

instance, one organization was described as having "managed to keep some research alive by agreeing to be acquired." An industry analyst commenting on the sale of another asserted: "This is the first break [the organization] caught. They just never caught on."

We coded an organization as having failed if it disbanded (seven cases), disappeared without a trace (11 cases), or was acquired and the success code was 1 or 2 on the 5-point scale (11 cases): a total of 29 failures.[6]

We recorded the exact dates of (completed) IPOs using information provided in the CRSP US Stock database (Center for Research in Security Prices, 2000).

For market capitalization, we used the CRSP data as the source of end-of-trading-day prices and numbers of shares outstanding. [For 2001, we used information from *Datastream International* (Thomson Financial Securities Data, 2001b).] We measured market capitalization (total value of publicly traded shares outstanding) at the end of each month from the date of IPO through June 2001 or, if the organization was acquired or merged, on its last day of trading.

5.2 Covariates

We measured change in the blueprint in two ways: a simple binary distinction (zero if the founder and CEO espoused the same blueprint, one otherwise) and the number of dimensions (zero to three) that differed between the two blueprints. Estimated effects and model fits are very similar when we use each measure; therefore we report results using the simpler binary distinction.

To parse out the effects of blueprint change and change due to appointment of an outsider CEO, we constructed three binary variables that distinguish cases with an outsider CEO but unchanged blueprint ($N = 18$), changed blueprints but no outsider CEO ($N = 33$), and both kinds of changes ($N = 42$); the omitted category contains organizations experiencing neither type of change.

We control for primary industry using binary variables for the computing, telecommunications/networking, semiconductors, component manufacturing, and research, with the omitted category representing medical devices/biotechnology.[7]

Measuring financial returns was complicated because many yearly spells pertain to privately held companies, for which we rarely could acquire the relevant detailed information. We collected yearly series of total revenues from any of a diverse set of sources. Our main sources were company reports (such as annual reports and press

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[6]We obtained similar results if acquired firms with a success code of 3 on our 5-point scale were included among those deemed to have failed.

[7]We also controlled for business strategies, using a simple classification that proved useful in our earlier research: (i) technological innovation; (ii) enhancing existing technology; (iii) emphasizing marketing, customer support, and/or customization to client specifications; (iv) combinations of 3 with 1 or 2; and (v) cost minimization. Founder and CEO reports of strategic intent did not relate systematically to the outcomes we study, net of industry, and the other controls in our analyses.

releases), *CorpTech* (OneSource Information Services, 2001a), and *U.S. Business Browser* (OneSource Information Services, 2001b). We could not find reports of revenues for 32% of the monthly spells, usually for an organization's early years, which suggests that missing data indicate the absence of revenues. Rather than omit such spells, we set revenue for such spells to zero and constructed a binary variable that equals one when revenues were either reported to be zero or were not observed and equals zero otherwise.[8]

To control for differences in opportunities and endowments, we constructed variables for whether firms had obtained venture-capital financing by the start of the month (Thomson Financial Securities Data, 2001a); the number of IPOs in a focal firm's industry during the prior month (Thomson Financial Securities Data, 2001c); and (in analyses of failure) whether they had become public companies. Finally, we included additional controls for macroeconomic conditions that might affect performance: the bank prime-loan interest rate for the last week of the prior month (U.S. Federal Reserve Bank, 2001), a time trend, and stock market conditions (the level of the NASDAQ index at the end of the prior month, or, in our analyses of market capitalization, growth/decline in the NASDAQ index over the preceding month).

6. Stochastic models and methods of analysis

6.1 The hazards of failure and IPO

We define a pair of random variables that record the status of organizations with respect to failure and public/private status by age. We estimate specifications in which the underlying hazards depend log-linearly on time-invariant organizational characteristics and on organizational and environmental characteristics that vary over time. We report test statistics (z-scores) based upon robust ("sandwich") estimates of standard errors.

We examined the possibility that the covariates of greatest interest (blueprints, changes in blueprints, and appointment of first outsider CEO) have nonproportional effects, that is, that their effects change over age periods. We did not find significant nonproportionality; therefore we report estimates of proportional-effect specifications. We also found that specifications with parametric and nonparametric specifications of age dependence did not improve significantly over constant-rates models. Therefore, we report estimates of specifications of the form:

$$h(t \mid \mathbf{x}, \mathbf{z_t}) = \exp(\mathbf{x}\,\beta + \mathbf{z_t}\,\gamma), \tag{1}$$

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[8]If we use separate dummies for measured revenues of zero versus missing data on revenues, the two dichotomies have similar effects on the outcomes we study, and estimates of other model parameters are insensitive to the alternative treatments.

where h is a hazard of interest, $\mathbf{x}$ is a vector of fixed organizational properties (including blueprints and change in blueprint), $\mathbf{z}_t$ is a vector of time-varying control variables, and α and β are vectors of coefficients to be estimated.

6.2 Growth in market capitalization

The data on changes in market capitalization consist of an unbalanced pooled cross-section/time series of monthly data for each publicly traded company (the number of monthly observations varies among organizations because IPOs occurred at different times). Much recent organizational research analyzes such panel data with fixed-effect estimators, which analyze only the within-organization over-time variation. This choice is unappealing for our research question because our measure of blueprint change is fixed for an organization—we do not know when the prevailing blueprint changed. Instead, we use robust estimators that allow variation among organizations and within organizations over time to be analyzed. Specifically, we use the method of generalized estimating equations (GEE) developed by Liang and Zeger (Liang and Zeger, 1986; Zeger and Liang, 1986). This approach, which generalizes quasi-likelihood estimation to the panel context, requires specification of only the first and second moments of the distribution, rather than the full distribution as is required for maximum likelihood. Under mild regularity conditions, GEE estimators are consistent and asymptotically normal.

We face a selectivity issue in this analysis: seven organizations were delisted from the NASDAQ market for failing to meet the mandated capital requirements or because their stock prices fell below the minimum threshold. Obviously these organizations were faring very poorly in the market.[9] A delisting ends a firm's record on market capitalization. Analyses that do not address this issue face a classic form of specification error due to sampling on the dependent variable. To address this concern, we estimated the hazard of delisting and include the predicted hazard associated with each spell as a covariate in specifications of growth in market capitalization.

We estimated specifications in which the natural log of a firm's market capitalization (in thousands of dollars) at the end of a given month, $\ln(y_t)$, depends linearly on the log of its value at the end of the previous month; the log of the ratio of the NASDAQ index at the end of the month to its value at the end of the previous month, $\ln(N_t/N_{t-1})$; a set of covariates, $\mathbf{X}_{t-1}$, that includes both fixed and time-varying attributes; a time trend K_t (measured in years, with 1982.0 as the zero reference point); and the predicted hazard of delisting, $\hat{h}_t$, based on the values of covariates, $\mathbf{X}_{t-1}$, at the

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[9]One might argue that delisting should be one of the "failure" events. We decided against this idea because private companies were not at risk of delisting. If a public and private firm each experienced the same decline in valuation, we can observe delisting only for the public enterprise.

end of the previous month. Hence, the equation we estimate in predicting market capitalization is

$$\ln(y_t) = \alpha \ln(\mathbf{y}_{t-1}) + \beta \ln \left(\frac{N_t}{N_{t-1}} \right) + \gamma \mathbf{X}_{t-1} + \upsilon K_t + \delta \hat{h}_t + u_t. \tag{2}$$

We expected the disturbance process, u_t, to exhibit autocorrelation of the usual panel type: observations for the same organization will tend to be correlated due to permanent and/or gradually changing unobserved properties of each organization. However, we assume that observations are uncorrelated for different organizations. In particular, we assume:

$$E(\mathbf{uu}') = \varphi \begin{pmatrix} \mathbf{A}_1 & 0 & & 0 \\ 0 & \mathbf{A}_2 & 0 & 0 \\ & 0 & & \\ & & & 0 \\ 0 & & 0 & \mathbf{A}_I \end{pmatrix}, \tag{3}$$

where ϕ is a scale parameter, $\mathbf{A} = \mathbf{R}$, and the matrix $\mathbf{R}$ satisfies the properties of a correlation matrix. The GEE requires a specification of a "working" correlation matrix. The implementation we used—the XTGEE routine in version 7.0 of STATA (StataCorp, 2001)—allows a menu of choices for the working correlation matrix. We experimented with several, including the exchangeable correlation structure, as well as first-order and second-order serial autocorrelation. Autocorrelation turned out to be negligible, and effect estimates for measured covariates are extremely similar under alternative specifications. Therefore, we report results based on the assumption of independent disturbances. Again we rely on robust standard errors.

7. Results

7.1 Descriptive statistics

Table 3 reports descriptive statistics separately for the fixed and time-varying covariates. (The marginal distribution of founders' blueprints and the pattern of change can be found in Table 2.) Table 3 summarizes that 49% of the organizations experienced a change from the founder's blueprint (the average number of dimensions changed was 0.73).

7.2 The hazard of failure

We begin with the outcome that speaks most clearly to our argument: survival versus failure. As noted above, we have complete information on blueprints for only 154 of

Table 3 Descriptive statistics

Fixed covariates	Number of firms	Mean[a]		
Founder's employment blueprint				
Autocracy	156	0.032		
Bureaucracy	156	0.051		
Commitment	156	0.071		
Engineering	156	0.314		
Star	156	0.083		
Nontype	156	0.449		
Organizational change				
Changed employment blueprint but did not hire outsider CEO	156	0.211		
Hired an outsider CEO but did not change blueprint	156	0.115		
Changed employment blueprint and hired an outsider CEO	156	0.269		
Industry				
Computer	156	0.506		
Manufacturing	156	0.045		
Medical/biotechnology	156	0.141		
Semiconductor	156	0.096		
Research	156	0.013		
Telecom/networking	156	0.200		

Time-varying covariates	IPO analysis			Failure analysis		
	Monthly spells	Mean	SD[a]	Monthly spells	Mean	SD[a]
Revenue						
Level (millions)	4465	12.23	30.73	8657	94.23	557.2
No revenue	4465	0.09		8657	0.05	
Venture-capital financing by start of spell	4465	0.54		8657	0.72	
Prime interest rate	4465	8.34	0.66	8657	8.36	0.63
IPOs in own industry	4465	4.55	4.46	8657	4.13	4.31
Public company by start of spell				8657	0.47	

Table 3 Continued

	Growth in market capitalization analysis		
	Monthly spells	Mean	SD
ln(market cap/1000)	1809	12.23	1.55
Predicted hazard of delisting	1809	0.046	0.296

CEO, chief executive officer; IPO, initial public offering.

[a]Standard deviations (SD) not shown for binary variables.

the 171 organizations at risk of failure at the time of sampling. Only one of the 17 organizations with missing information on blueprints failed, whereas 28 of the remaining 154 organizations experienced failure. (The difference in raw failure probabilities is insignificant: p 0.12.) On average, the organizations with missing values for blueprints had higher revenues (at time of sampling) and were more likely to have received venture-capital financing. However, we cannot reliably predict the decision not to complete the interviews.[10]

Given the sampling plan, only postsampling spells provide meaningful information about the odds of failure. Table 4 reports results based on specifications with effects of founders' blueprints. (Estimates of specifications with effects of CEOs' blueprints are very similar.) As predicted, change in the blueprint markedly increases the chances of failure, tripling the (net) hazard ($p < 0.05$). In contrast, appointing an outsider as CEO has no appreciable net effect on the hazard of failure. This pair of results supports Hypotheses H1a and H2.

Supplementary analyses revealed that neither incorporating the effects of CEOs' blueprints nor allowing the effect of change to vary by blueprint improves the fit significantly over the simpler specifications in Table 4.

We find one strong "content" effect: no organization founded on a commitment model failed. (Very large negative effects in Tables 4–6 mean that no events occurred in the relevant set of time points or covariate values; such estimates imply a hazard of zero.) None of the other founder blueprints differs significantly from the excluded engineering model at the 0.05 level.

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[10]In a statistical model predicting participation status from the measured covariates available for all SPEC firms (industry, revenues, and venture-capital financing at time of sampling), none of the individual predictors is statistically significant, nor is the fit of the model as a whole. We also explored possible sample-selection bias in estimates of how covariates affect the hazard of failure in the sample with measured employment models. Adding each firm's predicted probability of full participation as a covariate does not alter the results or improve model fit for any analyses reported in Tables 4–6.

Table 4 Effects of organizational and environmental characteristics on the hazard of failure after the time of sampling (ML estimates of constant-rate specifications)

	Effect	*z*-score
Constant	5.67	1.47
Industry (versus med/biotech)		
Computer	0.14	0.18
Semiconductor	0.71	0.67
Telecom/networking	0.06	0.08
Manufacturing	1.18	1.19
Research	16.34	17.65
Revenue$_{t-1}$		
Level (10 millions)	0.11	0.65
No revenue	0.45	0.43
Venture-capital financing$_{t-1}$	0.40	0.74
Prime interest rate$_{t-1}$	0.07	0.20
IPOs in own industry$_{t-1}$	0.16	2.23
NASDAQ$_{t-1}$/1000	0.18	0.44
Time trend (1982.0 = 0)	0.15	0.80
Public company$_{t-1}$	0.0005	0.00
Founder's employment blueprint (versus engineering)		
Autocracy	0.84	1.19
Bureaucracy	0.46	0.41
Commitment	16.63	22.52
Star	1.24	1.13
Nontype	0.65	1.49
Organizational change		
Change in employment blueprint only	1.18	2.25
Outsider CEO only	0.12	0.14
Both model change and outsider CEO	1.13	2.00
Number of firms	154	
Number of firm-month spells	8657	
Number of failures	28	
Log-likelihood	67.83	

CEO, chief executive officer; IPO, initial public offering.

The effects of other covariates in Table 4 generally are slight and statistically insignificant. Surprisingly, revenues did not affect the hazard of failure. Public companies had higher hazards, whereas the odds of failure were lower for firms backed by venture capital and in industries experiencing a high level of IPO activity. There is little systematic variation in the net hazard by industry, aside from a hazard that is essentially zero for the few research shops in our sample.

7.3   The hazard of IPO

The picture is different for the odds of an IPO. Neither change in the blueprint nor appointment of an outsider CEO significantly affected the hazard of IPO (Table 5). These results do not support the hypotheses.

As with failure, we see that the net hazard of IPO for the commitment model is significantly higher than for the (excluded) engineering model. This is the only blueprint

Table 5 Effects of organizational and environmental characteristics on the hazard of IPO after the time of sampling (ML estimates of constant-rate specifications)

	Effect	z-score
Constant	1.53	0.54
Industry (versus med/biotech)		
Computer	1.49	1.79
Semiconductor	0.36	0.43
Telecom/networking	0.38	0.41
Manufacturing	17.08	13.21
Research	16.41	14.12
Revenue$_{t-1}$		
Level (10 millions)	0.08	2.89
No revenue	17.55	29.78
Venture-capital financing$_{t-1}$	1.93	2.53
Prime interest rate$_{t-1}$	0.20	0.85
IPOs in own industry$_{t-1}$	0.20	5.67
NASDAQ$_{t-1}$/1000	0.27	0.61
Time trend (1982.0 = 0)	0.26	1.31
Founder's employment blueprint (versus engineering)		
Autocracy	0.27	0.43
Bureaucracy	0.41	0.84
Commitment	1.15	2.56
Star	0.25	0.25
Nontype	0.73	1.60
Organizational change		
Change in employment blueprint only	0.48	0.61
Outsider CEO only	0.46	1.00
Both model change and outsider CEO	0.37	1.01
Number of firms	119	
Number of firm-month spells	4465	
Number of events (IPOs)	42	
Log-likelihood	77.54	

CEO, chief executive officer; IPO, initial public offering.

for which this pattern holds. The implied net hazard of IPO for organizations with commitment-model founders was more than triple (i.e., $e^{1.15}$ 3.16) that of organizations with engineering-model founders.

7.4 Growth in market capitalization

We considered post-IPO outcomes in the public equity markets in two steps. First, for the 42 organizations with IPOs after the date of sampling, we examined *initial* market capitalization: the value of the company's publicly traded stock at the end of the first day of trading (the IPO date). The results can be summarized easily: we find little, if any, relationship between (logged) initial market capitalization and the other variables we analyze (including blueprint change and the appointment of an outsider as CEO), other than revenues.

Second, we analyzed monthly growth/decline in market capitalization from the time of the IPO through the last observation period for each firm (Table 6). Note that the estimated effect of the lagged dependent variable is close to unity, in which case the growth model takes the appealingly simple form of a generalization of Gibrat's law: the expected growth rate is independent of market capitalization, given any combination of covariates.

Analysis of market returns yields results similar to those for failure: blueprint change depresses growth rates and has more deleterious effects than appointment of an outsider CEO does (Table 6). Change in the blueprint without appointing an outsider CEO has a significant negative effect on growth in valuation: the predicted growth rate was 5% lower (i.e., $e^{-0.05}$ 0.95) for organizations with changed blueprints. In contrast, outsider CEO by himself/herself had no appreciable effect on growth rates. If both changes occurred, the predicted growth rate is 3% lower, relative to otherwise comparable companies.[11] Thus, we find support for Hypotheses H1c and H2.

Recall that we analyze month-to-month changes in market capitalization over several years (39 months for the average company analyzed in Table 6). Hence, assessing consequences over longer periods requires compounding the effects in Table 6. Suppose that two companies started with the same initial market capitalization and were otherwise

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[11]The weaker effect of a combination of changes as compared with the effect of changing only the blueprint suggests that a change in the employment blueprint by a stable leadership is more destabilizing than when blueprint change is accompanied by new, outside leadership. This is consistent with the idea that employment relationships form a key facet of organizational identity within this context. When it is instigated by the same leaders with whom employees established their understandings of the employment relationship, blueprint change is likely to be regarded as a greater violation of implicit trusts than when it is pushed forward by a new regime. Existing beliefs regarding identity will thus be challenged to a greater degree. External audiences may also punish companies that change their blueprints under founding leadership more than those that undertake changes with new leadership. This may be because they expect new leadership to make changes in organizational practices while they suspect problems with the companies that change their blueprints without any apparent reason.

Table 6 Effects of organizational characteristics on monthly growth in LN (market capitalization/1000) for firms with post-sampling IPOs (GEE estimates of the specification in equations 2 and 3)

	Effect	z-score
ln(market cap$_{t-1}$)	0.98	166.79
Revenue$_{t-1}$ (10 millions)	0.002	3.20
Age$_{t-1}$	0.0003	0.14
Predicted hazard of delisting$_{t-1}$	0.17	13.37
ln(NASDAQ$_t$/NASDAQ$_{t-1}$	1.31	13.16
Prime interest rate$_{t-1}$	0.01	0.64
IPOs in own industry$_{t-1}$	0.003	1.70
Industry (versus medical/biotech)		
Computer	0.01	0.56
Semiconductor	0.01	0.32
Telecom/networking	0.02	0.73
Time trend (1982.00)	0.01	1.58
Founder's employment blueprint (versus engineering)		
Autocracy	0.05	3.37
Bureaucracy	0.01	0.88
Commitment	0.02	0.88
Star	0.06	2.97
Nontype	0.01	0.70
Organizational change		
Change in employment blueprint only	0.05	2.63
Appoint an outsider CEO only	0.01	0.69
Both model change and outsider CEO	0.03	2.00
Number of firms	42	
Number of firm-month spells	1594	
Pearson X^2	98.32	

CEO, chief executive officer; GEE, generalized estimating equations; IPO, initial public offering.

identical, except that one experienced blueprint change and the other did not. The 5% per month difference in growth rates compounded over, say, 36 months implies that the predicted market capitalization after three years with a stable blueprint would be approximately 5.8 times (1.05^{36}) higher than the other firm that changed.[12]

This analysis examines only firms that went public *after* entering the sample. Hence, the measured changes in blueprints were temporally prior to both the IPO and the measured changes in market capitalization. Consequently, we do not think that changes in market valuations could plausibly cause the observed changes in blueprints.

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[12]This estimate is for firms that had not appointed an outsider CEO by the time of sampling.

Rather, these results suggest that organizations paid an enduring penalty for altering their labor market identities, even after surmounting the obstacles to becoming a public company.

Table 6 provides additional evidence of content effects: founder's blueprints affect subsequent changes in market capitalization. In this instance, the star model stands out: average growth rates for public firms with star founding blueprints exceeded those for every other type. According to the estimated contrasts, the predicted monthly growth rate for an organization founded on a star model was 6% above that of a comparable enterprise founded with engineering premises.

The predicted growth rate was lowest for the autocracy model, roughly 5% below engineering. Although organizations founded along commitment lines fared best in terms of survival and time to IPO, their predicted growth in equity was not stellar: 2% lower than for the baseline.[13]

Only one organizational characteristic significantly affected post-IPO changes in firms' valuations: revenue. Not surprisingly, market values for the SPEC firms also tended to move with the NASDAQ index. (However, the estimated effect, 1.31, indicates that these firms grew and contracted disproportionately relative to the NASDAQ index, which reflects the movement of stock prices of much larger firms.) Finally, our concern with selectivity was warranted: the predicted hazard of delisting has a significant negative effect on the growth rate.

7.5 Process and content effects of blueprint change

The persistent adverse effect of changing blueprints—across multiple dimensions of performance—supports the claim that organizations are disrupted when they alter core features of their identities, such as the premises governing employment relationships. However, such disruption can sometimes be offset by beneficial "content effects," which increase the organization's fitness for its current environment (Barnett and Carroll, 1995). Positive content effects might occur, for example, when organizations change to fit predominant notions held by important constituencies for what organizations of a certain type should look like or adopt identities that are more attractive to employees. We explored the possibility that content-based effects may offset the negative impact of changing blueprints by comparing the effects of particular transitions.

Because the blueprint categories employed in this study were derived inductively from the data, we lack strong theoretical bases for positing specific transitions that should be particularly advantageous or harmful. Another difficulty in exhaustively exploring possible content effects is that it is not difficult to formulate highly contingent propositions about when specific changes might be beneficial (or harmful). Most organizational samples—including ours—lack the power to detect such complicated interactions.

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[13]With star as the baseline, the contrast with commitment is significant at the 0.05 level with founder blueprints (Table 6); this is not the case with CEO blueprints.

Hence, to maximize the chances of detecting beneficial content effects that might offset the adverse process effects of changing blueprints, we conducted numerous supplementary analyses, exploring whether there were identifiable conditions under which blueprint change improved performance. For each outcome, we examined all types of specific transitions from founder's to CEO's blueprint, but we did not detect any robust, meaningful results. We also looked for interactions of blueprint change with strategy change, specific founder and CEO blueprints, the post-2000 market downturn, and change in CEO. Except for an apparent benefit of abandoning the star model for the hazard of IPO, we did not detect any robust, significant interactions.

Note that we detected strong, significant, and substantively sensible effects of blueprints and blueprint change with contrasts that were often based on small numbers of cases. This suggests that our limited sample size does not preclude detecting significant differences based on contrasts involving small numbers of firms. That we failed to detect any such positive content effects does not, of course, guarantee that none exist, only that they are neither obvious nor overpowering. In contrast, the overall "process effect" of blueprint change is persistently and strongly negative, not only in the analyses reported here but in other studies using the SPEC data (Baron *et al.*, 2001). The evidence suggests that, at least in this sample, any positive content effects are very subtle or concentrated in few organizations.

7.6 Contrasts among blueprints

In addition to supporting our hypotheses about change, these analyses also support the broad conclusion that *origins matter*. Exactly how origins affect performance is a subtle and tricky question. Although neo-institutional perspectives have emphasized the benefits of conforming to prevalent and accepted organizational models, our findings underscore the potential benefits of differentiation and distinctiveness, especially for aspects of structure and practice that might activate gift-exchange processes when organizations deviate from what is customary or normative.

Interestingly, although commitment was widely pronounced dead in Silicon Valley not long after we sampled these companies in the mid-1990s (e.g., Pink, 1997), few companies founded on a commitment model actually outperformed the rest in terms of survival and speed to IPO. Perhaps the strong performance of the commitment blueprint in this context stems *precisely* from its running counter to the conventional wisdom, which pronounced it unworkable in Silicon Valley. After all, the signals an organization sends by championing commitment are especially powerful in a world in which these signals are rare and costly to send.

Scholars and practitioners alike argue that organizations learn (and *should* learn) by emulating "best practice." Our analyses point to a potential peril associated with this strategy of changing flexibly in response to developments in best practice: the destabilizing effects of altering identity-relevant features frequently might offset any performance improvements.

It is noteworthy that the commitment and star models have performed better as founding models than the alternatives. Baron *et al.* (1999c) speculated that starting with either of these models might represent a higher risk, higher return human resource strategy for young technology companies. The expected return is higher because these distinctive blueprints have greater potential for eliciting consummate performance (through appeals directed at the stars in the star model and by anointing everyone as a star in the commitment model). We think our results in this article accord with the view of the star model as involving very high potential returns at high risk.

7.7 A note on unobserved heterogeneity

A legitimate concern is whether our analyses omit some key variable(s) associated with both blueprint change and organizational performance that might explain the observed effects. We cannot dispel that concern completely, but several sets of results are reassuring. First, in supplementary analyses, we modeled the likelihood of blueprint change. Aside from industry, revenue, and age, which are controlled in our analyses, we identified no significant predictors of blueprint change. Second, Baron *et al.* (2001) report that, although blueprint change seems to have been associated with various disruptive events (e.g., downsizings, executive changes, financial, or legal turmoil) in the subset of SPEC companies they analyzed, controls for the latter did not attenuate the significant negative effect of blueprint change on employee turnover. Third, we scoured interview transcripts for comments about factors precipitating changes in culture and/or employment relations, but there was no consensus about causes (though there seemed to be consensus about the disruptive *consequences*).

Finally, recall that, in analyzing post-IPO stock market valuations, we found effects of initial blueprint and blueprint change on *growth* in market valuation but not the *initial* market valuations of the firms. If blueprint change was a proxy for some other attribute affecting corporate performance, this presumably should have been visible to market actors at the time of the IPO and reflected in their initial valuations. If it was *not* visible to the market, then one is hard pressed to explain why blueprint change should have affected changes in market capitalization after firms went public.

8. Discussion

Organizational sociologists, particularly ecologists, have emphasized the disruptive effects of change in core features. Such change is thought to destabilize organizations primarily by altering the premises, values, and routines that members have come to internalize. We have tried in this article to get closer to the mechanisms at the heart of theories of organizational inertia, by: (i) operationalizing the premises (blueprints) on which founders built organizations as a way to examine identity-related processes, (ii) measuring changes over time in those premises, and (iii) relating them to survival and performance.

We found considerable evidence that changing the organizational blueprint diminishes chances of early success. Specifically, change increased the odds of failure and reduced growth in market capitalization. On balance, our results support the view of neo-institutionalists and organizational ecologists, echoing Stinchcombe (1965), that founders impose cultural templates on nascent organizations, as well as ecologists' claim that altering such templates destabilizes organizations.

Ecologists assert that change is perilous insofar as it threatens internal and external identity; hence, researchers should be studying changes in those organizational features that are most relevant to identities. We contend that organizational ecology has underappreciated the importance of premises governing employment relations as one core feature of organizational identity (Baron, 2004). We have had to make our case indirectly, by demonstrating: (i) that differences in cultural blueprints for employment relations can be operationalized; (ii) that changes to those blueprints are highly destabilizing, across *multiple* dimensions of organizational performance; and (iii) that such changes appear to be even more disruptive than CEO succession, which previous literature has treated as a momentous organizational event. We acknowledge that other sorts of transformations—in products, technologies, markets, leadership, and the like—might be no less important for organizational evolution. But the strength and prevalence of the negative consequences associated with changing employment blueprints, even in a setting like Silicon Valley, is *prima facie* evidence that those premises are central to organizational identity.

The external environment facing this sample of high-tech firms changed markedly during the period under investigation. In following this sample until June 2001, we have examined both the boom of the late 1990s and the bust that began around March 2000.[14] That our results seem to hold up across such varied environmental circumstances provides encouragement regarding the generalizability of our main findings. We hope future studies will assess how well our findings generalize to other time periods, types of enterprises, environments, stages of organizational development, and dimensions of performance. Future research should also devote more attention to learning how cultural blueprints get selected and imprinted on organizations during their infancy and how they get sustained, modified, or discarded over time.

Finally, there is abundant opportunity for conceptual and methodological refinements in seeking to operationalize identity-based organizational forms and particularly to incorporate labor market considerations. The SPEC study was not designed specifically to address issues of organizational identity, and the employment blueprints identified among SPEC companies are certainly not the only, nor the optimal, way of capturing culturally based identities. Our results suggest that by enriching our concepts of organizational forms to include labor market-based identities, we are likely to

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[14]Supplementary analyses not reported here suggest that our broad conclusions about relative performance among blueprints and the adverse effects of blueprint change apply generally to *both* the boom and bust periods.

learn a great deal about organizational evolution and performance. The research approach taken here can no doubt be improved upon substantially; we will code our venture as a success if it prompts such work.

Acknowledgements

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